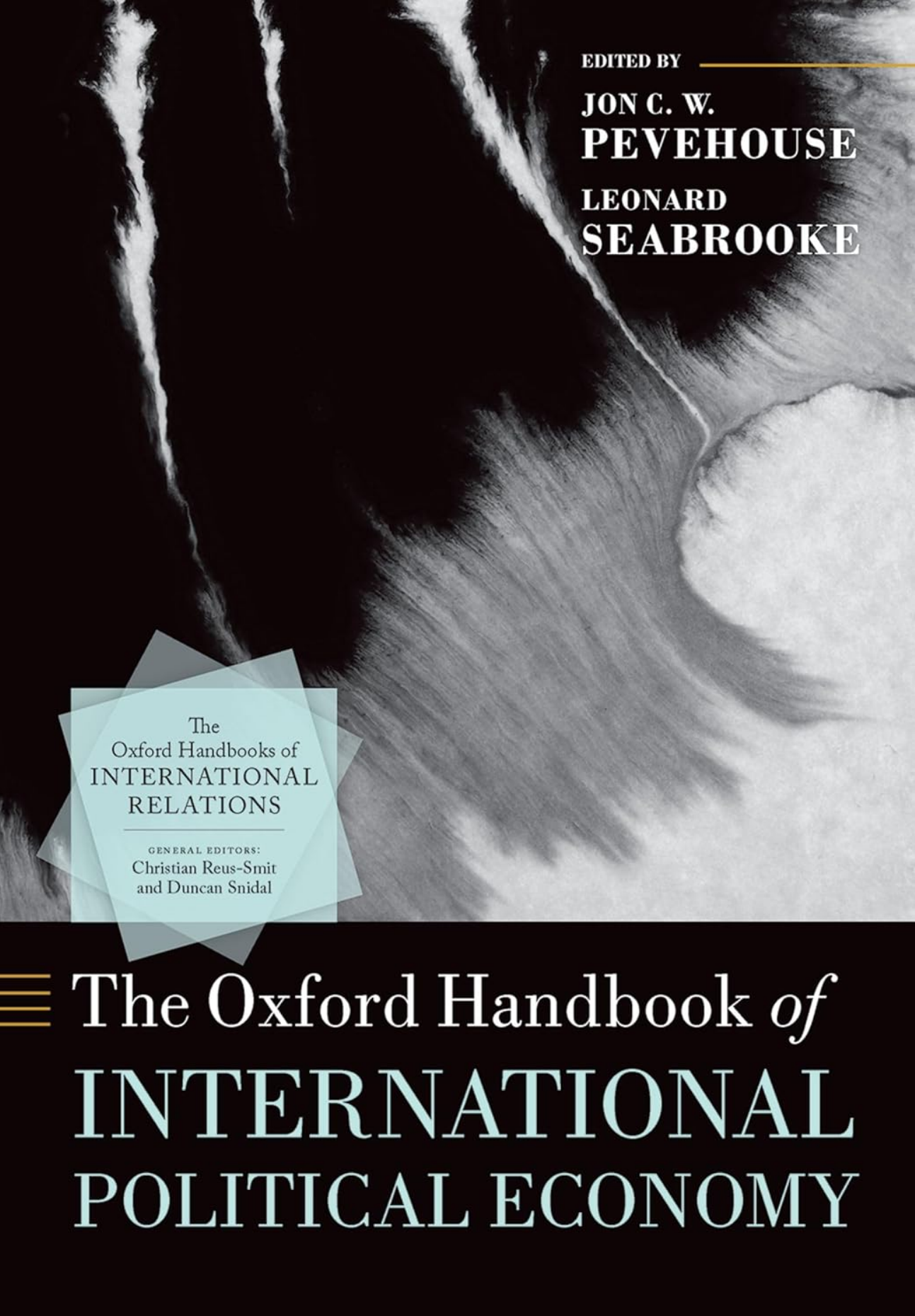




EDITED BY

JON C. W.
PEVEHOUSE

LEONARD
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The
Oxford Handbooks of
**INTERNATIONAL
RELATIONS**

GENERAL EDITORS:
Christian Reus-Smit
and Duncan Snidal

≡ The Oxford Handbook of
**INTERNATIONAL
POLITICAL ECONOMY**

THE OXFORD HANDBOOK OF
INTERNATIONAL
POLITICAL
ECONOMY

THE OXFORD HANDBOOKS OF INTERNATIONAL RELATIONS

GENERAL EDITORS

*Christian Reus-Smit of the University of Queensland and
Duncan Snidal of the University of Oxford*

The *Oxford Handbooks of International Relations* is a multi-volume set of reference books offering authoritative and innovative engagements with the principal sub-fields of International Relations.

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THE OXFORD HANDBOOK OF

INTERNATIONAL
POLITICAL
ECONOMY

Edited by

JON C. W. PEVEHOUSE

and

LEONARD SEABROOKE

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CHAPTER 1

INTRODUCTION

JON C. W. PEVEHOUSE AND LEONARD SEABROOKE

THE field of International Political Economy (IPE) is relatively young. Yet, both its rapid development and its interdisciplinary roots have contributed to making it a rich and productive field of inquiry. The field is still growing and evolving. This Oxford Handbook reviews and evaluates the research in this young field. This introductory chapter aims to provide an overview of the evolution of IPE scholarship while laying out some of the themes that will be developed across the chapters in the book.

Other IPE handbooks in political science and international relations have concentrated on “global conversations” (Blyth 2009) that seek to integrate different schools of thought or “state of the art” examinations of established topics, such as financial regulation, trade regimes, even specific international organizations (Martin 2015; Narlikar, Daunton, and Stern 2012; Weiss and Daws 2007). This IPE handbook concentrates on the relationship between theory and method as applied to a wide range of substantive topics. Our position is that the momentum of debate in IPE is driven more by the relationship between theoretical and methodological commitments as well as how empirical and social facts are treated rather than by any defense of particular paradigms, approaches, or “isms.” In short, we believe the field of IPE is problem-driven, although there remains significant diversity in the theories and empirical tools used to analyze these problems. This conclusion implies that any division of the field into camps may not be problematic, but also that researchers perform their due diligence as they contribute to research on a problem.

Recent analysis of IPE scholars from the “Teaching, Research, and International Policy” (TRIPs) survey has found that IPE scholars adhere less to “American” or “British” schools of thought or to firm positions as “Constructivists” or “Rationalists,” and are more pragmatic in how they approach the field (Maliniak and Tierney 2009). Such pragmatism does not imply a lack of theoretical or ethical commitment, but that IPE scholars concentrate on what theories and methods provide insights on particular topics and what can withstand sustained criticism. The core contribution of the chapters in this volume focus on a problem-centered approach to each subject, which we argue has become dominant in the past 20 years in IPE, although its roots go many decades prior.

Of course, we recognize that some theoretical debates linger. For example, the contestation of earlier decades between “critical” and “problem-solving” approaches still exists, with the former emphasizing the need for IPE scholarship to act as an emancipatory project

and the latter emphasizing the normal science approach to the field (compare Cox 2004 and Lake 2009; see also Young 2021). However, we argue that one key difference today in IPE (versus the 1980s and 1990s debates) is that scholars from either approach do not engage each other, largely existing in separate worlds of scholarship. What has emerged, in our view, is a process of “constructive non-engagement” where research relying on different theoretical traditions progresses apace, with little engagement across these traditions.

We argue that such a division of labor is a natural outgrowth of the move toward a problem-centered approach: scholars bring their toolkit and their assumptions concerned about explanation, leaving other meta-debates behind. In the next section, we offer some hypotheses on how and why that division has emerged.

In this Handbook, we have asked authors to engage the question of whether this lack of dialogue is preventing progress in the field. In their particular area of research, is anything lost in the current state of the field? Are there important insights lost by the “silo” approach to topics in IPE? How much do approaches they align themselves with exist in opposition to others? At a more basic level, is our description of their particular area accurate?

Where they exist, authors are also asked to highlight areas of productive overlap and engagement between different groups while giving significant attention to where scholarship concentrates on applying theories and methods to particular cases and providing defensible claims. Our assessment is that IPE’s “constructive non-engagement” has led to parallel tracks of insights on similar phenomena. While we appreciate the difficulties in promoting a global conversation, there is much to be learnt from each other than can enrich the respective tracks. Areas of obvious productive overlaps can be found in the use of common methods from different ontological and epistemological positions. The recent use of social network analysis provides an example, whereby these techniques have been adopted to look at corporations’ interlocking directorates to comment on the drivers of global capitalism (Heemskerk and Takes 2016), as well as to look at financial networks underpinning crises (Oatley et al. 2013). Such innovations provide a basis for common discussion over research findings by sharing a common method, even if the ontological and epistemological commitments differ.

The purpose of this introductory chapter is to offer the framework outlined above, laying the ground for the chapters to come. We believe the historical development of the field has played an important role in this arc toward the practical. Thus, we briefly review the evolution of the field and examine data to provide some evidence for our argument. Our final section is a short guide to lay out the remainder of the volume.

THE EVOLUTION OF THE IPE FIELD

Although it is beyond the scope of the chapter to provide a comprehensive history of the field, it is worth noting some key phases of intellectual history in order to set the stage for our own argument.¹ Indeed, for whatever reason, assessing the sociology of the field has become somewhat of a cottage industry in the past 15 years. And while we agree that some of this self-assessment is healthy, we would argue that the lack of such navel gazing in other fields (International Organizations (IOs), International Security, etc.) does not seem to have undermined intellectual progress in those areas.²

But rather than focus on the question of whether there are deep divides between American and European IPE or whether methodological differences doom IPE tribes never to communicate, we wish to establish more pedestrian arguments. Specifically, we want to emphasize what are some of the commonalities among IPE scholars, recognizing that there is always diversity in any intellectual enterprise. Moreover, while we will not paper-over the differences between groups of scholars working on IPE questions, our intent is to suggest that at the current time, those differences have not undermined the field. This does not imply more dialogue would be harmful to intellectual progress nor can we reflect on the historical counterfactual (if our differences had been worked out, we would have a self-enlightened field solving all issues before us). Rather, we argue that IPE was born as a practical, problem-oriented field. It continues to be so today, even if there appear to be theoretical and methodological camps engaging in differing research programs.

Again, a complete history of the field is beyond our remit and others have tilled that fertile ground (Cohen 2008). We would point out, however, that in those histories, a common theme of the birth stories and coming-of-age tales is that the field rose in response to real world events (see Cohen 2008, 21–3). Whether it was the rise of the postwar economies of Europe and of Japan, the concomitant rise in trade interdependence between those actors and the US, the growing power of the European Community, the “end of Bretton Woods,” the oil crisis of 1973, or resource shortages, the interdependence of economics and politics was reaching the front pages of newspapers, thus sparking new research programs among international relations scholars. No longer was economics or foreign economic policy the stuff of “low politics.” Economics mattered to politics and many of the dominant theories of postwar scholarship did not recognize that fact.

Neither were these global changes the stuff of the field of economics (Cohen 2008, 19). Susan Strange (1970) was critical of both international relations and economics as she witnessed what she viewed as fundamental changes to the global economy, arguing that scholars in both fields should engage with one another. Strange, working in Britain, had common cause with her American colleagues. Nor was this “reciprocal and dynamic interaction in international relations of the pursuit of wealth and the pursuit of power” (as famously defined by Gilpin [1975, 43]) a concern only for academics. As Robert Keohane (1975) reflected in his inaugural issue as editor of *International Organization* (IO) “Many of the problems [of interdependence] from which such issues arise . . . have immediate impacts on people’s daily lives.”

IPE was thus born out of a recognition that politics mattered for economics and vice versa. In his own editor’s Introduction in 1981, new IO editor Peter Katzenstein (1981) argued that journals “both reveal and reflect on the politics of their times.” Similarly, when reflecting on his editorship of IO in the 1980s, Stephen Krasner (2016) argued “the study of international relations has never been divorced from contemporary events.” The field of IPE pushed against the (now infamous) Morgenthau (1948, 11) dictum that “Intellectually, the political realist maintains the autonomy of the political sphere, as the economist, the lawyer, the moralist maintain theirs.”

Perusing the early journals dedicated to the study of IPE suggests the reflections of Katzenstein, Krasner, and others about the origins of the field hold water. The pages of *International Organization* discussed the implications of interdependence in trade, exchange rate reforms in the aftermath of the Smithsonian agreement, the growing power of international financial institutions, and the growing power of multinational corporations.

The journal *Review of International Political Economy* (RIPE) was launched on the premise that economies were becoming financialized, technological innovations transnationalized, that corporations were now global, states were bargaining with firms under regulatory uncertainty, global culture was transforming, and production was being geographically rescaled on a global basis (RIPE Editors 1994). While these journals differed in their claims to being best practice social science in the field or a new “heterodoxy,” they shared concerns about contemporary change in the international political economy. The same is equally true today. Today’s IPE journals are filled with explorations of the Great Recession, shifting public opinion on trade, the effectiveness of foreign aid, the determinants of foreign direct investment (FDI), the role of remittances in the global economy, and international financial and trade institutions. And while the methods used to explore those topics are increasingly specialized, the underlying questions reflect a concern with events in the “real world” and the concerns of policymakers.

While IPE is sometimes maligned for being too narrow (Oatley 2011) or not reflective enough of significant events like the Great Recession (Helleiner 2011; Blyth and Matthijs 2017), the field does tackle key questions regarding the international political economy. While there may be some lag between events and theorizing about them, we take this as evidence not of fiddling while Rome burns, but rather a reflection that IPE scholars are not journalists. In their assessment of the role of IPE scholarship in shaping policy, Mansfield and Pevehouse (2020) suggest that although academic IPE work has had a limited impact on policy debate, those very policy debates inspire academic work and guide the IPE research community’s agenda.

To take two recent examples, the race to understand the emergence and popularity of populist nationalism, and its political and economic effects, has drawn many IPE scholars into the study of public opinion and electoral politics (e.g., Konstantinidis, Matakos, and Mutlu-Eren 2019; Milner 2019; Margalit 2019). Even more recently, the COVID-19 global pandemic and resultant economic challenges drew significant and nearly immediate attention from IPE scholars (Stasavage 2020; Yi Dionne and Turkmen 2020). Indeed, the economic uncertainty of the 2020 global pandemic and economic consequences are likely to endure as a subject for IPE scholarship just as the aftermath of the Great Recession is still interrogated for its domestic and international effects. Unanswerable at this moment is whether the various research communities we discuss in the next section will develop fundamentally different approaches to the study of the parallel economic, health, and race-based crises.

But we believe that if there is a strong bias in IPE, it is toward scholars valuing theory—although not theory for theory’s sake. This bias is what helps separate IPE from other approaches and disciplines. Again, IPE scholars are not journalists but analysts. And as journals have moved online, provide quicker turn-around, and can disseminate knowledge more quickly, we argue it is essential to anchor analysis of current events to the rich theories and concepts that are the core of IPE.

Those who contend that the field is too theoretically oriented miss the mark. While no one would describe the field as a-theoretical—an examination of the TRIPs journal data reveals that between 1995 and 2014, a mere 1.4 percent of IPE articles can be classified as a-theoretical.³ Thus the vast majority of IPE scholars engage in some type of theoretical work.

Yet rarely are the debates in our field about the theories themselves (e.g., the neo-neo debate in Industrial Relations (IR) of the 1980s). Rather, theory is used to give IPE scholars a

tool, even provide a shorthand for a set of assumptions. And for the most part, those tools provide analytical leverage for investigating problems that confront policymakers and global citizens on an everyday basis. Again, turning to the TRIPs journal project, the overwhelming picture that emerges is of a field (at least for the TRIPs journals) where theoretical debates are rare. In the latter 20 years of that data, articles classified as non-paradigmatic or Liberal make up 91.3 percent of published papers in IPE (the two numbers are 43.9 and 47.4 respectively).

Of course, one could see the high number of papers hewing to the Liberal tradition as evidence of a very theoretically introspective field. We doubt that interpretation. First, the TRIPs data also codes what theory is used to counter the one presented in each article. In other words, what theories are commonly chosen for the other side of the theoretical fight. Within IPE papers, a full 71 percent are coded as “none.” This is hardly the mark of a field where large theoretical debates are ruling the day.

Second, we suspect that even within the Liberal paradigm, what leads most of the papers to be coded as Liberal is their discussion of domestic politics. Indeed, TRIPs coders are instructed to classify papers that develop domestic political explanations as Liberal theory (Maliniak et al. 2011). All of this evidence points to a field that values theory, but values it as a tool to understand contemporary problems in political economy.

Why, then, do others (e.g., Maliniak and Tierney 2009) conclude that theoretical debates have dominated the intellectual landscape of IPE? Is it possible analyses of the TRIPs data were incorrect? Is our recall of our days of graduate training ruined by academic committee duties? Are there not still deep theoretical schisms that dominate the field?

While our recall may be faulty, we will provide data in the next section that buttresses our case that the divisions in the IPE field are overstated. Moreover, it is worth reflecting why the myth of the theory-dominated field has emerged.

One possible reason for this perception is that the debate in the Cold War over the veracity of the dueling ideologies of the day, capitalism versus communism/Marxism, occurred over IPE-related issues. For example, World Systems Theory spoke to the global organization of the means of production (Wallerstein 2011). According to the TRIPs journal data, between 1980 and 1994, nearly 14 percent of IPE papers utilized Marxism as their theoretical lens of choice. Of course, after the fall of the Soviet Union, this changes dramatically. That same number in the 1995–2014 period is not quite 2 percent. We recognize that while explicit commitments to Marxist approaches have greatly reduced, post-Marxist approaches have proliferated through the “Amsterdam school” and the growth of “cultural political economy” (Overbeek 2004; Jessop and Sum 2006).

Second, we would argue that the “neo-neo” debate of the 1980s and the related “isms” debates featured some key IPE figures as protagonists—scholars like Robert Keohane, Lisa Martin, and Stephen Krasner. It was thus easy to view IPE at the center of these debates, especially when some of the key papers appeared in one of the flagship journals of IPE, *International Organization*. Even in the heyday of these debates, key figures like Krasner suggested that “For social science, and especially for international relations, the number of cases is often too small and the nature of the evidence too ambiguous to put opposing camps to flight” (Krasner 1994, 18). The debate was often a reflection of ontological and epistemological scholarly commitments.

A third reason for this disconnect is found in multiple studies of research and teaching in IPE. Both Maliniak and Tierney (2009) and more recently Seabrooke and Young (2017) find that there is a significant disjuncture between what is taught in IPE seminars and

what is contained in IPE research. The short version of the finding is that our IPE seminars often structure the field as pitched theoretical debates that structure the questions that are answered. Yet these same studies, especially Seabrooke and Young (2017), suggest that the evolution of the literature over the past decade has not followed along these theoretical debate-structured lines.

This leads to a set second of questions regarding the field: even if it is not dominated by arcane theoretical debates and responds to “real world” events, is IPE not deeply divided by method and epistemological debates? As a first answer to this question, we would point out that even if such divisions existed, it does not imply that IPE scholars are not practical and focused on contemporary issues. Empirical methods, like theories, are tools. The choice to use one over another is the choice of individual scholars. It may not be surprising that, within particular research communities or even particular geographically-limited scholarly communities, certain tools gain favor while others do not.

Thus, it is not inaccurate to suggest that there are somewhat distinct research communities that have developed on both sides of the Atlantic or that some research communities have felt under-represented or misrepresented in the field (Phillips and Weaver 2010; Helleiner 2009; Murphy 2009; Underhill 2009). As we develop more in the next section, our view of the current structure of the field is one of “constructive non-engagement,” where IPE scholars around the world engage in practical questions, each armed with their own set of theoretical and methodological tools.

THE STRUCTURE OF THE FIELD

This analysis is based largely on Seabrooke and Young (2017), who investigate the intellectual structure of the field using community detection methods. They do so in multiple venues: publications, graduate training (through syllabi), and conference participation. They juxtapose two possible organizational patterns for the field: reduction to polarity versus niche proliferation. The former has often been used to describe the trajectory of IPE: “British” versus “American,” rationalist versus constructivist, or positivist versus post-positivist. The latter is consistent with scholars “constructing local environments out of bits of the larger intellectual community that they identify with” (Seabrooke and Young 2017, 3).

Their empirical inquiry begins with collecting several dozen IPE textbooks as well as over 170 syllabi from 16 countries. To create a list of journals to analyze for citations for detection purposes, they code the journals referenced in those textbooks and syllabi. After selecting IPE articles in the core set of IPE journals, they analyzed citation patterns in 645 articles. Next, community detection methods were used to analyze citation patterns in these articles. These communities are detected around common referents from works that themselves are adequately cited enough to be important in IPE. So, if Helen Milner and Nicola Phillips have independent works that are above average citation threshold for IPE articles and those works both cite a work from Susan Sell, then they have a tie within the network. Following this logic, Seabrooke and Young use a community detection algorithm that applies this logic across the entire citation network for IPE between 1994 and 2015, building distinct communities based on citation patterns.

They then examine three windows of scholarship, 1994–2000, 2001–2008, 2009–2015, to assess how networks of IPE scholarship have changed over time. They find that in the first window there were scholarly groups based around a common theoretical approach, particularly a post-Marxist group, but most groups were already concentrated on particular issues such as finance, the environment, or multinational corporations. In the second window the clustering of groups was more around issues areas, such as international organizations, and global value chains, and also around quantitative and qualitative methodologies. In the third and most recent window there is a large rationalist group sharing a common analytical approach but most of the network is clustered primarily around their issue areas with mixed methodological orientations, with a strong focus on international organizations and financial regulation. In this window the post-Marxist scholars are less present, as are those working on development and global production issues. Seabrooke and Young suggest that these scholars nurtured their own specialist journals outside of the mainstream or migrated to different scholarly fields where they are more appreciated, such as economic geography.

This issue-centered framework is consistent with our arguments in the previous section: global problems drive IPE scholarship. What boundaries may exist between IPE scholars are about research areas, not methodology or epistemology. And while there are certainly differences in methodology and approach between scholars in Europe and North America, the citation data suggests distinct clusters of scholarship around core questions in the field.

What of Seabrooke and Young's findings of a more bipolar structure on syllabi and in meeting attendance? These findings are especially interesting given that the goal of both of these activities is publication in peer-reviewed journals. If our teaching and professional networks are more bifurcated than our scholarship, it suggests that while we may use "great debates" to structure our courses and that we would like our working papers to receive comments from relatively like-minded individuals, the publication process seems to correct for these preferences. Authors, knowing they must cite and engage a broad variety of work, expand their horizons to others who perhaps work outside their own corner.

We note that these findings are consistent with others' observations of the field. Blyth (2009), for example, notes that no community has cornered the market on exclusionary thinking in their research. Yet, he notes, despite views of "the other" that are exclusionary, there is an openness in most IPE communities to intellectual engagement with that "other." Ravenhill (2007) is also critical of the idea of polarizing the field into two variants of British and American schools. Rather, he argues, much of the interesting work in the field is done in the "middle."

How has the field come to be structured this way? One possibility is that the enormity of the field of IPE has allowed a thousand flowers to bloom. In a narrower field with few topics to explore, scholars must naturally turn to engaging one another on all issues related to an outcome or process. There is a strong incentive for IPE scholars to concentrate on explaining "what goes on," regardless of their theoretical viewpoint.

This also has practical consequences for how IPE works as a field. Scholars engage each other on their capacity to explain phenomena around which there is a perceived stake as an object of analysis. IPE scholars compete from their capacity to explain, for example, what drives financial regulation (Pagliari and Young 2014; Quaglia and Spendzharova 2017) or the determinants of FDI (Jensen and Rosas 2007; Arel-Bundock 2017). Not much throat clearing is needed for competing explanations to be established because the stress is not on demonstrating the superiority of the theoretical approach at a meta level. (This is also why

IPE journals have lower impact factors compared to fields such as economic geography or management studies, where articles spend masses of time and space surveying, and citing, prior contributions).

We have presented this plurality of research communities, which do not extensively engage with one another, as a neutral state of affairs. What is the ultimate cost of this constructive non-engagement? Before reflecting on that question, we would make one point about the non-engagement. Our earlier, quite nervy hypothesis that a theoretically and empirically convergent field would be solving all of the world's problems is humorous not only because it conjures the image of academics saving the world, but also because it relies on some process by which real intellectual differences are either resolved or set aside.

This would not be a pretty sight. As one can recall from the "isms" wars in IR theory in the 1980s, the debates become philosophical wars as much as they become questions about policy or politics. Substance becomes lost in favor of debates over assumptions. And while we believe assumptions can and should be fruitfully debated (unlike Waltz 1979), doing so outside the context of a specific research question risks a reduction to philosophical memes. And while this may be the stuff of PhD comprehensive exams, it does not make for successful discovery in a problem-driven enterprise (similarly, see Lake 2009).

We make this case to emphasize that costs of non-engagement are relative. Yes, scholars should engage in literatures that go beyond their own ontological or epistemological biases. But that requires that there are other healthy bodies of literature to reflect upon. This is what has been developing in our young field over the past 30-plus years.

ORGANIZATION OF THE HANDBOOK

The problem-driven approach to IPE suggests an organization strategy that revolves around the problems themselves. While not each chapter in the Handbook is indeed a "problem" to be solved (notably the Field Overview and Methods sections), nearly all chapters focus on a particular area of research that has emerged as a distinct set of arguments. Again, with this approach we hope to move past earlier debates in IPE that were characterized by Susan Strange (1994, 218) as "'Catholic complexity' (eclecticism) versus 'Protestant parsimony.'" We have attempted to combine both elements by bringing together a diverse range of perspectives on important and emergent topics. The contributing authors were asked to survey their particular topics but in a manner that pushes debates in the field forward while challenging and provoking readers. Again, we also asked authors to assess the state of "constructive non-engagement" in their area of research.

The eight sections of the Handbook focus on field overviews, methods, processes, forums, actors, flows, outcomes, and assets and resources. Our list of topics is not exhaustive but does cover large parts of the field of IPE. We have pushed authors to speak to IPE as a general field rather than to their immediate intellectual peers. Ensuring this will enable the Handbook to speak to IPE scholars in North America, Europe, Asia, Australasia, and elsewhere.

The section on "The Field of IPE" provides chapters on the state of the art from the major approaches in the field. After a historical analysis of the field, the section moves through several approaches. We stress that these chapters do not provide surveys of "isms," such as

Liberal Intergovernmentalism or Constructivism, but focus on how IPE is treated by scholars who have an interest in, for example, foreign economic policy, or viewing IPE dynamics as reflections of power asymmetries in global social structures. We have included key traditions here, such as those working on states and markets (the Strange tradition), foreign economic policy, open economy politics, and historical materialism (which combines the Marxist and post-Marxist literature). We also include chapters dealing with two themes that have received comparatively less attention in the field's young history: gender (through a feminist lens) and race. These field-level essays reflect on the state of affairs in these approaches and also locate the various disciplines that inform IPE, including Political Science, Economics, Sociology, Economic Geography, and others.

The section on "Methods" introduces readers to the current state of the art methods in IPE. The chapters will also serve as a platform to support the argument that methods can be used to bridge the gaps between scholars of different theoretical traditions. Included in this section are contributions discussing formal methods, experimental methods, networks (including social network analysis), content analysis, process tracing, and historical methods (including archival methods). These chapters provide an overview of the methods and contributors were asked to discuss these methods in substantive areas familiar to an IPE audience. In this way the chapters should spark a discussion about the diversity of methods in IPE and what is best suited to making robust intellectual claims.

The remaining sections all focus on some aspect of key issues and concepts that are the focus of the field of IPE. The "Processes" section discusses the key mechanisms that have provided much of the debate in IPE in recent decades and have been the focus of a great deal of work in the field. The chapters here include discussions of diffusion, coercion, and learning, well-known processes in much IPE work but rarely given space to be discussed on their own. This section also includes a chapter on regulation that focuses on how regulation is made in different contexts and systems in the international political economy, as well as a chapter on accumulation that is a crucial process for modern capitalism and speaks to Marxist and non-Marxist scholars alike.

The section on "Forums" focuses on places of decision-making activity between different actors. These forums are arenas for competition between agents in deciding policy and governance outcomes in the international political economy. The chapters in this section focus on a range of institutions, such as formal intergovernmental organizations, informal organizations, international financial institutions, and regional institutions. The section also includes more event-based institutions such as trade agreements, investment agreements, and corporate networks, where a range of public, private, and non-governmental actors interact.

The section on "Flows" concentrates on the substance of what is being exchanged in the international political economy. It also concentrates on the type and form of exchange, as well as what is being exchanged. Chapters here include topics such as money, trade, investment, labor, migration, and foreign aid. This wide range of flows covers a great deal of activity in the international political economy and contributors were asked to focus not only on the public face of what is being exchanged but also, where relevant, illicit flows.

The section on "Actors" discusses how different actors in IPE are conceptualized and understood, ranging from international organizations such as the International Monetary Fund (IMF), or the role of households in "everyday" international political economy. The

chapters here also engage actors such as states, firms, interest groups, households, and NGOs. We note that households should be included because of the role of housing debt and consumption in the world economy—indeed the most recent international financial crisis could not be explained without looking at housing.

The section on “Outcomes” explores policy evolution, stability, and change, in a range of systems. The chapters include some well-known phenomena to IPE scholars, as well as lesser-known but important topics. The chapters discuss policies and outcome including liberalization, gender mainstreaming, political-military relations, financial systems, development, informal markets (including corruption), and environmental cooperation.

The section on “Assets and Resources” places attention on objects of dispute in the international political economy, where there are contests and attempts at cooperation. As with the above section on Outcomes there are some well-known cases here and some newcomers, including oil and gas, knowledge (which includes themes of human capital production), territory, and capital. As the building blocks of IPE these chapters will explore how these assets and resources are treated and traded.

In each of these sections (from “Processes” to “Assets and Resources”), we have asked authors to focus very specifically on how attention to these particular issues has helped move the field away from larger theoretical debates to a focus on the issues themselves. And where our “constructive non-engagement” exists, whether it has led to more or less knowledge accumulation.

Again, with any volume that purports to survey a field, readers will no doubt find holes in the topics, theories, and methods discussed. Some will lament the lack of an exclusive focus on positivist-oriented work. Others may criticize a lack of attention to the emancipatory potential of scholarship in this area. Still others will wish to see more critical reflection on the state of the divide between the British and American schools.

Yet we hope that in this Introduction we have laid out a compelling case that problems and challenges confronted by peoples, states, and firms lie at the center of research in IPE. And while individual scholars make different choices as to the theoretical tools they use to understand and analyze those problems, and there may be patterns to those choices, all of this is in the service of improving knowledge about the interactions that form the international political economy.

NOTES

1. For extensive discussions of the history of the field, see Cohen 2008; Cohen 2014; Ravenhill 2007; Katzenstein, Keohane, and Krasner 1998.
2. We realize a) both of us have undertaken and published such navel gazing (Seabrooke and Young 2017; Pevehouse and von Borzskykowski 2016) and b) nearly every navel gazing paper contains a sentence akin to this one.
3. The journals coded by the TRIPs project are: *American Political Science Review*, *American Journal of Political Science*, *British Journal of Political Science*, *European Journal of International Relations*, *International Organization*, *International Security*, *International Studies Quarterly*, *Journal of Conflict Resolution*, *Journal of Peace Research*, *Journal of Politics*, *Security Studies*, and *World Politics*.

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PART I

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THE FIELD OF IPE
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CHAPTER 2

MODERN IPE

Lessons from a Counter-History

RANDALL GERMAIN

INTRODUCTION

As the Introduction to this volume suggests, there is a remarkable consensus about the modern origins of International Political Economy (IPE). Its call to arms is often associated with an article published in 1970 by Susan Strange, in which she decries the mutual neglect by two well-established disciplines to consider how they interact with and affect each other's primary subject matter (Strange 1970). Her principal argument, that international relations needs to understand how the organization of markets affects politics and that international economics needs to understand how considerations of power affect the economy, was a clarion call for the establishment of a new discipline within the academy. Many follow Benjamin Cohen in marking this as the formal birth of modern IPE (Cohen 2008, 1).

In this chapter I interrogate this consensus by considering the curious case of IPE's failure to launch as an academic discipline after the Second World War, when the demand for the kind of knowledge it produces was highly salient. Starting in the 1930s and continuing throughout the war, a concerted debate had arisen about the meaning of what E. H. Carr (1946/2000) famously called the "twenty years crisis," in which the question of how to reassemble a functioning world economic and political system was particularly prominent. This debate generated many diagnoses and prescriptions for how to set the world aright after 1945, but it is instructive that nowhere among this avalanche of ideas was there a call to launch a new discipline of academic teaching and research comparable to the inauguration of the Wilson Chair in International Politics at Aberystwyth University in 1919, whose remit included a teaching program in the new subject of "politics among nations." Instead, questions concerning the international features of political economy became the provenance of economists, civil servants, and some journalists, few of whom connected in a systematic way the principles and insights of a conception of global political economy as we would understand it from the vantage point of modern IPE.

Yet, as I detail in this chapter, there were incredibly perceptive and insightful interventions that at the time were very much framed in terms which are recognizably "IPE," and which

held the promise of launching a genuine academic conversation about the imperatives and forces which were shaping an increasingly intertwined international economic and political system. That these interventions failed to launch an academic conversation carries interesting implications for contemporary scholars of IPE, as it suggests that the academic future of IPE rests much more emphatically than we perhaps realize with carving out a sustainable institutional “home” for our work, and perhaps even more importantly (and controversially) that this “home” should be built within a single disciplinary architecture rather than across a multi- or inter-disciplinary one. The main reason for this is that disciplines prosper or fail in part on the basis of the rules laid out for the conversations which they entertain, and where there are no or few agreed upon rules, disciplinary structure is very difficult to maintain. Kevin Young (2020, 5–7) has argued that such a disciplinary structure encourages the emergence of a form of “alienated pluralism,” where disciplinary conversations become unfocused and splintered. My excavation of IPE-inflected debates at mid-century supports his concern, as this is indeed one lesson we may draw from the experience of the non-emergence of IPE after the Second World War.

1968 AND ALL THAT: THE DISCIPLINARY PRE-HISTORY OF MODERN IPE

When Strange penned what some call her “IPE manifesto” (Cox 2009, 319), she was responding to the growing realization that the established conventions of the post-1945 global economy were collapsing under the growing weight of what today we identify as “globalization.” She was trying to bring together a series of disconnected analyses which, in different ways, were searching for an intellectual framework that could tie together a singular account of these pressures. What she recognized, and which subsequent IPE scholarship has accepted, is that globalization is the specific and particular outcome of the intersection of multiple strands of behavior, some of economic origin, some political (in which she included military aspects), some legal, and some springing from cultural or civilizational roots. And while many scholars had grasped individual elements of this complex ensemble, none in her view had yet provided a sound disciplinary grounding that would enable the diverse features of these phenomena to be considered as a totality in their own right. This for her was the promise of establishing a new disciplinary landscape.¹

Of those efforts undertaken to analyze the interconnected nature of the corrosion of Bretton Woods, Strange was most impressed by the 1968 publication of Richard Cooper’s *Economics of Interdependence*. She considered this to be a landmark publication because it correctly identified the political consequences of growing economic interdependence, which was precisely that further cross-border economic integration required not only international political cooperation for its management, but also deeper political adjustments within those states which were hurtling down the interdependence highway (Cooper 1968, 264–9). Here was an economist at a respected top-level American university who clearly recognized that the expansion of market economy was dragging political structures connected to states in important new directions. In her view, Cooper was among the first to argue that interdependence was as much a political as an economic phenomenon, and that both political

and economic dynamics had to be considered to provide a complete analysis of its genesis and future possibilities.

The fullest account yet of this formative period of modern IPE comes to us from Benjamin Cohen (2008), who tracks Strange's own history quite closely. He details how in the decade following the publication of Cooper's path-breaking work, subsequent research established with greater clarity the precise ways in which political and economic dynamics intersected to form the scope conditions within which the global political economy evolves. The important point made by Cohen is that some economists—such as Charles Kindleberger—recognized that international politics were a critical foundation of a global economy, and this admission was seized upon by a determined group of political scientists to establish a new framework through which to situate and analyze the structures, organization, and attendant issues connected to what we would come to understand as “interdependence.” By the 1980s these scholars were calling this new framework IPE, and they were teaching and researching it from university departments of political science all across Europe and North America. Thus did modern IPE emerge as a field of study.

This version of modern IPE's birth, however, has not gone unchallenged. Perhaps the most sustained challenge to Cohen's history has been over his conception of IPE itself, which many see as much too narrow and centered on debates with a considerable American inflection. Whether it is about contesting the Euro-centric bias intrinsic to defining the issues and themes connected to IPE (Hobson 2013), or about recovering forms of thought that are not entirely bound up with Western ideas (Chin et al. 2013; Helleiner and Rosales 2017), many feel compelled to interrogate Cohen's initial premise that IPE remains tightly bound to Western Enlightenment thought, or at the very least that it needs to broaden its intellectual horizons if it is to become a genuinely global conversation (Phillips 2005; Blyth 2009).

Almost as significant among Cohen's critics are those who question the intellectual lineage that he has traced for IPE. Even within what John Hobson (2013) identifies as a Euro-centric tradition, Cohen confines himself to a fairly narrow range of ideas that he connects to modern IPE. These foundational sets of ideas are associated with classical and liberal political economy, for the most part, and pertain to those thinkers who have moved analytically from the domestic to the international arena, including Smith, Hume, Ricardo, and List. It is not so much that Cohen ignores a broad swathe of thinkers who many modern IPE scholars hold as their intellectual inspirations, but that he collects and categorizes foundational thinkers overwhelmingly in terms of their connection to the liberal tradition of political economy. Many find this does a disservice to the much broader over-arching framework of IPE, because it ignores contributions to IPE from historians, sociologists, and ethnologists, as well as from legal, race, gender, and postcolonial scholars, among others.²

Even if we agree with many of Cohen's critics that there is a far richer range of ideas to draw on as inspiration for our IPE analyses, and that IPE itself should be constituted in ways that are broader than Cohen seems to allow, it is instructive that one of his principal contentions—picked up from Susan Strange—remains largely intact, namely that the basic outline of modern IPE was launched only in 1968 or thereabouts. How can it be, given the myriad of fundamental political economy problems on an international scale that have afflicted societies over the entire twentieth century, that IPE is only about a half century old as a formal, systematic field of enquiry, even if its roots (wherever they may lie and however far back in time they reach) are considerably older? Inspirations need to be harvested, and our harvesting, it seems, only really took flight after 1968.

But is this really the case? I think it is, despite the arguments that can be made about previous IPE-inflected conversations which we might highlight. Some efforts, like the debate ignited by Keynes's classic treatise on the economic effects of the Versailles Treaty, remained confined to elite-level conversations,³ while others, such as the considerable efforts made by those in the Marxist tradition to theorize global circuits of capital, were largely organized around pre-existing conversations dedicated to the advancement of a particular view of the world (or a distinct political cause).⁴ We might also point to scholars from different traditions who have provided inspiration for IPE research, even if it has taken some time for their contributions to be recognized as such.⁵ In all these cases, the work in question—while subsequently of much value to IPE scholarship—failed on its own to securely anchor a recognizable IPE conversation. This requires institutional settings and ground rules, and these had yet to be established.

Exhibit A of how this works can be provided by considering a conversation that failed to launch, despite everything being in place for it to occur. Almost all of the necessary building blocks were in place for some kind of IPE conversation to emerge immediately after 1945. The Second World War capped a tumultuous decades-long period in which fundamental economic and political changes cracked empires, engendered revolutions, thrust new states to the fore, unleashed technological innovations, refashioned all manner of existing institutions, and rewired social relations in terms of how people mobilized and what they demanded. Governments, businesses, private groups, and intellectuals were all grappling to understand and react to what was happening, and in universities across Europe and North America social scientists and historians were working to provide solutions to the transformations which confronted everyone.⁶ How could IPE be absent at such a moment?

In what follows I want to lay out what I consider to be an opening to IPE at this precise moment in time, constituted by the publication of a series of works that made a deep and fundamentally IPE-oriented contribution to public debate about the shape of the postwar world. Yet these works did not result in a call for IPE to be established, nor did they lead to the teaching of courses on a subject that resembled IPE. All of this was only kicked off after 1968. Why? My answer to this question informs the lessons I draw from this experience.

ELEMENTS OF IPE AT MID-CENTURY: POLANYI, CARR, AND MITRANY

Framing the Prospects of an IPE Conversation

At first glance, the claim that the work of Karl Polanyi, E. H. Carr, and David Mitrany⁷ can be yoked together to constitute an opening to IPE may appear counter-intuitive. Carr and Mitrany are rarely mentioned in existing disciplinary histories of IPE, and if they are they are usually counter-posed to each other intellectually because of their seemingly contrasting views on international organizations.⁸ Polanyi, on the other hand, has a strong presence in the field, powered primarily by the way in which several concepts that he developed have been widely taken up in IPE scholarship, among them the distinction between embedded and disembedded economies, the idea of a double or counter-movement, and his conception of

the institutional foundations of the nineteenth-century international economy.⁹ Yet, as I will detail, not only did these three scholars agree on the fundamental nature of the challenges facing the postwar world and the direction to be taken to meet them, many aspects of their respective solutions also resonated with each other. From the mid-1930s through to the early 1950s they pursued what should be described as an effort to consider the problems of the global political economy from the perspective of what we can identify today as modern IPE.

It is also worth noting that they themselves knew each other and interacted professionally to some extent during this period. For a while all three lived in London or its environs, and each was active in different forms of public intellectual life. Polanyi, for example, taught and lectured in workers' venues and educational institutions during his years in London (Dale 2016b Ch. 4). Carr and Mitrany were both involved with Chatham House, host to the Royal Institute of International Affairs, over much of their professional lives, and during the Second World War Carr and Mitrany participated in study groups sponsored by Chatham House and connected to the British war effort. It is almost certain that they would have attended some of the same events.¹⁰ Indeed, there are many similarities between sections of Carr's *Conditions of Peace* (1942) and Mitrany's *A Working Peace System* (1943/1966), both of which were in development at the same time (and most likely discussed at Chatham House). There is no direct evidence that Polanyi and Mitrany interacted professionally, although Mitrany did correspond in the 1950s with Polanyi's younger brother, then a lecturer at Manchester University, over issues connected to Michael Polanyi's role with the Congress of Cultural Freedom.¹¹ He also apparently hired Polanyi's wife, Ilona Duczynska, to undertake some research for him in connection with his book *Marx Against the Peasant*, but nothing beyond this is suggested in his archives.¹² Carr and Polanyi, on the other hand, carried on a correspondence over many years, stemming in part from Polanyi's deep admiration for Carr's analysis in *The Twenty Years Crisis* (Dale 2016a, 2016b). All told, in other words, not only were these three scholars writing on world events using what I consider to be a modern IPE frame of reference, they were well positioned professionally and even to a certain extent personally to launch an IPE "conversation."

How then can we describe the work of these three intellectuals as engaged in a conversation deeply inflected by IPE as we would understand it today? I think their conversation coheres around three thematic concerns that are recognizably IPE in orientation: the fundamentally transformed foundations of the world and its political economy; the international scale of the economic challenges facing the world; and the implications of expanded state power for the conduct of international politics. I will assemble each element of their "conversation" in turn.

The Challenge of Fundamental Change

The starting point for each of their analyses coalesced around the origins and consequence of the Industrial Revolution. For Polanyi, of course, the Industrial Revolution was grounded in the release of the market economy from the shackles of social convention, and this unprecedented movement severed the social bonds connecting English laborers to their society, which nearly destroyed their social fabric in the process. Nothing could be more fundamental or transformative than this process, which set the template for the reaction of societal self-protection which he labels the "double movement" (Polanyi 1944/1957, 76). Carr

emphasizes a slightly different outcome of the Industrial Revolution, namely how it recast the economic landscape. While both Polanyi and Carr are preoccupied with and critical of the role of liberalism in organizing market economy, Carr pays more attention to the way in which one feature of the Industrial Revolution—the rise to prominence of large, centralized business concerns—upended the classical market economy of competitive firms (Carr 1951/1957, 27–32). The main effect of this was to draw the state very deeply into the organizational logic of the market economy. As he notes, by mid-century the *laissez faire* competitive economy of the classical period had been replaced with the planned economy, and the state now guided the organization of economic life around cartels and large business concerns (Carr 1951/1957, 36).

Of the three authors considered here, Mitrany had perhaps the most openness to the positive effects of big business and its capacity to weather the sea-changes of the first half of the twentieth century. Indeed, he occupied a most unusual position as a public intellectual and academic who also straddled the business world. Not only was he appointed to a prestigious Professorship at Princeton University's Institute for Advanced Study, where he spent six months a year (except for the war years) from 1933 until the mid-1950s, but for several decades from about 1930 he was heavily involved in a private organization called PEP (Political and Economic Planning), based at Queen Anne's Gate in London and funded by British business concerns. Even more interestingly, from 1943 he was appointed "Advisor on International Affairs" to the board of Lever Bros and Unilever Ltd, an internationally active Anglo-Dutch conglomerate. This appointment provided him with a modest stipend, an office at Unilever House, and access to some of their corporate research resources until he retired in the mid-1960s (Pedler 1976, 199).¹³

Perhaps because of this direct involvement in the world of business, Mitrany entirely concurred with Carr on the economic effects of the Industrial Revolution. Most importantly, they redrew the economic landscape to push it toward a planning model, where large organizations had an advantage, but which also involved the state intimately and organically in the planning process. Even more forcefully than Carr, Mitrany recognized that as states became more embroiled in national planning, this imperative quite naturally—indeed, necessarily—extended into the international economy. In a pair of articles published just after the war he charted the consequences for international political life of the state's need to undertake planning to stabilize and make functional the international economy under postwar conditions.¹⁴ In his view this would sharpen international politics because more was at stake for the states involved; as the private sector became more dependent upon states to provide the common economic infrastructure that allowed private exchange to continue, formerly "private" issues acquired a much deeper public component, increasing the pressure on states to provide for economic success. In his view this made international politics a much more zero-sum game (Mitrany 1946, 1947; cf. 1943/1966, 70–4).

Mitrany joined Carr on another aspect of the deep-seated nature of change confronting the postwar world, which was how to organize and provide for what they both called the "service state," or what we would today call the welfare state. They concurred on the historic significance of the advent of social security and its provision by the state, and saw in this development a profound and deep-seated transformation in the way that citizens relate to each other, to the economy, and to their polity. But they also recognized—Mitrany slightly more emphatically than Carr—that this development raised the stakes for international cooperation. With states now seemingly held responsible for the social security of their

citizens—reinforced in many rich countries by the extension of suffrage to all adult citizens who would exercise their vote in line with the increasing salience of welfare-related issues (such as education, healthcare, housing, and even culture and leisure)—the consequences of providing for such security made the operation of market economy an even more important political issue with potentially conflictual international consequences. It is on this final point that Mitrany, Carr, and Polanyi are also joined: the scope conditions of the “great transformation” lay in the organization of *world* market economy, and especially in the imperative to recast its operative principles. How to do this was a critical concern which they agreed required concerted state action.

The International Scale of the Economic Challenge

Modern IPE is split on the relationship of the domestic to the global political economy. On one side are many who adopt what is now identified as an “open economy politics” (OEP) view, which is that the global economy is the sum total of all domestic (or national) economies added together in terms of their international economic exchange.¹⁵ IPE from an OEP perspective derives the structure of the global political economy from the cumulative interactions of its constituent elements; its structure and organization reflect how the preferences and interests of these elements get transmitted upward (and hence the focus on the triptych of ideas, interests, and institutions). On the other side are those who view the structure of the global political economy as its central determining feature, where pressures from the structure as a whole are refracted downward to the constituent units. The structure itself is the source of the dynamics which move it, whether we call this structure “global capitalism,” “global economy,” or even “world system.”¹⁶ Modern IPE is in part a dialogue among these competing visions over the weight and shape of the “global” for our subject.

Polanyi, Carr, and Mitrany are right at home in this dialogue, although they would come out on slightly different sides of the divide. On the OEP side, as it were, I would place Carr and Mitrany, but not without a caveat. For each of them the line of formation of market economy moves from the national to the global, and runs most importantly through the Great Powers of the day, principally the US and Russia, followed at some distance by Britain. For Carr and Mitrany, as we saw earlier, this would inevitably lead to the need to organize and plan also the global economy. Carr foresaw this very clearly in his wartime work, *Conditions of Peace*, in which he observes that the postwar world will be a world of disorganized capitalism, desperately short of raw materials and investment, but yet at the same time faced with overwhelming demand for reconstruction and employment opportunities (Carr 1942, 173–7 and 182–4). Only a few years after the war, in the lectures on *The New Society*, he extended these observations to include the effects of a deep-seated change in the attitude toward work and material living standards, both of which amplified the crucial requirement to plan economic exchange on an international scale (Carr 1951/1957, 87). Furthermore, he saw that the colonial revolution—then in its infancy but spreading quickly—would add to this demand because the new states of the world would not be satisfied with joining a world left to the dictates of *laissez faire* economics and the so-called “harmony of interests” (Carr 1951/1957, 95–6). From every vantage point that he contemplated the future of the international system, Carr saw the central role of state planning.

Mitrany also considered the importance of planning on an international scale for the global economy to operate successfully, but he did not always and necessarily connect this to states per se. Carr and Mitrany are often contrasted in terms of their views on the importance of international organizations in international relations, but in one important respect this is a false distinction, for both saw the intrinsic need for planning even though they believed it could best be carried out by different means. Carr's reading of modern history placed the state at the center of the action, whereas Mitrany was rather more impressed by the myriad activities carried out by delegated authorities that enabled modern life to progress easily from day to day, and which were not directly subject to ongoing government influence (even if they required government direction to be established). This was how, for him, politics and economics had become progressively entwined during the nineteenth century within national states, and he was convinced this was also the path to be followed on an international scale (Mitrany 1932, 692–7). The global economy required a common, public infrastructure for all the reasons Carr laid out, but Mitrany saw this infrastructure being established less by constitutional means (i.e., by formal treaties between governments) and more by functional means, by which sovereignty would, in his terms, be “abridged” from centralized to functional authorities which were better placed to deliver the services modern life demanded (Mitrany 1943/1966, 30).¹⁷

If Carr and Mitrany are in some respects precursors to today's OEP scholars, Polanyi comes down much more firmly (albeit not unambiguously) on the side of their critics. While he locates the origins of market economy in the history of the English countryside and the emergence there of artificial markets for labor, land, and money (his so-called “fictitious commodities”), he is also clear that the self-regulating market is the fount and matrix of nineteenth-century civilization, which is a market civilization (Polanyi 1944/1957, 3–5). It may be that the self-regulating market was an English invention, but it was also a global phenomenon, and this was an inevitable and necessary development. Market economy in effect needed to become world market economy to survive. It was also its tragic destiny to unravel precisely at the point that it became achieved, because for him world market economy is unsustainable socially, politically, and economically. This is the principal claim advanced by Polanyi about the utopia of *laissez faire* and the liberal dream of a market economy severed from its social moorings. But we cannot lose sight of the fact that for Polanyi the scale of market economy is necessarily worldwide. When it fails, no constituent unit can stand on its own; the entire system must be refashioned. This is the tragedy of the great transformation even as it is the promise of the double movement. It is, as Sandra Halperin (2004, 264) notes rather critically, a top-down conception of the global political economy.

On the question of the scale of the economic challenge facing the postwar world, our three authors are united in recognizing not only that a fundamental and deep-seated change has taken hold, but that this change derives from the aftershocks of the Industrial Revolution and the collapse of organized capitalism on a global scale. They understand that a great social revolution has accompanied this collapse, and that this revolution has generated a new form of collective political mobilization that has powered forward a double movement which is making new and unprecedented demands on behalf of workers and citizens around the world, whether they live in old established nations or new or about-to-become independent ones. As Carr put it in *The New Society*, the social and colonial revolutions provided the bedrock for a world transformed, and no feature of the old nineteenth-century institutions could offer guidance for the future.¹⁸ To chart the way forward required a clear understanding of

the problems confronting the world, the scale on which those problems were organized, and a set of prescriptions for how to tackle them. And whatever their prescriptions, Polanyi, Carr, and Mitrany were for the most part agreed that the initiative had to come from the state in its new, scaled up capacity.

The International Politics of an Expanded State

All three of our authors participated in the search for a sustainable postwar world, one that could meet the complex challenges thrown up by what they considered to be a great transformation. What united their efforts, when looking forward to the international politics of the postwar world, was the indispensable and powerful place of an expanded state in how the world would be ordered. They were adamant that a liberal system of world market economy was finished, and that something unprecedented and new was taking its place. They also all placed great emphasis on the world historical consequences of Russia's Five-Year Plans and America's New Deal, with Polanyi for example calling Russia's turn to "Socialism in One Country" the beginning of its second revolution and "the *first* of the great social changes that transformed our world in the thirties . . . [it] formed part of a simultaneous universal transformation" (Polanyi 1944/1957, 247, emphasis in original). This turn—whether in Russia or America or Britain—was everywhere led by states, which was necessary in order to preserve and extend the arena of freedom for individual citizens, which each associated with the provision of enhanced social security to citizens by their states.

Here, all three agreed on the nature of freedom in the modern world. They saw how "freedom" under liberal political economy had become subject to what Carr (1951/1957) called the "economic whip," and that to make it real required a new degree of social protection which was impossible to achieve in a self-regulating market. To fulfill the demand for social protection required extensive economic and social planning, and this planning could not stop at a country's borders. Mitrany (1946, 1947) was perhaps the clearest on how extensive international planning would have to become, but both Carr and Polanyi also recognized that international trade and investment would henceforth fall under the preview of some form of planning agency, whether global or—as in Polanyi's vision—regional in scope (Polanyi 1945; Dale 2016a, 417–20). The critical consideration here though was the movement of citizens *qua* workers from being subject to economic discipline to becoming active determinants of their own freedom: this movement would be achieved entirely through a greater role for the state in the organization of the market economy both nationally and globally.

The increasing importance of the state led our authors to consider more closely its democratic form. Although this concern is expressed differently by each, the overlap among them points to an injunction that the postwar world still needed to become much more democratic if the fruits of moving beyond liberal market economy were to be realized. For Polanyi, freedom requires society to protect its citizens from material privation and social deprivation, which is best accomplished by enabling individual citizens to work together to repair the bonds of social community without fear of starvation and poverty. Because only a common effort—that is, working through government—could achieve this, the rights of citizens would need to be strengthened so that government could not, as under fascism, arbitrarily strip its own citizens of their rights (Polanyi 1944/1957, 255). This to my mind is

co-equal with a strengthening of democracy itself, to place it on a sounder and more sustainable footing. Carr concurs with this conclusion, even if his reasoning runs more in terms of how new forms of collective political mobilization were spurring a deepening of democracy under new social conditions (Carr 1951/1957, 78–9). And for Mitrany, every advance in the provision of the technical necessities of modern life had been accompanied not by a loss of control for citizens over their daily lives, but rather by a strengthening of their sovereign control over the organization of modern life (Mitrany 1933, 122–30). Ultimately their shared point is that the demand for social protection had drawn the state much more deeply into the organization of the economy, and if the newfound power of the state for control of both national and international activities is not to be exploited and/or abused, stronger democratic ties between citizens and their governments needed to be formed. In this they see the possibility of a new era of democracy dawning in the postwar period as part and parcel of the new society.

The final point on which our authors cohere with respect to the international politics of the postwar world—and which resonates very strongly today—lies with the formative role of nationalism in political economy. This occupies a large place in how each analyses the evolution of the state and the particular way in which state structures respond to the multi-pronged pressures of world market economy. In Mitrany's view, while the nationalism of the nineteenth century was cultural insofar as it pertained to identity, the nationalism of the twentieth century is far more deep-rooted because it is about society and socialization (Mitrany 1943/1966, 41–2). As the goals of nationalist thought and behavior evolved, states would be pushed to yoke together more strongly the arenas of the national and the international, and he foresaw problematic consequences for how states would be able to negotiate the common infrastructure of the global political economy. This is why for him the functional approach to international administration offers such promise: if followed it has the best chance of obviating the high stakes that populations and their polities were placing on international negotiations for common institutions (cf. Ashworth and Long 1999). For Mitrany, in other words, precisely because politics was now much more deeply entwined within and across societies, it needed to be tempered and insulated from the delivery of modern life's core social security concerns. This for him was the chief promise of the functional approach to politics, both nationally but especially internationally.

Carr and Polanyi also recognized the enormous new power of nationalism, particularly its key role in unleashing fascism on the world. For both, in effect, fascism did not represent evil incarnate; rather, it was simply one possible response to the parallel development of nationalism alongside the corrosive effects of an unraveling liberal market economy. Polanyi's analysis of fascism in *The Great Transformation* saw it as the inevitable outgrowth of the utopian tragedy of liberalism; the link between the breakdown of market economy and the fascist scourge was for him direct and universal; it was not confined only to revisionist states (Polanyi 1944/1957, 239–42). Removing the tragedy of market economy—international capitalism as he also styled it—could shift the track of nationalism from a negative to a positive force in society, where it could secure the commitment to a welfare state because the social roots of nationalism recognized the societal need for re-embedding the economy into societal interests—for emphasizing “habitation” over “improvement” (Polanyi 1944/1957, 249). To realign nationalism away from conservative and counter-revolutionary forces and toward progressive, forward-looking elements of society which embraced planning at both the national and global levels was for him the clearest way forward to secure freedom in the

postwar world, precisely because it acknowledged society and its embedded and reciprocal social relations as the starting point to freedom (Polanyi 1944/1957, 258A).

Carr followed Polanyi in recognizing nationalism to be one part of the equation that disassembled liberalism as a social organizing principle. The liberal world was not able to withstand the rise of nationalism because the “re-discovery” of society allowed nationalism to challenge the myth of the harmony of interests and arrange an array of powerful forces against it. These forces were more naturally aligned with ideas expressing community and sociality, whether of the New Deal or socialist (i.e., Soviet) variety. Nationalism, in a way, was the revenge of society on liberalism, and it was unstoppable once called into being (Carr 1945, 26–34). Planning was also an essential feature of nationalism in the modern world, so for Carr the pertinent question concerned how far the institutions under nationalism’s purview would coalesce: whether on some kind of inter-imperial basis, a regional basis, or on some kind of universal basis. His answer evolved quite substantially over the period from 1942 to the early 1950s, but eventually settled on a form of state sovereignty overlain by the extensive capacities which the postwar superpowers had so convincingly acquired via their negotiation of the political mobilization mass democracy had ushered in (Carr 1951/1957, 81–91). By the time of the *New Society* lectures, he clearly saw that nationalism would remain captured by domestic social forces and institutions rather than universal ones, reinforcing the conclusions he first arrived at in the *Twenty Years Crisis* concerning the compromised potential of international organization as an expression of an effectual universal will (Carr 1946/2000, 209–13). Nationalism enhanced the force and durability of national states, thereby containing and restricting the “international” in his account of world politics. It deepened the power of states, and especially the Great Powers, and so would be part and parcel of the new world order for as far ahead as he could see.

CONCLUSION: LESSONS FOR THE DISCIPLINARY HISTORY OF IPE

I have laid out the case that modern IPE was ready to emerge directly after the Second World War. It had powerful intellects in its corner who agreed on the shape of the problems confronting the world, which, although they were many and intertwined, intersected around a core of political economy problems operating on an international scale. They further agreed that the new political and economic landscape confronting policymakers after the Second World War could not rely on established institutional arrangements for guidance; the world had entered a new era and new thinking was demanded. Why then did modern IPE not burst forth in 1945?

The literature on academic disciplinary history provides some food for thought with which to consider this puzzle (Schmidt 1998; Gunnell 1998; Abbott 2001). Academic disciplines coalesce when at least two preconditions are met. First, there has to be a conversation that is recognizable as such to its participants; they need to understand the debates as part of a string of interventions which build on each other. And second, there needs to be some kind of institutional locus or “home” within which such a conversation can unfold. The debate I have assembled in this chapter I think corresponds to a conversation that the

participants could easily recognize as belonging to a singular idiom or language of science, and there is little doubt that they at least in part engaged with and built on each other's contributions.¹⁹

What they did not share, however, was an institutional locus or home from which to engage in their conversation. Throughout this period Polanyi had only periodic appointments to educational institutions (Dale 2016b), while Mitrany failed to secure a permanent academic home in Britain, despite his part-time appointment to the Institute for Advanced Study. Carr was the only one of the three to enjoy a more secure academic employment, but even this was not without interruptions during this period, especially after the end of the war. And when he was finally appointed to a Fellowship at Trinity College Cambridge in 1955, he plunged directly into his research on the history of Soviet Russia, a project which was all consuming for him (Haslam 1999). The lack of secure institutional affiliations during the exact moment of their IPE-inflected conversations deprived these scholars of the means to build the networks that alone sustain disciplines, such as training students and inculcating them with an interest in the subject matter (cf. Seabrooke and Young 2017; Cohen 2008). Simply put, Polanyi, Carr, and Mitrany did not have students of any kind during this period, and because they were not attached to institutions in any permanent way they could not build up any kind of a network. They may have begun a conversation, but they were not in a position to carry it forward in any meaningful way.

There were other reasons as well for why their nascent conversation remained stunted, above and beyond the lack of an institutional home. Perhaps the most important ancillary reason was that they themselves came from very different disciplinary backgrounds. Polanyi's training was in sociology, anthropology, and law, and was drawn from an educational *milieu* which was thoroughly European. Carr had a classical Victorian education in history and the arts, and went directly into the civil service upon matriculation from Cambridge. Mitrany, originally from Romania, was schooled across Europe before arriving in Britain around the turn of the century, where he attended the University of London and was taught by L. T. Hobhouse and Graham Wallas, who developed his appreciation for the social sciences as a legitimate feature of the academy. He considered himself to be classically trained in political science, and throughout his career identified himself in numerous letters always as a "student" of politics or political science, and often also as a "Manchester liberal" in sensibility and intuition. In this sense they had quite different understandings of what a disciplinary conversation entailed, and were without a unifying set of ground rules to guide them through their conversation. With neither institutions nor ground rules to fall back upon, their conversation dissipated.

A final minor point to note is that in the absence of secure institutional homes and agreed upon ground rules, the interests and egos of participants become more salient features conditioning the development of a conversation. In the case of our authors, while Carr and Polanyi became over time quite warm toward each other, Carr and Mitrany remained throughout their careers mutually antagonistic competitors who were also highly invested in their own egos. In the end, it seems, they had little interest in carrying their conversation past its initial outpouring.²⁰ It would take another 20 or more years before modern IPE was ready to take off.

What lessons might we take from the curious case of IPE's non-emergence after the Second World War? For me, the most important lesson is that ground rules for conversations are critical, and that these are largely established through institutional means that include

providing an academic home or base for such conversations. Polanyi, Carr, and Mitrany did not have such a home base; thus their conversation ended when they moved on in different directions. Not only was IPE's establishment delayed for a generation, but both students and policymakers were denied the fruits of IPE research and teaching for that same period of time. We who are invested in the discipline should recognize that as a dead loss from every angle.

The logical corollary to this lesson is that, as scholars, we should perhaps be more concerned with the institutional health of our discipline than with its ideational health. As this volume attests, the traditions out of which IPE has coalesced remain as vibrant as ever, and in some ways are expanding as we turn to new (or indeed very old) academic fields to harvest their ideas for our own purposes. Indeed, modern IPE has never had a problem locating and using ideas; these are in a sense widely and readily available. Rather, we need to pay more attention to securing our institutional base, which includes ensuring we have positions at universities from where we can teach and research, generating funding for our research, nurturing networks where we can exchange and disseminate knowledge (i.e., conduct conversations), and conducting outreach to bring our ideas and research to wider audiences including policymakers and opinion-formers.

What does this mean in practical terms? At one level, as Cohen (2008) and Young (2020) have argued, it means that bridge-building in its most open and generous formulation is to be encouraged. IPE is strengthened to the extent that we develop the conversations which are such an organic part of disciplinary history and organization. But it also means we need to teach the next generation of students to populate our networks, and to apply for and receive grants to fund our research. Institutions value teaching and research funding because they bring in revenue, and without revenue institutions wither and eventually retrench or succumb. Most importantly, in other words, it means that we as scholars of modern IPE must become involved in our institutions, to make the case for our subject as an integral component of the academy. We must fight for resources and go out and raise them where possible, to secure places where IPE can be conducted on its own terms precisely because it is self-sustaining from an institutional perspective. This, much more than the battle over ideas and schools and existing biases within our scholarship, will ensure that IPE has a real future, and it is these institution-building efforts which will—more than anything else—determine whether the next generation of students and practitioners will have a robust field of study to inform their worldviews and actions, unlike those who followed Polanyi, Carr, and Mitrany. This is the central lesson of the curious case of the non-emergence of IPE in 1945.

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NOTES

1. Strange's place in the disciplinary history of modern IPE has been well covered. See Lawton, Rosenau, and Verdun (2000); Cohen (2008); and Germain (2016) for overviews.
2. Many of these criticisms are gathered together in Phillips and Weaver (2011). Cohen has responded to his critics on more than one occasion (Cohen 2011, 2019).
3. Michael Cox (in Keynes 1919/2019, 21–9) provides a discussion of the energetic debate ignited by the publication of *Economic Consequences of the Peace*.
4. While this is a contested point, I would argue that the Marxist tradition only became engaged with recognizably IPE-inflected themes since the 1980s, under the influence of the Gramscian turn in IR theory more generally. See, e.g., Cox (1981); van der Pijl (1984); Augelli and Murphy (1988); and Gill (1990).
5. Twentieth-century scholars who have been used in this way include, among others, Fernand Braudel, Herbert Feis, Albert Hirschman, Hyman Minsky, and Eugene Staley.
6. We may note in this respect the establishment in 1922 of two important institutions to help inform debate on international affairs: the Council on Foreign Relations in New York and Chatham House in London. Interestingly, their research and outreach programs kept the subjects of international relations and international economics largely separate from each other (Parmar 2002).
7. Although each of these individuals published continuously throughout their careers, I am particularly interested in their published work in the years leading up to and through the war and immediately following it, including most importantly for Polanyi, *The Great Transformation*; for Carr, *The Conditions of Peace, Nationalism and After*, and *The New Society*; and for Mitrany several pieces published in the 1930s on the international political implications of planning, together with *A Working Peace System*.
8. On Carr's presence in IPE see Germain (2019b), while for Mitrany see Ashworth and Long (1999).
9. Polanyi and IPE were only joined together in the early 1980s on the back of John Ruggie's seminal use of *The Great Transformation*. See Ruggie (1982); Dale (2016a); and Germain (2019a).
10. It should be further noted that both Carr and Mitrany worked for the British Foreign Office during the First World War, and they were involved in competing bids to provide wartime research to the Foreign Office during the Second World War. The bid with which Mitrany was connected was ultimately successful, and he spent two years in Oxford working for the Foreign Research and Press Service, responsible for research related to southeastern Europe.
11. *Mitrany Papers* Box 64: International Activities; File "International Activities—A–L," letter to Michael Polanyi dated April 18, 1957.
12. *Mitrany Papers* Box 24: Planning; File "Marx Against the Peasant," "Notes on incidents while working on Marx Against the Peasant."
13. See Anderson (1998) for an overview of Mitrany's career.
14. However, he had considered the domestic and international consequences of planning earlier in in the 1930s (Mitrany 1932, 1933).
15. For discussion of open economy politics (OEP) as an approach to IPE, see Cohen (2008); Lake (2009); and Oatley (2011).
16. Privileging the structural characteristics or features of the global political economy is not the exclusive preserve of the Marxist tradition in IPE, although it can certainly be found

- there. Rather, it belongs more generally to those strands of IPE which are somehow structural and holistic in their orientation, whichever tradition they may embrace. And of course, there are many scholars who move rather fluidly across both these frontiers, such as Susan Strange (1996) and Robert Cox (1987).
17. Carr and Mitrany also agreed that this common, public, international economic infrastructure would come from some combination of American and Soviet leadership, due to the outsized impact on global political and economic arrangements they were bound to produce. In this sense they are not total OEP scholars insofar as they would insist that the structure of the global political economy is disproportionately influenced by only a handful of states, namely those which are also Great Powers.
 18. Polanyi put this point in the following way: "Far as mankind is from adapting itself to the use of machines (a key effect of the Industrial Revolution, ed), and great as the pending changes are, the restoration of the past is as impossible as the transferring of our troubles to another planet" (Polanyi, 1944/1957, 250–1). Mitrany sees it thus: "Hence the argument that opposes democracy to totalitarianism does not call the real issue. It is much too simple. Society is everywhere in transition. Its problem after a century of laissez faire philosophy is to sift anew, in the light of new economic possibilities and of new social aspirations, what is private from what has to be public, and in the latter sphere, what is local and national from what is wider" (Mitrany 1943/1966, 99).
 19. Polanyi used Carr's work in situating his use of the ideas of nationalism and the balance of power in *The Great Transformation* (Polanyi 1944/1957, 204 and 263; Dale 2016a, 406), while Carr and Mitrany not only critically reviewed each other's work, they also on occasion utilized each other's expertise. In the early stages of his work on the history of Soviet Russia, e.g., Carr corresponded with Mitrany to enquire about a part of the Comintern's activities on which he was not well-informed. *Mitrany Papers* Box 59: Books, Publishers, Bellagio Conference; File "Books, Articles, Reviews, Criticisms": letter Carr to Mitrany dated March 31, 1952.
 20. There were many reasons for this rather antagonistic relationship, including their critical reviews of each other's work and the very different role each saw that agriculture (and the peasantry more broadly) would play in the future of newly independent states. See, e.g., Mitrany's complaint about Carr's review of his book *Marx Against the Peasant*. *Mitrany Papers* Box 24: Planning; File "Marx Against the Peasant," letters to Margaret Lambert dated December 8, 1951, December 20, 1951, and February 5, 1952.

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CHAPTER 3

THE WELCOME RETURN OF POLITICAL ECONOMY TO INTERNATIONAL POLITICAL ECONOMY SCHOLARSHIP

States, Markets, and Governance

GEOFFREY R. D. UNDERHILL

THIS chapter begins with the observations of the editors in their Introduction: that International Political Economy (IPE) as a field has drifted into a state of constructive, problem-solving non-engagement that is more positive than otherwise. The assessment here is cast more broadly than in the Introduction, and also focuses on what is new. To the standoff between “critical theory” and the rest,¹ one should add other approaches that have proven deliberately resistant to cross-fertilization, such as Open Economy Politics (OPE), the residue of Marxism and dependency approaches, or other “grand theories” based on broad and unverifiable, system-level generalities. The methodological and other divisions observed in the “Cohen debate”² have proven unstable, and a healthy, cross-fertilization dynamic has taken its place, albeit with lingering exceptions. This chapter will advance three interrelated arguments in defense of this position.³

Firstly, the key developments since Cohen wisely stirred things up are threefold: (1) a long-fused end to the methodological and “conflicting approaches” conflicts of the 1990s and earlier 2000s wherein methodological innovation is now part of a renewed, if still fragmented openness; (2) enhanced data availability; and (3) evolving research technologies, frequently imported from other disciplines, permitting new forms of enquiry. The discipline has expanded as well, numerically and in terms of intellectual terrain. Many new scholars with reduced historical baggage and better methodological training have entered the field. Many of these have training and/or professional experience on both sides of the Atlantic or in Asia. While subfield, PhD background, and empirical specialization naturally remain and produce differences, there has been a pragmatic abandonment of the IPE-Comparative Political Economy (CPE) “levels of analysis” distinction and a variegated commitment to understanding levels-interlinkages in terms of national, regional, and historical context. The

understanding of governance, public and private, has long since superseded any earlier excessive focus on what states and formal institutions of government do. Pragmatic, plural, and productive: the pragmatism of the problem-solvers is having a highly integrative effect on IPE scholarship, with lots of critical argument in between. While this is indeed an optimistic take with which some colleagues may disagree, the point is that the enemy is not diversity or fragmentation, but the practice of exclusionary club-formation and careerist, intellectual rent-seeking.

Secondly, this means that those approaches to the field that subsist on boundary and gatekeeping exercises, for example the OEP or “critical theory” schools, have become over time more isolated from a range of important insights accepted as part of the IPE toolkit by the ever-broadening (if diverse) mainstream of the field. The economist-imitator OEP leadership may be observed as steadily abandoning the clubhouse for more open ground (Lake 2009, 2010, 2021). For the “critical theory” school, economics and much mainstream IPE remains both monolithic and largely a negative influence, whereas crucial insights from critical theory continue to be absorbed by other scholars, arguably placing at risk the critical enterprise itself (Jahn 2021). “Mainstream” IPE is addressing the normative concerns of the “critical” school efficiently with well-grounded and innovative empirical evidence and has returned to its classical political economy origins as *the* integrated “science of society,” and has “swamped” OEP approaches in the process. For the problem-solvers, the level of economic expertise has likewise vastly improved. To varying degrees depending on who and where, the problem-solvers are eagerly interacting with and innovatively adopting insights from specialists in economics, political science, political theory and philosophy, history, socio-economics and other social sciences, and, increasingly, the natural sciences. Solving problems through this pragmatic and plural attitude to scholarship is likely to take us far. All of these threads share an ancestry that pre-dates the calcified if ultimately unstable divisions observed by Cohen.

Thirdly, while recent bibliometric researches (Seabrooke and Young 2017; Clift, Kristensen, and Rosamond 2022) focus on boundary and intellectual community detection exercises, we should adopt a substantive definition when we seek to define (International) Political Economy or (I)PE as a field of enquiry. Arguably the core issue remains the intersection of states and markets, from which stems a range of specialized enquiries. Given the classical heritage and wealth of evidence, one should put this with greater nuance: a focus on the interface between what people do as they go about their material life, and the many collective forms of governance and organization, private or public, formal or informal, local or cross-border, that emerge as a result (Ostrom 1990; Leeson 2007; Skarbek 2020).

There are historical reasons for this focus (Underhill 2000), but given contemporary developments it remains relevant simply because of the sheer amount of empirically-observable variation in forms of market and governance, the problems we face, and the related cross-disciplinary intellectual borrowing that goes on around the study of these phenomena. If the field depends on how it is defined by its own specialist practitioners, important as it is to understand these emerging patterns, this likely yields a less-than-helpful definition that focuses on boundaries and gatekeeping. More helpful is to observe how, despite a welcome diversity, this substantive definition unites rather than divides scholarly effort across the “natural” boundaries of specialization. As long as the problem-solvers engage in an open, dynamic, and plural version of “niche proliferation” (Seabrooke and Young 2017; Young 2021), the disagreements need not fix sterile boundaries. A field that prides itself

on interdisciplinarity and openness loses these characteristics as soon as boundaries are set. Neither Smith nor Ricardo would have agreed to that.

These arguments can be tied together by pointing out that it is through specific attention to real-world problems that theoretical and methodological progress has been made in our field. Phenomena that have always intrigued or eluded satisfactory explanation push us to look at what other disciplines are doing. This applies directly to work in political economy that explicitly explores the governance-market relationship in some way or other, which is most of it. This concern for concrete issues that plague our societies has led to much of the methodological and interdisciplinary engagement of individual and groups of (I) PE scholars, and to much innovation when it comes to new sources and uses of data. The pragmatism associated with problem-solving thus has a great deal to do with an iterative bridging of the gap between (I)PE approaches and other fields of enquiry, simply because the problems we identify involve normative and ideational big-question foundations that no group of scholars can ignore for too long. All of this requires the constant fuel of a strong dose of critical thinking that consists of being most skeptical of ideas and arguments that fall in our personal comfort zones, and being most open to those ideas that make us most uncomfortable.

The remainder of this chapter starts by briefly examining the residue of the Cohen debate, an important punctuation point in the emergence of the contemporary field of (I)PE. The second section examines how real-world events and generational change helped to “bridge the gap” between the divided “non-engagement” fragments of a diverse field of enquiry. “Big Questions” of states and markets simply refused to abandon the stage. A growing acceptance of the complementarity of quantitative and qualitative methods, as well as the increasing ubiquity and sophistication of the former, revealed how problem-solving could explore them in new ways that complemented some and undermined other more macro approaches. Problem-solving could be, in critical and pragmatic hands, an inherently integrative form of enquiry. Problem-centered enquiry into the state-market relationship can and indeed has produced important theoretical and empirical research agendas that take us away from boundaries and toward the open skies, while simultaneously linking the contemporary field to its deep roots in historical scholarship and philosophical enquiry. The fit is imperfect: the incentive to distinguish one’s “ism” has not disappeared, some research remains necessarily specialized and focused, but these developments arguably return (I)PE to the substantive state-market-society agenda and leave scope for theory too, as long as this pluralism remains “engaged” (Young 2021). This depends on individual scholars; those with strong commitments to particular approaches, methods, or ontologies are less likely to engage than the more pragmatic. The chapter concludes with a forward look at what is new as a result of these changes.

THE COHEN DEBATE: PUNCTUATION AND TURNING POINT

The most fundamental schism in political economy over time was the emergence during the nineteenth century “marginal revolution” of neoclassical and formal economics as separate

from the broader social and historical concerns of the classical political economists and their successors. Nevertheless, “minority” economists kept (international) political economy alive through “trespassers” from development economics such as Albert Hirschmann (1970, 1981), Raul Prebisch (1950), or Amartya Sen (1991), and also by the interwar emergence of institutional economics (Commons 1924, 1932, 1950; Coase 1937) followed by its more recent revival (Williamson 1975, 1985; North 1990, 2005; Acemoglu, Johnson and Robinson 2005; Greif 2006; North Wallis and Weingast 2009). The public choice scholars (Buchanan and Tullock 1962; Buchanan 1987)⁴ and the vast range of work by Barry Eichengreen⁵ could be supplemented by the scholarship that emerged from business and strategic management as a field where the political, institutional, and private influence elements could not be ignored (Vernon 1966, 1971; Dicken 1986;⁶ Stopford and Strange 1991; Dunning 1988; Baron 1995, 2001, 2014). This highly incomplete tour testifies to a far more plural set of theoretical and methodological toolkits than in “standard” economics, and the innovation has continued with, among others, behavioral (Smith 1991) and experimental (Kahneman 2011) economics.

The punch line is that (I)PE scholars have borrowed extensively both from “standard” and these more eclectic contributions from economists, while the reverse has almost never happened (Jones 2020, 54). Although Economics is too often presented as both rival and enemy to (I)PE and anathema to some, the field continues to absorb insights from Economics pragmatically to its benefit.

This hybrid richness of (I)PE, cross-fertilized by contributions from dissenting economists, is well illustrated by two surveys in *International Affairs* (Underhill 2000; Dickins 2006). Like Cohen’s book, these are accounts of the postwar “revival” (Underhill 2000, 794) of the older, broader field of study rooted in the classical tradition of the Enlightenment that subsequently fragmented into the specialized social science approaches we know today: (economic, political, other varieties of) sociology, (neoclassical and Keynesian) economics, political science and several subfields like international relations, law, human geography, and so on. Both pieces emphasized the breadth and common historical origins of the contemporary field, yet also the conflictual state of affairs in terms of methodological, normative, and theoretical differences. These divisions were represented by the competing but overlapping insights of Dickins’ species *Ratiosaurus Rex* (interstate competition and cooperation tradition in IPE, stemming from International Relations (IR) and the OEP school) versus genus *Querimonia* (states-markets interaction tradition). Each article made a case for synthesis based on less querulous interpretations of the field and its possibilities—a synthesis that this chapter argues is now taking place in a decentralized way.

If these articles and a range of textbooks exposed the divisions, Jerry Cohen (2008) sounded an alarm. Cohen (2007, 2008, 3–5, Chs. 1–2), employing what he admitted was a somewhat risky simplification of observable diversity, organized these divisions analytically into the “US School” (professional, empirically systematic and rigorous, yet arguably conventional and increasingly narrow “small bite” focused) versus the “British School” (socially critical and interpretive, historical-institutional, still asking big questions, yet arguably less than systematic analysis). Each had a different “take” on our understanding of the state-market relationship, and Cohen in turn sparked a debate encapsulated in two special issues of key journals (RIPE 2009; NPE 2009). The constructive mischief Cohen intended was successfully stirred up and this trouble became a turning point in our understanding of states and markets as well. While some argued that Cohen’s typology was too crude or just wrong, many on either “side” argued that, simplification aside, it was all too true and the

divide augured badly for the future of (I)PE and other social sciences more generally (e.g., Mearsheimer and Walt 2013)—after all, Cohen had reserved his harshest criticism for the narrowness of the US School.⁷

Methodology and the use of data were key points that sustained the divide: the “economics imitationists” of the OEP/US School insisted on the primacy of powerful new quantitative methodologies and the exploitation of related data that was being rapidly developed, running the risk that data availability drove the research agenda. UK-school acolytes, sometimes doggedly, defended the primacy of qualitative methods grounded in traditional historical and normative social science and contextual expertise, and some (see Cameron and Palan 2009) seemed inclined to dispense with the need for empirical verification and data altogether. Cohen’s book exposed and lamented the division between the two schools, looking back on an earlier age of plural “invisible college” transatlantic engagement now marred by the splendid yet regrettable isolation of each.

BRIDGING THE GAP

Cohen and the two special issues drew attention to and elaborated on the sources, motivations, and negative results of the divide. A particular lament was the disappearance of the once-fruitful cross-fertilization of the (I)PE postwar revival and its focus on the big-question nature of states and markets. The field had moved from an attitude of critical enquiry, constructive difference, and engagement to a world of club-goods scholarship wherein “approaches” also defined what scholars must do to remain credible and legitimate in the eyes of “selectorate” peers, and this infected journal editing and reviewing as well.⁸ To stylize the point at the risk of injustice, OEP laid out specific elements of ontology and methodology required for graduate training and publication in top journals, while senior scholar reviewers actively defended the turf; in turn, critical theory approaches were no longer about an attitude toward understanding, one of questioning established ways of thinking, but arguably likewise became a set of things one must do and cite in order to remain credible as a member of the club. Scholars retreated to their respective self-made ghettos.

Bridging the gap seemed a tall order unlikely to materialize with despatch, given the way in which careers and peer review might depend on the choice of one school over the other.⁹ However, the divide proved, as predicted (Underhill 2009), unstable. The ubiquity of computer technologies; online-available numerical, textual, and survey data exploitation; and the ultimate mutual compatibility of qualitative and quantitative methodologies, the very elements that seemed so divisive, in time turned out to be the source of an eventual, decentralized integration across the field to the current dynamic status quo. Many senior scholars in the US took Cohen seriously (Lake 2010; Mearsheimer and Walt 2013).

Real-world events also intervened. The financial crash of 2007–2008 and subsequent Arab Spring put big state-market social and political economy issues squarely at the center of the agenda on both sides: inequalities; the balance between the open economy and social protection; the state-market and private-public authority balance in (international) economic governance; the relationship between development, authoritarianism, and democracy. This was fertile ground for the problem-solvers but was also the terrain of those posing big questions. The monumental work of Thomas Piketty (2014) on the long-run dynamics of

inequality; Robert Gordon's reignition of the debate around growth, productivity, and innovation (2016); or Thomas Philippon on market competition (2019), each demonstrated that economists, too, were back in the big question, (I)PE economy business. Political economists were not to be outdone: Ansell and Samuels (2014), draw on enlightenment philosophy and data to rethink the historical relationships between development, elite competition, rising inequality, and democratization. Carles Boix in *Democracy and Redistribution* (2003) employed new data to theorize political transitions in relation to the nature and distribution of economic assets and the balance of power among competing social groups. The same author later (2015) challenges institutional economists, offering novel data sets and a "productionist" argument focusing on military technologies and competition.

There were enough individuals and informal networks across the divides who had an interest in overcoming divisions whenever the need arose, and who managed to work around the obstacles to publication.¹⁰ Indeed, there were already examples of "bridging the gap," including my own cross-disciplinary work with economists. Claessens, Underhill and Zhang (2008) argued through qualitative process-tracing that the Basel II global supervisory standards of the Basle Committee on Banking Supervision, though intended to apply to a wide range of financial institutions in developed and developing economies, advanced the interests of powerful market players with less regard for smaller, less sophisticated banks, especially in developing and emerging-market economies. The article went on to establish through quantitative methodologies the extent to which this was indeed the case: borrowers in poor countries faced higher supervisory charges and thus costs as their economies sought to catch up. The cost imposed was considerable when banks used the data of external ratings agencies to measure risk, yet less when banks used the more sensitive "internal ratings" approach to assessing the risks of lending to clients in developing economies. Banks had finer and better knowledge of their client risks than did the ratings agencies. The point is simple: these findings would not have been possible without the simultaneous use of each of the "rival" methodologies coveted by so many who reinforced the US-UK school divide. One might add that engagement with economists was far from the toxic danger of which so many in more critical (I)PE warned.

Others aided and abetted this nascent cross-fertilization. Burgoon (2001, 2009, 2012, 2013; Burgoon, Oliver, and Trubowitz 2017) pursued a systematic exploration of the relationships between policies of economic openness, national-level labor market and welfare-state regimes, and sociopolitical support for a liberal economic order. Eichengreen and Leblang (2008) in turn engaged in a sophisticated and refreshing use of data and quantitative techniques that demonstrated the mutually constitutive relationship over time between the multiple dimensions of global market integration, "globalization" for short, and the emergence of democracy.¹¹ The institutional economists North, Wallis, and Weingast (2009, 140ff) provided a more traditional discursive argument concerning the "new logic of collective action" as to how and why democracy might generate political economy dynamics that reinforced the openness of economies. These responses to big questions employed a combination of methodologies, and scholars were leaving little out.

Perhaps most importantly, bastions of UK-school traditionalism with strong and historically grounded allergies to quantitative approaches, such as IR and politics at Oxford, were over time totally transformed in terms of methodological foundations, PhD training, and faculty hiring. The quality and methodological breadth of a number of prominent PhD programs in the UK and Europe both improved in quality and embraced plural

methodologies, frequently more easily than US institutions. Postgraduate programs on UK-school territory and in neighboring Europe now formed scholars more systematically. What emerged were centers of hybrid and heterodox scholarship such as the Copenhagen Business School, the Graduate Institute in Geneva, the Amsterdam Institute for Social Science, the EUI in Florence, or SAIS Washington and Bologna wherein scholars with particular attributes could easily cross-fertilize and collaborate. There was much transatlantic trade in research teams and academic personnel, with a good deal of the bridging of the gap coming from the UK and Europe. Mixed and combined methodologies increasingly became a mainstay of postgraduate curricula and PhD programs and faculty hiring. While divisions remained stark in some US universities, methods wars eventually simmered to a truce at worst and collaboration and cross-fertilization began to take hold. This was fruitful ground for the innovative use of new forms of data and modeling technologies, for problem-solving, and for answering big questions.

In short, what had been most divisive, a methodology-centered conflict between two splendid isolations, evolved into a matter of pragmatic, problem-solving choice based on research question, data availability, and what we already knew and still had yet to find out. Cohen's book appeared at the right moment for IPE and helped to catalyze what was already, ever so gingerly, taking place. The intersection of states and markets remained a central, arguably *the* central, preoccupation of (I)PE.

These anecdotal claims above are insufficient to support the argument. Articles using the TRIP data referred to in the Introduction to this volume and more bespoke datasets (Seabrooke and Young 2017; Clift et al. 2022) have examined the field as a whole, or at least a large part of it, and reached similar conclusions. Yet how does this work, in research practice on states and markets? A more focused examination of several articles that represent the trend I argue has emerged will provide further support. The argument is that pluralistic methodological developments have permitted a systematic exploration of state-market, or market-governance interactions, and the embeddedness of both in society across local, national, and transnational levels of institutions. Given the enormous growth in the volume of publications since Cohen's great divide emerged, the choice of articles may appear arbitrary, yet I rely on material assembled for postgraduate teaching across the master and doctoral levels during the past 15 or so years, often developed conjointly with colleagues. The argument in favor of such an approach is that the choices were made from a very wide range of available literature, and that they were far from arbitrary, designed as they were to make available to students the central arguments of this article about the evolution of (I)PE over time.

Methodology Unites

Let us begin with methodology in order to demonstrate how the debate has moved from an emphasis on the superiority of one to the complementarity of quantitative, qualitative, and a range of new forms of computer-based modeling and simulation techniques. The debate on methodology and the divisions Cohen observed ran across the social sciences, and so the examples are not limited to specific (I)PE research. Two articles in dialogue with each other illustrate the point. Evan Liebermann (2005)¹² elaborated the concept of "nested analysis" in mixed-methods strategies. His argument, not per se unique but for the first time

systematically developed for application, was that Large-N, quantitative data-based analyses and case-based Small-N case study analyses were synergistic in a particular way. Large-N studies would reveal the broader patterns of statistical relationships; these relationships should then guide case selection wherein Small-N studies explore and test the plausibility of the relationships the data revealed. Case studies would also allow scholars to develop further theoretical insights through, for example, outlier or “most different” cases. A strategy integrating the two approaches makes up for the weaknesses of each: firstly, Large-N studies might have “insufficient data to assess statistical relationships” or “the nature of causal order could not be confidently inferred” (Liebermann 2005, 440). Secondly, “Because causal inference in the nested approach does not rely solely on the Small-N portion, the standard pitfalls of selection bias are less likely to lead to faulty inferences” (446).

Rolfing¹³ (2008) responded: Not so fast! It is more complicated than that: “the specific methodological problems of nested designs have not been fully appreciated” because “ontological misspecification has methodological implications that are difficult, if not impossible, to detect in a nested analysis in its current form” (1493). Large-N statistical analysis may establish convincing yet misleading causal pathways, and nested analysis may lead to greater losses than gains wherein cross-validation may be more apparent than real. Rolfing strongly supported the mixed-methods approach and went on to suggest alterations to Liebermann’s nested analysis framework that, “renders nested analysis even more demanding, given the increasing degree of sophistication of quantitative and qualitative methods” alike, but rendering the combination more valid as well (1511).

The methods debate across the social sciences (if less so in Economics) has, then, for some time been firmly focused less on which of quant or qual is better, but on when each works best and how to combine them most advantageously. Advances in statistics and modeling have been complemented by a burgeoning literature in and beyond (I)PE on case studies and process-tracing qualitative techniques (Bennett and Checkel 2014; Checkel in this volume). Greater rigor and understanding have emerged on both sides of the erstwhile, now less divided, methodological landscape.

Big Questions on States and Markets, Problem-Solving, and (I)PE Research

The analysis now turns to three articles that help us understand how these methodological shifts have opened up problem-solving approaches to big questions in the field of state-market interaction. These were selected because each is arguably in the “problem-solving” basket, they each respond to big questions about the core (I)PE focus on the cross-border state-market-society nexus, and each requires a commitment to methodological pluralism and innovation. The first (Eichengreen and Leblang 2008) is a bravado piece of statistical analysis that opened up new frontiers for our exploration of the relationship between forms of governance and deep-going market integration.

Harvard IPE development specialist Dani Rodrik wrote several articles and books (e.g., 2000) in which he argued persuasively and discursively that deep cross-border market integration had gone too far: “The political trilemma of the world economy is that international economic integration, the nation-state, and mass politics cannot co-exist. We have to pick

two out of three.” In short, if we wish to preserve national democracy, deep market integration has to go.

Yet how do we know if he is right? Well, we can see that people do not always like it, and generations ago Polanyi (1944) warned of the tensions between radically liberal markets and political and social stability. The evidence of market-driven disruption of societies and politics is clear and we can pile it up. Yet to this author the argument seemed unlikely: years later, neither states nor open markets seem to be going anywhere, though market adjustment presented plenty of challenges as usual, the financial crisis among them. Eichengreen and Leblang (2008) tested the proposition in ways that others had not, using a much longer and richer historical dataset that combined both trade and financial integration data and various historical waves of market (dis-)integration from 1815 onward. Their principal finding was that the relationship between globalization and democracy was a positive one, and that the causal pathways over time went in both directions, suggesting positive feedback loops between the two. That said, they openly accepted (shades of Rolfing) that the analysis only demonstrated general patterns and *not* the causal interactions at any particular time and place (319). This raises the possibility that democracy (echoes of Polanyi) shapes and drives market integration in ways that undermine the trilemma argument (see also North, Wallis, and Weingast 2009, 141ff). This opens up a vast terrain for (I)PE state-market problem-solvers to explore the context and cases of time and place, requiring specialist, qualitative understandings as well as statistical methods. Chwioroth and Walter (2019) filled in an important part of this space in the domain of finance. Krapohl, Ocelík, and Walentek (2021) have used Python software coding to re-open and advance the work of Axelrod on evolutionary game theory as a tool of analysis in (I)PE research, in this case looking at the (in-) stability of the liberal trading order.

A second article by Pagliari and Young (2016) was the last of a series in their successful mixed-methods collaboration on the question of private sector influence in financial governance. Many (including this author) had argued on the basis of qualitative case research that policy capture and private preferences had unduly informed global regulatory and supervisory standards in the financial sector, potentially undermining financial stability and producing crisis (Underhill 1995; Baker 2010). The argument was likely sound in relation to the cases examined, but was there something more general going on? However, the financial sector does not always get its way (Young 2012). Is policy capture and private preference a problem in the financial sector more generally? Pagliari and Young addressed this question step by step in their work, concluding in 2016 that interest group pluralism is particularly low across the “interest ecology” of national financial sectors, while mobilized dissent is in turn weak and disjointed. Financial policymaking across country cases is dominated by financial interests (312). Apparently, there *is* something different and worrisome about the financial sector after all. Methodological innovation was central to their findings: their “analysis takes strong cues from existing ‘population ecology’ approaches to the study of interest groups” (311), examining through textual analysis and qualitatively analysing huge numbers of interest group responses to regulatory issues in financial governance, measuring *and* assessing qualitatively “what groups speak up, and what they say” (315). This big question problem seems settled until further notice.

A third article again employs theoretical, methodological, and modeling innovation to open up genuinely new horizons in our understanding of states and markets, this time reviving an old question of the nature US structural power and the validity of arguments

about US decline (Winecoff 2020).¹⁴ Here the long-standing concept of structural power and the persistent myth of lost US hegemony (Strange 1987) is reconfigured as a network phenomenon, linking it methodologically to complex network science. The work replaces the typical “attribute- or capability-based approach” (212) to structural power by adopting the “fitness plus preferential attachment” model (210, 212–17) to demonstrate the ongoing “high centrality” of the US across several (213) the domains of finance, trade, security, and knowledge networks. Complex network science permits an understanding of the evolutionary dynamics of interdependence and the endogenous, feedback-based patterns and “nodes” of power and network performance that we do not observe in traditional data. “Because the system is not just the sum of its parts, it should be studied holistically—not just the units but also the connections between them—and modern network science provides an advanced conceptual, analytical, and methodological toolkit for doing so” (214). This research design likewise permits a far better understanding than traditional methodologies of the interactions in complex interdependence, a key concept in (I)PE that has always been as difficult to operationalize as (structural) power, wherein the inference is based on an observed, historical outcome (216). Complex network science permits a proper interactive modeling of structure, agency, and change in a holistic framework.

The findings are intriguing. “There are reasons to believe that the geopolitical advantages the United States has enjoyed in the post-war era would not have obtained absent network prominence. Will this continue?” (242). Furthermore, as Strange argued so long ago, reports of US decline and the relative rise of China are overstated: “if emerging markets continue to pursue development by integrating into the economic and political networks that the United States maintains . . . then American prominence is likely to be further reinforced by their growth” (243). Winecoff concludes that shifts in the global economy and security structures have had more negative effects on Europe than the US.

This brief survey of recent literature demonstrates that state-market problem-solvers are addressing big questions, and that methodology is no longer the source of division that it once was. The emergence of younger scholars trained across the panoply of methodologies has truncated the standoff. On the contrary, methodology has become a source of synthesis across the comparative-IPE divide and also a source of new insights and ways of seeing. Problem-solvers are delving into and responding to big state-market questions that matter to national and international political economy. Secondly, while there clearly is problem-solving driven fragmentation across the field—after all, the world is a complex place such that specialization and plurality should not be viewed in negative terms—there is also constructive engagement with long-standing theoretical and more problem-oriented debates. The more dogged adherents to intellectual clubs are simply being left behind. Lastly, these articles also show that the relationship between states and markets, or market exchange and governance, or “state-society complexes” (Cox 1986), however one prefers to phrase it—is still very much at the core of the (I)PE intellectual agenda and binds it together. These questions and the answers we find matter to the future of humankind and the planet.

This state-market agenda is also unlikely to dissipate any time soon, simply because it is at the core of the human dilemma. This admits of a simple, bottom-up, and agent-centric explanation that provides a good metaphor for the developments I have argued took place. As Buchanan and Tullock argued some time ago (1962), we cannot accomplish much of what is dear to ourselves, our most self-interested preferences, without engaging others. This “dilemma of interdependence” soon constrains and condemns us to resolve (or not!)

the tensions we experience with those around us, producing formal or informal norms and “rules of the game.” In short, this dilemma of mutual dependence generates patterns of coordination and collective action. We call this “organization,” and, at scale, institutions and governance, such that political economy dynamics lie at the very roots of society. Violence remains one solution (as we are reminded persistently), but it is not very efficient. If we opt for a more appealing solution, we discover that one cannot have organization without resources and a division of labor, and one cannot have a division of labor and deployment of resources without organization. We continue to differ amongst ourselves about how this should be done, and about the distribution of the costs and benefits of the collective choices we make. Global integration has rendered this increasingly complex and variegated in terms of institutional form and patterns of interaction. Some scholars focus more on the “international” (or “global” if one prefers), others on the more national or local, others on specific areas of production, exchange, or finance, but the levels of analysis distinction so dear to IR remains an irrelevance in (I)PE.

CONCLUSION: SO WHAT IS NEW?

The conclusion to the previous section implies that this long-standing, core state-market agenda is always an open field. Human interaction in the face of distributional and normative tensions is seldom static for long. A good deal of innovative recent work has already been cited in this article, yet there is plenty more. A changing world as it confronts our human curiosity is one source of “newness”—an issue-driven agenda. New methodologies and modeling techniques also means that we have new ways to deal with state-market-integration complexity: new theoretical horizons that may provide a better explanation and understanding of change.

There is far too much research being produced in this vein to attempt an exhaustive or even representative account.¹⁵ Let me therefore focus on examples of work that combines methodological and theoretical innovation with an eye on better understanding the state-market problems and dynamics that we face as we approach the mid-twenty-first century. First up is a new focus on interdependence that takes issue with the OEP approach, arguably launched by Farrell and Newman as the “new politics of interdependence” (2014, 2019). Thomas Oatley (2019) has introduced complexity theories, ecology, and information theory to the concept, and this complements the evolutionary biology approach cited above (Krapohl et al. 2021). As the work by Pagliari and Young discussed above implies, global interdependence and the hydra of global finance continues to keep state-market scholars busy: Heather Ba (2021); Oatley et al. (2013), Winecoff (2015), Chwioroth and Walter (2019). There has been a revival of work on corporate elites with new methodological flair (Valeeva, Heemskerk, and Takes 2020; Valeeva, Takes, and Heemskerk 2022; Fichtner et al. in this volume) and on labor markets and workers’ rights (Lim, Moseley, and Prakash 2015; Moseley 2017; Moseley and Malesky 2018). One should not forget recent work on rising powers: see Chen (2021) on the implications of China’s Belt and Road initiative, and Otero-Iglesias and Weissenegger (2019) on China’s foreign investment (and many others). One must not forget climate change and the Anthropocene (e.g., Bernstein 2020). I have not yet said anything about the rise of populism against market integration, or the political economy of pandemics!

Lastly, what is waiting to be born? The dilemma as always is that specialization leads some scholars to focus on the “big picture” macro or systemic dimension of the (I)PE of states and markets. As I argued above, this picture needs to tally with a well-fashioned aggregation story. In parallel, the demands of complexity mean that many scholars rightly focus on specific “bite-sized” issues and analytical frameworks in our contemporary (I)PE, attempting to identify cause and effect relationships between the multiple facets of what we observe around us. Both may be problem-solving, asking big questions, and methodologically challenging. Yet more difficult is ensuring that scholars pay sufficient attention to how these two different sorts of enquiries fit together. These two ways of seeing research lay behind Cohen’s great divide. This article has argued that they need not do so, and that there is much more integration and engagement than before.

Yet “big picture” and specialized, issue-focused scholars too often do not mix and may still claim the superiority of one over the other. This article is an invocation for scholars to make the effort to understand each other across this ever-potential divide: to understand why they should fit together. We cannot understand our world and its problems without focused data and specialized methods, and the nitty-gritty of research frequently involves narrow hypothesis-testing. Yet issue-focused scholars can and *should* link their research agendas to big picture approaches.¹⁶ Put polemically, stacking up the findings and calling that coherence is insufficient. Likewise, big picture questions cannot be answered through vague, macro generalities invoking “the nature of the system.” If the claim is that free-market capitalism (or state intervention) is bad (or good), one must demonstrate systematically the various negative/positive effects that different variants of markets and governance produce, *and* why they are good/bad. This necessarily involves a normative dimension or at least starting point, and once again the aggregation problem is daunting. The issue is one of moving from micro- to macro-level, and back again, while dealing with a high degree of heterogeneity and endogenous dynamic relationships: the “everything is related to everything else” problem. Too frequently, the two approaches remain separate because linking them is hugely challenging. The ongoing tensions between micro- and macro-economists illustrate the point.

What might help? Mathematicians will tell us any real-world phenomenon may be modeled. Simulation or game theory and agent-based modeling techniques can reveal bottom-up complex adaptive systems as one solution, and have been explored in political science and economics.¹⁷ This is highly promising if still a niche pursuit; as with all modeling, much still depends on choices of simplifying assumptions, agent motivation, and the design of feedback loops. There is another possibility. As the core of the field, the (I)PE of states and markets, with its constant and dynamic variability producing enormous complexity in terms of interactive relations among heterogeneous agents and an almost endless multiplicity of possible variables, is arguably appropriate to quantum approaches taken from particle physics. The field is full of “quantum uncertainty” and open-ended outcome possibilities, and thinking of ourselves as “objects” has long been recognized as unhelpful.

Alexander Wendt has made serious attempts to do this in a more general IR and social sciences context (2015, 2018), and there was a forum on his work in *Journal for the Theory of Social Behaviour*.¹⁸ Another recent IR contribution came from Chengxin Pan (2018, 2020), with reference to the “relational turn” in IR and IPE (e.g., Nordin et al. 2019). The aim of such approaches is to deal with the “whole and its parts” and structure and agency problems, often thought of as chicken-and-egg dilemmas. There is debate about whether the approach should be “scientific realist” as adopted by Wendt, or more by way of analogy (Pan 2020,

20–1; Donald 2018). Taking such an approach seriously would require some re-tooling for most social scientists, whose understanding of the quantum world, like my own, is typically limited. Yet could this help scholars solve the big-picture–bite-sized research problem, system wholeness, the parts, and order referred to above?

A serious attempt to adapt this initiative to state-market (I)PE would be a substantive companion to complex network theory, which also attempts to focus on relations and their dynamics across systems, yielding a better explanation of change over time. The goal would be to move (I)PE from a classical Newtonian, “parts add up to system” approach to a more sophisticated world of interactive quantum theorizing, because political economy, like the broader social world, is always “becoming,” involving the simultaneous interaction of individuals, collectivities, ideas and consciousness, and systemic parameters yet which are, somehow, all part of the same thing.

Challenge is what the field is all about. Someone should take this up.

NOTES

1. A standoff for which there is no inherent logic, and that not all critical theorists embrace (Jahn 2021).
2. See Cohen (2008), and special issues of *Review of International Political Economy* (2009) and *New Political Economy* (2009).
3. The author was, from 2018–2021, the Associate Editor (IPE) and Editor-in-Chief of the *European Journal of International Relations*. This vantage point was particularly useful for observing the development of IPE in relation to other subsets of Political Economy writ large.
4. Wherein political and economic exchange are considered essentially the same: (Buchanan and Tulloch 1962 Ch. 3); also “Politics is a structure of complex exchange among Individuals, a structure within which persons seek to secure collectively their own privately defined objectives that cannot be efficiently secured through simple market exchanges” (Buchanan 1987, 246).
5. Too numerous to do him justice, but see Eichengreen (1992) or (2018) to start with.
6. At least six editions of this work have been published.
7. Maliniak and Tierney (2009), in their article in the *RIPE* (2009) special issue, analyzed data from their journal article data base and from two surveys of IR scholars in the US and Canada. Their findings largely confirmed the trend identified by Cohen.
8. The problem is arguably still very much present in some journals on either side of the divide.
9. For many PhD candidates, there was not much choice at all; junior scholars aiming high frequently had to eschew a personal tendency to eclecticism and deliver what supervisors, and above all editors and the reviewers they selected, expected. There was a cost to ploughing one’s own furrow, while one must learn much in the first place before that can occur.
10. Some journals thankfully remained more open than others. That said, the numerical growth of the field combined with the pressure from universities for impact factor and ratings made publication just plain difficult for all.
11. More on this article later.
12. A comparativist at Yale, Princeton, and then MIT focusing on developing Africa.

13. Professor at Bremen and latterly Cologne whose work is on methods and party competition.
14. Winecoff is by no means alone in embracing complex network approaches to (I)PE; see also Cooley and Nexon (2013); Heemskerk and Takes (2016); Goddard (2018).
15. The selection that follows was made largely on the basis of recent issues of the *European Journal of International Relations*, a highly-rated generalist IR journal with about 25% (I) PE content. There is no pretence that this is a proper representative sample, with apologies to rival editors, but it serves the purpose here. Together the collection of articles in this volume will offer more comprehensive coverage of new developments.
16. Of course, many do indeed do so.
17. See the highly accessible discussions by Benoit, also Cederman in (2001) *The Political Methodologist*; in Economics, a recent example is Gerdes et al. (2022).
18. Vol. 48 Issue 2 (June 2018); this includes some robust pushback from the world of physics (Donald 2018).

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CHAPTER 4

FOREIGN ECONOMIC POLICY

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As the title indicates, this chapter provides an overview of foreign economic policy. From the outset, the reader should understand that this essay will not (and indeed cannot due to space constraints) provide a comprehensive literature review on this subject. Instead, it has three other sequential objectives. First, it begins by defining what is (and is not) foreign economic policy as it relates to the discipline of Political Science. This exercise is useful not only in terms of setting some reasonable limits for this chapter, but it also helps demonstrate how this discipline's research on foreign economic policy is problem-centered and what problem is its focus: explaining the variation in foreign economic policy. Second, it will review (briefly) two major research programs related to this problem: the state-level variation based on political regime type and the individual-level preference variation for different foreign economic policies. Third, this essay will highlight the obstacle to connecting these two research programs: how to explain outcomes at the state level based on preferences at the individual level.

DEFINING FOREIGN ECONOMIC POLICY

This author and many (if not most) readers of this edited volume work within the Political Science field known as International Relations (IR). As a field, IR can be further decomposed into four subfields: International Political Economy (IPE), International Conflict/Security,¹ International Organization, and Foreign Policy. The first of these subfields (IPE), which is the broad topical focus of this Oxford Handbook, deals with the politics related to the various flows associated with economic globalization (e.g., international trade, international capital mobility, and international labor mobility, or immigration). International Conflict/Security represents another IR subfield defined *topically* with its focus on war, peace, and how countries try to make themselves more secure in the face of external threats. As topical IR subfields, IPE and International Conflict/Security are represented as columns in Figure 4.1.²

International Organization and Foreign Policy are not so much topical subfields in that they consider new IR subjects that are not already considered within IPE and

	International Political Economy	International Conflict/Security
International Organization		
Foreign Policy	<i>Foreign Economic Policy</i>	

FIGURE 4.1. Organizing IR by subfield.

International Conflict/Security; indeed, they both cover much the same topics as these first two subfields. Instead, as IR subfields, International Organization and Foreign Policy arguably represent different directions, or starting points, for understanding what we can observe in IR (e.g., trade, capital mobility, military conflict, and nuclear proliferation). The subfield of International Organization studies IR from the top down, or starting at the international level, often focusing on the structures (e.g., intergovernmental organizations) that regulate or moderate the behavior of state actors (or their absence and how they fail to do so). Conversely, Foreign Policy studies IR from the bottom up, or starting at the domestic level (and even sometimes at the individual level), focusing on how a state's external policies are constructed. As such, these two subfields are represented as rows in Figure 4.1.

This construction, or organization, of IR by subfields is a useful exercise for at least two different reasons. First, and most importantly for what follows in this chapter, it defines the domain of foreign economic policy as the conjunction of IPE topics and the foreign policy focus on how external policies are determined within the state. Foreign economic policy thus studies how different states formulate their trade, capital, exchange rate, and immigration policies (as IPE issue-areas) and how they are consistent with the preferences of some actors (i.e., the “winners”) but not others (i.e., the “losers”). While topically IPE, studies about the effectiveness of the World Trade Organization (WTO), the design of preferential trading arrangements, or the impact of being under International Monetary Fund (IMF)/World Bank structural adjustment programs, for example, fall within the upper left cell of Figure 4.1 and thus will not be considered within this chapter devoted to foreign economic policy.

Second, isolating what falls within the lower left cell of Figure 4.1 is valuable because it helps identify how the academic research on foreign economic policy is problem-driven and what are the problems that we are trying to solve. In their Introduction, Pevehouse and Seabrooke define problem-driven research as that which begins with an outcome of interest in the world. For scholars of foreign economic policy, the primary outcome of interest is the variation in trade, external investment, capital, exchange rate, and immigration policies at the state level since these policies are set by national governments. Although there may be general systemic trends (e.g., increased trade with reduced tariffs post-Second World War and the abandonment of intermediate exchange rate regimes in the post-Bretton Woods era),³ there remain important differences among states relative to these trends in how open or closed (flexible or fixed) are their trade, investment, capital, exchange rate, and immigration policies. As also described by Pevehouse and Seabrooke, the goal of problem-driven research is to better explain these outcomes of interest, or to identify what accounts for this variation in foreign economic policy.⁴ For example, what types of states have more

open trade policies *and why*? What types of states have more flexible exchange rate regimes *and why*?⁵

As one can quickly observe in these two stylized questions, each includes two separate queries. First, what factors are correlated with this observed variation in terms of foreign economic policy? As will be discussed below with a focus on political regime type, the research on foreign economic policy has identified a number of important relationships. But second, if we can observe, for example, that more democratic governments tend to have more open trade policies and more flexible exchange rate regimes, then why should this be true? As I will also discuss below, this is where the research on foreign economic policy still needs to make progress.

THE VARIATION IN FOREIGN ECONOMIC POLICY

Since this chapter is not intended to be a comprehensive literature review of all the observed relationships in foreign economic policy, this section will instead focus on two major research programs that fit within the lower left cell of Figure 4.1. The first research program concerns the variation in states' foreign economic policies based on their political regime type. The second is devoted to explaining the variation in individual-level preferences for different foreign economic policies. Not only are both prominent in the domain of foreign economic policy, but these research programs also account for variation at different levels of analysis. Furthermore, as will be explained below, they are linked research programs in terms of the stylized queries advanced above. The first identifies the source of the policy variation (i.e., the country's political regime), while the second attempts to answer the "why" question: the foreign economic policies of democracies *may* differ from autocracies because citizens can express their policy preferences through voting in the former.

Turning to the first research program, it is important to consider how it may be fundamental for IPE as a major IR subfield in that it links a basic political concept (regime type) to the central phenomenon of economic globalization. Going even further, if such a broad political difference across countries cannot help to explain the variation in foreign economic policy, then narrower political differences at best offer limited explanatory power. For example, more restricted differences, such as electoral system type or the partisan orientation of the elected government, can only explain the variation among the smaller set of more democratic governments.⁶ And if there is no systematic difference between democracies and autocracies in terms of their foreign economic policy, then the variation within each regime type may simply be idiosyncratic.⁷

Starting with international trade in goods and increasingly services, arguably the most studied global economic flow within IPE, there is a robust set of results showing that more democratic governments tend to hold more open trade policies (e.g., Bliss and Russett 1998; Remmer 1998; Mansfield, Milner, and Rosendorff 2000; Mitra, Thomakos, and Ulubasoglu 2002; Mansfield, Milner, and Rosendorff 2002; Milner and Kubota 2005; Kono 2006; Hollyer and Rosendorff 2012). Although there is less research devoted to foreign direct investment (FDI), which may often be a substitute for international trade, Pandya (2014) demonstrates

that democracies have fewer restrictions related to FDI inflows, while Pond (2018) also provides evidence that democracies have greater restrictions related to FDI outflows.

Switching IPE issue-areas, governments face a trade-off between fixing their exchange rate and retaining domestic monetary autonomy (thus producing flexible, or floating, exchange rates) when capital is internationally mobile. With regards to this scope condition (international capital mobility), Eichengreen and Leblang (2008) and Milner and Mukherjee (2009) show that democracies tend to have more open capital accounts. And given this condition, there is also a robust set of evidence to show that democracies generally operate with more flexible exchange rate regimes (e.g., Leblang 1999; Frieden, Ghezzi, and Stein 2001; Broz 2002; Bearce and Hallerberg 2011; Bearce 2013; Steinberg and Malhotra 2014), consistent with a choice for domestic monetary autonomy under international capital mobility.

Finally, in terms of international labor mobility, Bruenig, Cao, and Luedtke (2012) offer evidence to show that democratic destination countries admit fewer immigrants than non-democratic destination countries (e.g., Saudi Arabia and Singapore). While Peter's (2015) results do not show a consistent result for democracy, her argument that trade openness (which is greater in more democratic regimes) should be associated with more immigration restrictions implies a more closed immigration policy in more democratic regimes. And when Shin (2017) extends Peter's dataset, he finds results consistent with this conclusion: more restrictive immigration policies in more democratic regimes.

As mentioned above, a second major research program investigates citizen preferences over foreign economic policy, notably trade, immigration, and currency policy. As argued by Lake (2009) in his review of open economy politics, interests at this level represent the starting point, or the foundation, for the analysis in the first column of Figure 4.1 (IPE), consistent with the Foreign Policy subfield being organized as the bottom row in this same figure. The various models of individual-level preferences used in this research program are derived from well-established Economics frameworks, most notably Heckscher-Ohlin predicting factoral/skill divisions, Ricardo-Viner positing sectoral/industry differences, and New New Trade Theory expecting preference variation based on firm characteristics (Alt et al. 1996; Bernard et al. 2007).

Starting again with international trade policy, the preference models have tended to be based on factoral considerations (e.g., Rogowski 1989), largely focusing on skill as measured by educational attainment. Survey evidence in support of individual-level preferences in this issue-area being based on skill/education are provided by Scheve and Slaughter (2001a); Mayda and Rodrik (2005); and Rho and Tomz (2017), although recent analysis also suggests that one's trade policy preference may also be determined by occupational tasks (Owen and Johnston 2017), which may be weakly related to skill. But Hainmueller and Hiscox (2006) argue that the education result in surveys of trade policy preferences is more due to educational socialization than to actual labor market skill. Likewise, Mansfield and Mutz (2009) show that American trade policy attitudes are determined as much by sociotropic (i.e., national) concerns as they are by economic self-interest. Finally, Baker (2005) uses a Latin American sample to show that if trade policy preferences are based on economic self-interest, they may more consistent with citizens' interests as consumers (rather than as producers).

There has also been much research seeking to explain the variation in citizen preferences concerning international labor mobility, or immigration. Like for international trade, the analysis in this issue-area of foreign economic policy has tended to use a factoral framework based on skill as measured by educational attainment. Survey evidence provided by Scheve

and Slaughter (2001b); O'Rourke and Sinnott (2006); and Mayda (2006) support the proposition that citizens with less education tend to be less supportive of immigration openness. As done for the parallel result in trade policy, Hainmueller and Hiscox (2007) counter with the argument that this result can be explained by educational socialization as much as by labor market skill, although Bearce and Roosevelt (2019) show that even when controlling for socialized attitudes about immigration, skill as measured by education remains a strong predictor of attitudes in this issue-area. Finally, it is also important to note the evidence for immigration policy preferences being organized along industry/sectoral lines provided by Dancygier and Donnelly (2013).

Compared to the research on trade and immigration policy attitudes, there has been much less research to explain the variation in individual-level preferences directly related to exchange rate policy. But the preference models in this issue-area tend to emphasize sectoral considerations (Frieden 1991), although when Bearce and Tuxhorn (2017) test American currency/monetary policy preferences along different dimensions related to economic self-interest, they find greater support for firm-related factors, consistent with New Trade Theory.

As highlighted earlier, this chapter chose to review (albeit incompletely) these two foreign economic policy research programs—1) the state-level policy variation based on political regime type and 2) the individual-level variation in policy preferences—because they are (or at least should be) linked to each other. Since it may not be immediately obvious to all readers how or why this would be the case, it is important to explain the logic underlying this linkage. Stated simply, one way to account for the differences in terms of foreign economic policies in democracies versus autocracies, as stylized political regimes, is that citizens have greater rights to express their policy preferences *through voting* in the former compared to the latter. This understanding informs the opening sentence in Scheve and Slaughter's (2001a, 267) analysis of trade policy preferences: "A complete political-economy model of trade policy-making must somehow characterize individuals' preferences over trade policy." Likewise, as argued by Rodrik (1995, 1458), a political-economy model "must contain a description of individual preferences over the domain of policy choices available to policymakers."

LINKING PREFERENCES TO OUTCOMES

Given that democratic norms and institutions endeavor to make or constrain policymakers to choose policies that fit majoritarian preferences, the obvious logic is that foreign economic policies are more likely to meet this criterion in more democratic political regimes. So does this logic work to explain the variation in foreign economic policies that we can observe at the state level? Arguably, it does for certain issue-areas, but it performs poorly for others.

To understand where it works and where it does not, let us begin with immigration policy, or foreign policy related to international labor mobility. Among the various features associated with economic globalization, international labor mobility clearly stands as the most restricted (Peters 2015) with labor flows being especially constrained into the more democratic destination countries (Bruenig, Cao, and Luedtke 2012 and Shin 2017), although it should be noted that certain democratic destination countries have begun to open slowly their immigration policies with the end of the Cold War (Bearce and Hart 2017).

This variation at the state level would suggest that citizens have strong preferences against a more open immigration policy and that where they can vote based on that policy preference (i.e., in more democratic regimes), immigration policy should generally become even more restrictive. And the survey evidence seems to strongly accord with this understanding. As Facchini and Mayda (2009, 8) concluded from the cross-national surveys conducted by the International Social Survey Programme (ISSP) in 1995 and 2003, “the existing cross-country evidence on migration preferences suggests that individuals are on average remarkably averse to more open migration policies.” As Rosenblum and Cornelius (2012, 246) similarly described, “if there is a universal truth about immigration policy, it is that residents of industrialized states would prefer to see lower levels of immigration.”

Indeed, this logic concisely describes the dilemma facing democratic states in terms of their immigration policies: the very same domestic institutions that make the political system more open and less exclusive also serve to make the country’s external labor policy less open and more exclusive (Hollifield 1992). Among the issue-areas related to foreign economic policy, immigration arguably stands as the most salient (Mellon 2014; Boomgaarden and Vliegenthart 2007). In fact, for many voters, immigration policy is not simply an issue related to foreign economic policy, it is also and primarily a domestic policy issue given that immigrants (if/when they are admitted) might live near them and potentially compete for jobs against natives.

This understanding that a key dimension of foreign economic policy feels like a domestic policy issue for many citizens also helps to explain why the logic advanced above also helps explain why democracies often tend to have more flexible exchange rate regimes. The reader should recall that the exchange rate regime choice in terms of foreign economic policy has implications for domestic monetary autonomy given international capital mobility. If international labor mobility is the most restricted feature of economic globalization as discussed above, then international capital mobility is arguably the least restricted (Andrews 1994). Thus, governments effectively face a monetary policy trade-off: either fix the national currency (as their exchange rate regime choice) or exercise domestic monetary autonomy (effectively a choice for a flexible exchange rate regime). Achieving the former requires that monetary policy be directed toward keeping the currency’s value stable relative to some external benchmark with governments losing the ability to direct this policy instrument toward a purely domestic economic target (e.g., raising the growth rate, reducing unemployment, or reducing the inflation rate). If governments want to use monetary policy for any of these internal objectives (i.e., assert domestic monetary autonomy), then they must accept the currency variability associated with a flexible exchange rate regime.

So what are citizen preferences concerning this monetary policy trade-off? Compared to immigration policy (just discussed) and trade policy (to be discussed below), there are relatively few surveys that directly ask citizens about their preferences concerning the exchange rate regime. But using a series of American samples, Bearce and Tuxhorn (2017) show a stronger preference for domestic monetary autonomy, consistent with more flexible exchange rates. At this point, it should also be acknowledged that there are surveys about the European common currency, consistent with fixed exchange rates among the Eurozone countries, that will be discussed separately below. If one makes the (perhaps heroic) assumption that citizen preferences in many other non-European countries are similar to

those in the US (i.e., a greater preference for domestic monetary autonomy than for exchange rate fixity), then it is not hard to see why democracies should, on average, have more flexible exchange rate regimes.

To the extent that citizens want their government to address internal economic problems such as growth, unemployment, and/or inflation, and they can transmit this preference for domestic economic management by voting, then we would expect to observe more democratic governments opting for domestic monetary autonomy (over exchange rate stability) with international capital mobility. This choice does not occur because a majority of citizens are voting for more flexible exchange rates. Indeed, it does not even require that they understand what “domestic monetary autonomy” means. It simply requires that democratic policymakers believe that they may lose their office if they do not tend to domestic economic problems and that they desire the use of the monetary policy instrument to achieve these ends.⁸ Indeed, this logic works even if successive democratic governments have different preferences concerning a fuller employment versus lower inflation trade-off per the Phillips curve with right-wing governments wanting lower inflation and left-wing governments preferring fuller employment. Both lower inflation and fuller employment could be achieved through changes in monetary policy and are thus consistent with domestic monetary policy autonomy.⁹

But what about the common currency (consistent with perfectly fixed exchange rates) adopted by many European Union countries beginning in 1999? Does this significant IPE development run contrary to the logic described above: that democratic policymakers will formulate their foreign economic policy to be consistent with majoritarian citizen preferences? Based on the Eurobarometer data assembled by Roth, Jonung, and Nowak-Lehmann (2016), there has been net positive public support for the common currency within the countries that adopted it, both before and after 1999. Correspondingly, this support was lacking among the three countries (the UK, Denmark, and Sweden) that chose to remain outside of the Eurozone, thus retaining their domestic monetary autonomy. Thus, even the variation in European currency policies appears to fit with the logic described above.

But this logic becomes problematic when we shift to trade policy. First, it has been argued that international trade is not a particularly salient issue for most individuals (Guisinger 2009; Eichenberg 2016; Kleinberg and Fordham 2018). As a result, citizens do not understand the costs and benefits of open versus closed markets and do not cast their votes based on this issue even when they do have coherent trade policy preferences. We would thus not expect democratic governments to feel much electoral pressure for a more open market, so why should they have more open trade policies than autocracies (as has been observed)?

Perhaps it could be argued, as done above for exchange rate policy, that even if international trade policy is a non-salient issue for most voters, it is nonetheless directly connected to salient domestic policy outcomes like inflation and unemployment per the economic voting model (Palmer and Whitten 1999). Like many other forms of trade protection, tariffs are a tax on foreign producers, thus raising the price of imported goods. And with a higher price on imports, domestic producers may also be able to raise their prices, while still keeping them below the same charged by foreign producers. Hence, trade protection is inflationary, and the empirical evidence shows a negative relationship between import competition and prices (e.g., Auer and Fischer 2010).

If citizens are prepared to punish democratic policymakers for inflation, then the latter might be expected to lower tariffs preemptively (Bailey 2001), thus resulting in more open trade policies in more democratic regimes. This argument does not require that citizens actually vote for lower tariffs, which allows international trade to be a non-salient issue. Likewise, it does not require citizens to understand the relationship between trade protection and prices in the domestic economy. It only requires citizens to dislike rising prices and democratic policymakers to fear losing office due to inflation consistent with the economic voting model. Indeed, this is precisely the argument offered by Mansfield, Milner, and Rosendorff (2002) for why more democratic regimes are more likely to sign an international trade agreement. In their model, citizens are not voting directly for international trade openness; instead, they apply their electoral pressure *for lower domestic prices*.

But there are also problems with this economic voting model logic when applied to trade policy. First, recent evidence by Betz and Pond (2019) demonstrate that even while democracies have lower average tariff rates, their tariffs also increase for products with greater domestic consumption shares. This result certainly does not accord with the argument about voters applying electoral pressure for lower prices achieved with more open markets. Second, recent survey analysis by Bearce and Moya (2020) shows that American trade attitudes at the individual level are more driven by concerns about the job loss (i.e., the cost of an open market) than about a belief in lower prices (the primary benefit associated with an open market). Correspondingly, they present cross-national ISSP survey evidence to show that the trade policy preferences of the median voter in most countries appear to accord with *more*, not less, import restrictions.¹⁰

Thus, when the political logic advanced above is applied to the issue of international trade, it leads to a conclusion about the expected state-level policy variation that runs contrary to what we actually observe: *democracies should have more trade protection than autocracies*. Perhaps non-democratic governments can afford to keep tariffs low despite the negative job/income effects that many citizens associate with open trade because they do not face punishment through the electoral channel. But voters would punish democratic incumbents for doing the same. Thus, we need some other way to move from our individual-level preference models to the state-level variation in foreign economic policy. The final section will outline a brief research agenda directed toward this end.

FIRM LOBBYING?

If it is sometimes hard to connect these preference models, when they are applied to individuals as potential voters, to the foreign policy variation that we can observe at the state level, especially for international trade policy, then perhaps we need to make two adjustments to our research in this domain. First, the preference models can be applied to *firms* and not simply to individuals (e.g., Kim 2017; Broz, Frieden, and Weymouth 2008). Second, we need to focus more on how preferences might get translated into policy *through lobbying*, requiring us to think more about how special interest politics may operate differently in democracies compared to autocracies.

With regards to the first point, it would certainly be valuable to have more firm-level surveys that inquire not only about corporate lobbying behavior, but also about the policies

for which they are willing to make financial contributions and provide information to the state. Such surveys are, of course, much more expensive than parallel surveys of individuals and far more difficult to make representative, especially across countries. But if the political weight in terms of foreign economic policy is being carried more by corporate actors than by citizens as voters, then more individual-level surveys may provide little additional value to this area of IPE research.

Concerning the second point, while there is firm lobbying in both democracies and autocracies as stylized political regimes, we need to think more about how this special interest activity is conducted differently in various political regimes. For example, it has been hypothesized that more firms are able to lobby in more democratic regimes (Weymouth 2012). Others have argued that as the special interest channel gets wider in more democratic regimes, it is the exporting firms (with an expected preference for more open markets) and service firms (with an expected preference for domestic monetary autonomy) that become able to lobby the state where they were often blocked from such political activity in autocracies (Bearce and Velasco Guachalla 2020). If correct, then this understanding may better allow IPE scholars to use their preference models to help explain the foreign economic policy variation among states, which remains the primary problem that we are trying to solve.

NOTES

1. It is important to recognize that IR scholars often separate the research communities identified as “Conflict” and “Security.” But they are grouped here since this separation is less defined by topic and more defined by a methodological orientation.
2. Certain IR topics such as those related to the environment might seem to be outside of the structure in Figure 4.1. But research related to the economic impact of global environmental problems falls within the IPE column, while research related to environmental security problems can be assigned to the second column.
3. This kind of variation best fits with “system-centered” explanations as discussed by Ikenberry, Lake, and Mastanduno (1988).
4. There is also the related problem of explaining the variation in foreign economic policy *effectiveness* (e.g., what would improve a country’s trade policy outcomes?). But the question of policy effectiveness is more central to the discipline of Public Policy than to Political Science, although it may have implications for certain arguments in the latter discipline. Given this understanding, this chapter will not consider problems related to the variation in foreign economic policy effectiveness.
5. These questions arguably require “society-centered” and “state-centered” explanations, using the framework provided by Ikenberry, Lake, and Mastanduno (1988).
6. This is not meant to imply that the variation among democracies, e.g., is not also a problem that needs to be explained. Rogowski (1987); Nielson (2003); Grossman and Helpman (2005); Ehrlich (2007); Evans (2009); and Rickard (2012) offer explanations for the intra-democracy variation in terms of trade policy. Likewise, see Bernhard and Leblang (1999); Leblang (1999); Frieden (2002); and Bearce (2002) for explanations to help account for the intra-democracy variation in terms of currency policy. Finally, Bearce and Hart (2017) provide a theory to account for the intra-democracy variation in immigration policy.

7. There is also foreign economic policy variation among autocracies that needs to be explained. For efforts in this direction, see Hankla and Kuthy (2013) and Steinberg and Malhotra (2014).
8. One possible complication to this argument concerns fiscal policy, which is expected to become more effective with international capital mobility and fixed exchange rates. But if fiscal policy has become effectively immobilized (i.e., it cannot be easily expanded due to existing deficits and debt or quickly contracted due to the inertia of current spending priorities), then democratic governments are likely to favor the more usable instrument of monetary policy to address internal macroeconomic problems; hence, a democratic preference for domestic monetary autonomy with flexible exchange rates over a more effective fiscal policy with fixed exchange rates.
9. The Phillips curve implies a set of inflation/unemployment combinations. Thus, there would be only one macroeconomic policy target (one's preferred inflation/unemployment combination along a one-dimensional line or curve), requiring only a single policy instrument (in this case, monetary policy).
10. Indeed, the research on voting and trade policy, especially in the US, tends to show that voters punish incumbents for unemployment in low-skilled manufacturing (Jensen, Quinn, and Weymouth 2017) and for import competition (Autor et al. 2016), leading legislators to vote in a more protectionist direction on trade bills (Feigenbaum and Hall 2015).

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CHAPTER 5

OPEN ECONOMY POLITICS REVISITED

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SCHOLARS of International Political Economy (IPE) working in the open economy politics (OEP) tradition use interests, institutions and international bargaining to explain economic policies, as well as international economic relations. Because a large amount of IPE research today fits squarely within the OEP paradigm, OEP is itself an expedient subject for investigation. In 2009, Lake provided a candid and detailed discussion of the OEP framework—describing its intellectual origins, contributions to IPE scholarship, and potential weaknesses. At that time, Lake described OEP as “an emergent paradigm” (2009a, 220). How has it fared since then?

In this chapter, I examine the progress of the OEP paradigm over the past decade. I begin by briefly describing the OEP framework. I then discuss recent developments that have challenged the OEP framework, including the behavioral revolution and the growing importance of economic geography and consequent politics of place. I evaluate how the paradigm has evolved in response to these challenges. Finally, I conclude with a discussion about emerging issues that confront the OEP paradigm going forward, including the rise of China and the growing backlash against globalization and the liberal international order.

WHAT IS OEP?

Interests, institutions, and international bargaining lie at the heart of the OEP paradigm. Scholars working in the OEP tradition use these factors to explain countries’ economic policies and international economic relations. Although all three factors play a role in shaping international economic relations, OEP adopts a partial equilibrium approach where scholars typically focus on part of the causal chain to gain tractable leverage over theoretical puzzles. In the OEP framework, the causal chain implicitly proceeds from the micro-level (i.e., individuals’ interests) to the macro-level in a linear fashion—reflecting the idea that politics flows up from individuals to interstate bargaining in a unidirectional manner (Lake 2009a).

The OEP paradigm first gained traction in the mid-1990s. At that time, scholars working in the OPE tradition focused primarily on international trade and sought to explain countries' varied trade policies using interests, institutions, and international bargaining. By the late 2000s, scholars had adopted the OEP framework to investigate a wide range of international economic phenomena including, as Lake noted at the time (2009a, 225–6): monetary and financial relations (Frieden 1991), foreign direct investment (Jensen 2006), immigration (Scheve and Slaughter 2001), foreign aid (Milner 2006), regulation (Mattli and Woods 2009), corporate governance (Gourevitch and Shinn 2005), and global governance (Kahler and Lake 2003).

In the subsequent decade, OEP scholars have continued to be highly productive. The body of scholarship has progressed to include an ever wider range of international economic phenomena, including, for example, global supply chains (Johns and Wellhausen 2016; Kim 2017; Osgood 2018), labor rights (Caraway et al. 2012; Mosley 2010), populism (Colantone and Stanig 2018a; Hays, Lim, and Spoon 2019), offshoring (Owen 2017; Rickard 2021), and the environment (Bechtel, Genovese, and Scheve 2019).

A striking observation emerges from these wide-ranging, recent studies, namely that public support for globalization is waning. Although globalization has always attracted skeptics, opposition to globalization—and the liberal international order more generally—appears to be rising. Hostility towards globalization materializes in precisely those areas most exposed to foreign imports (e.g. Colantone and Stanig 2018a, 2018b). This development presents challenging questions for IPE scholars, many of which now lie at the forefront of OEP research, including: will globalization's success doom its future (e.g. Margalit 2012)? Is the liberal international order facing an existential threat (Lake et al. 2021)? What, if anything, can governments do to shore up public support for economic openness (e.g. Rickard 2015)? The OPE framework—with its focus on interests, institutions and international bargaining—promised to offer insightful answers to these emergent questions.

CHALLENGES TO OEP OVER THE PAST DECADE

Although the accumulating body of scholarship in the OEP tradition over the past decade has advanced our understanding of world politics, the paradigm has nonetheless faced challenges. I discuss two such challenges here—the behavioral revolution and growing importance of economic geography—and evaluate how the OEP paradigm has responded to each. A key point emerges from this assessment, namely that the OEP framework has not remained static over the past decade but has instead evolved to respond to intellectual challenges and important events in the global economy. In this way, the OEP framework has proven to be remarkably flexible.

The Behavioral Revolution

The behavioral revolution broadly refers to the large and growing body of empirical research on preferences—often conducted via surveys (DellaVigna 2009; Hafner-Burton

et al. 2017). Survey research in IPE has proliferated as scholars probe voters' preferences on a range of topics including, international trade (e.g. Guisinger 2017), offshoring (e.g. Mansfield and Mutz 2013) and foreign aid (e.g. Milner and Tingley 2010) —to name just a few. The growing empirical evidence on voters' preferences is potentially a boon for OEP scholars because preferences lie at the heart of OEP; in fact, they make up the first link in the paradigm's implicit causal chain (Lake 2009a). However, some recent studies call into question a keystone of the OEP paradigm: the connection between economic theory and preferences.

In the OEP framework, preferences over alternative outcomes (i.e., interests) are derived from economic theory. Deducing interests from economic theory was one of the core innovations of OEP (Lake 2009a). By using economic theory, OEP scholars identify the key actors in a given policy area and the actors' preferred outcomes. In the area of trade, for example, some OEP scholars employ the specific factors model (e.g., Gilligan 1997; Rickard 2009). This model was originally articulated by Jacob Viner and builds on Ricardian trade theory; as a result, it is sometimes referred to as the Ricardo-Viner model. The model was developed and formalized mathematically by Jones (1971) and Mussa (1974). The specific factors model implies that industries play a key role in trade politics because workers and owners of capital employed in the same industry share common economic interests. Their interests align because they are stuck in their current industry of employment in the short to medium term due to the high costs of moving to a different industry. Because the economic fortunes of workers and capital owners are tied to the economic performance of their current industry, the two groups share a common economic interest.

OEP scholars have built on this insight. Some studies argue and find evidence that industries producing goods for the domestic market demand protection from foreign imports when the costs of adjustment are high (e.g., Hiscox 2001). Others show that voters employed in export-oriented industries are more likely to support liberalizing trade agreements than those working in import-competing industries (e.g., Hicks et al. 2014). In this way, economic theory provides the micro-foundations for OEP by identifying both the key actors in a given policy area and their economic interests.

In recent years, however, this foundation has been shaken by a growing number of studies that find little support for the predictions derived from economic theory (Mutz 2018; Rho and Tomz 2017). For example, some studies find that the vulnerability of a respondent's industry to foreign competition has little effect on individuals' trade preferences (Blonigen 2011; Mansfield and Mutz 2009; Rho and Tomz 2017). In contrast, non-economic variables such as identity (Scheve and Stasavage 2006) and gender (Guisinger 2016) appear to have relatively large effects on peoples' preferences about economic policies.

Although conclusions from the behavioral revolution are many and varied, one key point emerges: individual preferences are more complex and varied than economic theory alone would propose. This observation has important implications for OEP because if individual preferences do not conform to economic theory, then the foundations of OEP may be unsound. However, in order to fully assess the challenge posed by the behavioral revolution, OEP scholars must understand *why* preferences diverge from economic theory. The reasons why individuals' preferences differ from economic theory may, in fact, be consistent with the OEP framework. Two such reasons have been suggested to date: incomplete information and increasing returns to scale.

The Role of Information

Incomplete information may explain why individuals' preferences diverge from economic theory (Rho and Tomz 2017). It may be unclear to many people how complex economic policies impact their personal economic well-being. They may lack the information necessary to connect specific economic policies, such as freer international trade, to their own pocketbook. As a result, individuals may express preferences that are inconsistent with their economic self-interest, and consequently economic theory. They do so not because they are irrational but rather because they are poorly-informed. Once individuals are informed about the distributive effects of the policy in question, the correlation observed between economic self-interest and expressed preferences may tighten, as Rho and Tomz (2017) demonstrate with respect to international trade.

The OEP framework builds on theories that assume complete information. However, the OEP framework could be modified to relax the assumption of complete information. Relaxing this assumption would open up new avenues for future research on the role of information and elite cues in preference formation. Although these topics have spawned a substantial amount of research in other fields, considerable scope remains in IPE to explore these subjects. If citizens lack a comprehensive understanding of trade policy, for example, they may take cues about the economic impacts of trade from trusted sources, such as political parties, the media, or labor unions. Membership in the International Longshore and Warehouse Union (ILWU), for example, has been shown to affect members' trade policy preferences and may help to explain why their preferences do not conform to predictions derived from economic theory (Ahlquist et al. 2014). Further investigations of the role of information in shaping preferences and the sources of such information presents a fruitful avenue for future research in the OEP tradition.

New, New Trade Theory

New, new trade theory (NNTT) provides a second potential explanation for the unexpected individual-level heterogeneity in trade policy preferences. Owners of assets, such as capital and labor, employed in the same industry—but at different firms—often express different trade policy preferences. Variation in trade preferences within a single industry contradicts expectations derived from the specific-factor model of trade discussed above. Such variation is, however, consistent with NNTT.

NNTT demonstrates that within a given industry, some firms export while other firms produce goods exclusively for the domestic market. In other words, some industries export and import at the same time. Such “intra-industry trade” occurs because of consumers' love of variety and increasing returns to scale (Krugman 1985). Intra-industry trade was “not a subject of significant research” in the OEP framework 10 years ago (Lake 2009a, 236). However, it has garnered greater attention in recent years and the framework has been successfully adapted to accommodate the realities of intra-industry trade.

Originally, the OEP paradigm worked with the assumption that all production is characterized by constant returns to scale—that is, a doubling of inputs leads to a doubling of output. By relaxing this assumption to allow for variation in the returns to scale, a new

generation of firm-centered studies has emerged in the OEP tradition (e.g., Jensen et al. 2015; Kim et al. 2019; Osgood 2017). Some seek to understand the distributive consequences of intra-industry trade (e.g., Kim 2017; Osgood 2016). These studies typically build on the Melitz (2003) model, which uses firm-level heterogeneity in productivity to explain why only a minority of firms in an industry export goods abroad. Because only the most productive firms in an industry export (Bernard et al. 2007), firms in the same industry can have different international profiles and given their varied positions in the international economy, firms in the same industry will hold different policy preferences. This observation provides a potential explanation for the unexpected within-industry individual-level heterogeneity in trade preferences. Contrary to expectations derived from the specific factor model, individuals employed in the same industry may express varied opinions about trade if they are employed at firms with different international profiles. Individuals employed at exporting firms will likely support trade openness, and globalization more generally, because it benefits their employer and thereby themselves. In contrast, people employed in the same industry but at an import-competing firm will likely oppose trade openness because competition from lower-cost foreign imports could reduce their wages or even result in unemployment.

This discussion makes a straightforward, yet noteworthy point. Innovation in economic theory often engenders advances in OEP. Because OEP scholars derive interests from economic theory, their specification of interests will change as economic theories advance. OEP research can similarly advance changes in economic theory, for example, by demonstrating the importance of politically-oriented dynamics that, when not accounted for, limit economists' models. In this way, OEP provides a bridge between economics and political science (Lake 2009a, 225). Working within agreed standards of scientific inquiry facilitates scholarly interactions across the two fields (Lake 2009a, 231).

Going Forward: The Role of Identity

Further research is needed to develop a more nuanced, yet still deductive conception of interests. One source of interests that merits further attention is identity. Identity appears to play an important role in shaping economic interests and conditioning the effects of economic shocks on preferences. Cultural identity drives support for populists (Inglehart and Norris 2016) and racial identity influences individuals' support for trade (Guisinger 2017). Whites and non-whites seem to respond differently to manufacturing decline in their communities (Knowles and Tropp 2018). Combining identity politics with economic interests is an important challenge for the OEP paradigm going forward (Broz et al. 2021).

THE POLITICS OF PLACE

Place-based politics present a second challenge to OEP. Place-based politics emerge when the locality in which people live influences their economic interests, policy preferences and voting behavior. People living in prosperous regions appear to have different interests than

people living in economically distressed regions. In affluent regions, voters are more likely to support continued economic openness, while voters in distressed regions are more likely to demand protectionist policies, like trade barriers (Malgouyres 2017). Voters' interests also differ between urban and rural areas (Rodden 2019). Voters in rural regions appear to be increasingly skeptical of trade, while those in urban regions tend to be more supportive of globalization.

The growing importance of geography and the ensuing "politics of place" have several implications for the OEP framework. First, place-based politics provide another potential explanation for individual-level heterogeneity in preferences. Second, the politics of place suggest an alternative unit of analysis for OEP research, which has traditionally focused on countries. Third, place-based politics renew questions about the ever-present aggregation issue—that is, how institutions aggregate interests. I discuss each of these in turn, after first describing some of the potential sources of place-based politics.

What Explains the Politics of Place?

While globalization has shrunk the importance of distance between countries, geography matters now more than ever within countries (Rickard 2020). Economic activities, such as production and employment, occur unevenly across space within countries. In the United States, for example, the footwear industry was largely concentrated in the Northeast, particularly Massachusetts, and in the corridor from St. Louis to Wisconsin (Sorenson and Audia 2000). Because economic activities like footwear manufacturing are unevenly distributed across areas within countries, globalization impacts various regions differently. Some regions benefit from globalization, while other areas lose out because of their varied employment and production profiles (e.g., Autor et al. 2013; McCann 2016). Areas heavily reliant on footwear manufacturing in the US were hit hard by rising imports of foreign-made shoes, which often enjoyed a cost advantage.

Economic geography—that is, the distribution of employment and production across space—engenders place-based politics (Rickard 2018, 2020) and erodes voters' trust in politicians and institutions. In England, for example, the uneven distribution of economic activity fuels voters' suspicion of politicians. Many voters, observing that London enjoys a disproportionate share of the country's wealth, believe that politicians shower London with unfair amounts of government money (Rickard 2020). In this way, the concentration of wealth in London and the south east of England erodes voters' confidence in politicians and politics. The UK's uneven economic geography is also credited with the geographic pattern of votes to leave the EU in the 2016 referendum (Colantone and Stanig 2018b).

Similarly, the French government faces rising discontent from voters frustrated by geographic inequalities. The *gilets jaunes* protests of 2018 were inspired, in part, by the availability of public services in Paris, as well as the city's relative wealth. An increase in the fuel tax initially sparked the protests. The proposed higher taxes would fall disproportionately on people living outside of Paris where public transport is less readily available. The fuel tax rapidly set alight other grievances among people living in rural and suburban areas (Lichfield 2019).

The growing geography of discontent and related "politics of place" arise, in part, because of the uneven distribution of employment and particularly manufacturing employment.

Within countries, some region's populations are primarily employed in manufacturing while other regions have little, if any, manufacturing employment. Regions with high shares of manufacturing employment find themselves increasingly vulnerable to globalization and specifically to imports from developing countries. In contrast, regions with large shares of the population employed in services or non-tradable sectors face less disruption from globalization. In short, the economic costs of globalization fall unevenly across regions within countries because of economic geography.

The "China shock"—where imports from China generate negative employment and wage effects in local labor markets (Autor et al. 2013)—transpires precisely because of the uneven geographic distribution of manufacturing employment. Import shocks from low-cost producers like China put downward pressure on wages and employment in affected local labor markets (Autor et al. 2013). These labor market effects lead to cascading regional effects: labor force participation in the area declines, young people leave, property values drop, local tax revenue falls, and local public services deteriorate (Feler and Senses 2017; Broz et al. 2021). At the same time, more affluent places increasingly pull away from poorer ones (Venables 2008). The productivity gap between the most productive 10 percent of regions and the bottom 75 percent grew by nearly 60 percent over the past 20 years in developed countries (Economist 2017). In the US, the largest metro areas (i.e., those with more than one million residents) accounted for 72 percent of the nation's employment growth since 2008 and over three-quarters of growth since 2015—even though these areas accounted for just 56 percent of the overall population (Hendrickson et al. 2018).

Yet even as regional disparities grow, people move less often within countries. In the US, the percentage of Americans who moved across state lines each year fell by 50 percent during the period from 1990–2018. Many people consequently remain in "left-behind places" with fewer and fewer economic opportunities. As areas within countries grow apart economically, voters pull apart politically. National politics in many countries is becoming increasingly polarized along regional and geographic divisions (Colantone and Stanig 2018a; Dippel et al. 2015; Dorn et al. 2020; Malgouyres 2017; Rodden 2019).

Implications for Interests

Because of growing economic disparities among regions within countries, peoples' interests are increasingly shaped by where they live. Residents in areas with vibrant local economies prefer policies and politicians different to those favored by residents in areas with declining local economies (Rickard 2021). Yet, despite the growing importance of geography, the importance of place is missing from standard theories of trade, which typically assume that individuals are atomistic and their welfare depends only on their individual income. As a result, standard theories overlook a potential source of influence on individuals' preferences.

The politics of place provide another potential explanation for the unexpected heterogeneity in individual level preferences. People living in a given region may hold more similar preferences than economic theory would predict. Because they live in the same community and experience the same local economic conditions, even people who work in different industries, sectors or firms may share similar trade preferences with their neighbors. Workers in non-tradable service sectors, for example, may support trade protection because of the negative impact imports had on their community even though their own

sector of employment does not face any direct competition from foreign trade. Similarly, individuals employed in the same industry may hold different policy preferences if they live in communities differentially effected by globalization.

A New Unit of Analysis

Place-based politics also suggests a new empirical focus for OEP research. Historically, countries were the most common unit of analysis in empirical OEP research and many investigations took the form of large-N, cross-national studies. In recent years, however, a growing number of OEP studies focus on subnational units within countries. Recent research also places a greater emphasis on causal identification and often seeks to randomize treatments—either via survey experiments (e.g., Chaudoin 2014) or field experiments (e.g., Findley, Nielson, and Sharman 2013).

The growing number of subnational studies in IPE may be due, at least in part, to the rising importance of economic geography and place-based politics. While this development repudiates critics' claim that OEP is overly state-focused, it raises several challenges for OEP scholars. First, IPE scholars must be mindful to explain how single-country, subnational studies contribute to understanding international relations. Second, researchers must be careful not to "overstretch" theories originally developed to explain state-to-state relations. Nations interact with one another differently than subnational units within countries and bargaining theories developed at the national level may not be appropriate for explaining political behavior at the subnational level. Subnational research may therefore require new or modified theories. Third, methodical concerns about spatial interdependence at the international level must be adapted for subnational research (Harbers and Ingram 2019).

Fourth, scholars must identify what subnational units are most theoretically appropriate. The answer will, of course, depend on the research question. Local labor markets offer one possibility. Local labor markets make up sub-economies within countries and often experience differential trade shocks because of their initial patterns of industry specialization (Autor et al. 2013). As a result, local labor markets may provide theoretically relevant units of analysis with which to explore the consequences of globalization. However, local labor markets are often difficult to identify *ex ante* and data is rarely available at this level of disaggregation. To overcome these methodological challenges, Autor et al. (2013) approximate local labor markets using commuting zones. In the UK, Ballard-Rosa et al. (2021) use "travel to work areas," which are self-contained, sub-regional labor markets that are constructed using commuting data. Other scholars adopt units that are more distant approximations of local labor markets, such as the EU's NUTS regions, which are territorial units used to compile statistics.

Geographically-defined electoral districts provide an enticing unit of analysis with which to examine the political consequences of globalization's varied regional effects. Geographically-defined constituencies aggregate voters' revealed preferences at election time. The aggregation of voters' preferences influences the identity of the political parties (and politicians) in government and ultimately policy outcomes. A number of studies in the OEP tradition have adopted politically-meaningful units of analysis, including, for example, electoral districts in Norway (Rickard 2018), states in the US (Broz 2011), and municipalities in both Brazil (Owen 2019) and Spain (Rickard 2021).

Going forward, OEP scholars may eschew arbitrary geographic boundaries and instead seek to identify individuals' relevant community. For many individuals, their pertinent community may not be their electoral district or local labor market. Instead, they may identify with their block, street, or school district. To identify an individual's relevant community, scholars could ask survey respondents to trace their community on a map. This method would allow respondents to define the geographic community most relevant to them (e.g., Coulton et al. 2011).

Finally, as the trend toward subnational research gains momentum, it is important not to lose sight of national-level variables. Country-level characteristics continue to influence politics and may systematically condition the effects of geographically-concentrated economic shocks. For example, some evidence suggests that the "China shock" elicits anti-globalization backlash only where income is already highly unequal (Georgiadou, Rori, and Roumanias 2018; Rogowski and Flaherty 2019). In countries with more equitable income distributions, exposure to imports may generate little or no support for anti-globalization movements (Rogowski and Flaherty 2019). Going forward, novel insights may be gleaned from combining subnational analyses with country-level variables, such as inequality.

Revisiting the Aggregation Issue

The aggregating issue refers to how theories move from individual to collective decision-making. Institutions play a central role in this process because political institutions aggregate individuals' preferences to produce political outcomes and ultimately policy decisions. Precisely how institutions aggregate individuals' preferences depends on both the institutions themselves and the distribution of preferences. However, many studies focus exclusively on institutions. For example, a prominent study of electoral institutions simply assumes that the distribution of policy preferences is similar in all electoral districts (Persson et al. 2007). But this assumption is unlikely to hold because economic activities are unevenly distributed across space within countries (e.g., Autor et al. 2013; Rickard 2012, 2018) and as previous studies have shown, voters in regions with different economic profiles have systematically different political preferences (e.g. Colantone and Stanig 2018b, Dorn et al. 2020).

Ignoring the geographic dispersion of voters with shared interests would be innocuous if politicians elected via different institutions were equally responsive to geographically concentrated (or geographically diffuse) interests. But different electoral systems provide varied incentives for politicians to represent geographically concentrated (or diffuse) interests (Rickard 2012, 2018). In countries with plurality electoral systems and candidate-centered election competition, politicians face powerful incentives to privilege the interests of geographically concentrated groups—particularly groups concentrated in their own constituency. As a result, policy outcomes more closely reflect the interests of geographically-concentrated groups in plurality systems. In proportional representation (PR) systems—and particularly closed-list PR systems with party-centered elections—leaders have incentives to cater to the interests of geographically diffuse groups and as a result, policies in PR systems tend to better represent the interests of geographically diffuse groups (Rickard 2012, 2018).

Other political institutions may also generate varied incentives for elected leaders to privilege either geographically concentrated or diffuse interests. Federal institutions accommodate territorially based diversity (Bermeo 2002), and as a result, concentrated interests may enjoy relatively greater expression in federal systems. Subnational governments may also be especially responsive to the policy demands of economic actors concentrated in their region (Rodden and Wibbels 2010; Wibbels 2000). In short, the politics of place bring renewed attention to the aggregation issue in international relations and suggests novel avenues for research in the OEP tradition.

Going Forward: The Geography of Firms

One potentially productive topic for future research is the connection between new, new trade theory—with its focus on firms—and the politics of place. The geography of firms—that is, where firms are located and where their employees live—may influence their political clout. Firms with employees in multiple states or electoral districts may enjoy greater political influence in some political systems than others. Firms with employees in multiple countries may enjoy more political leverage than firms with employees in a single national jurisdiction. The geography of firms, together with countries' electoral institutions, may help to explain certain types of policy outcomes, such as subsidies, and governments' varied responses to economic shocks.

A nascent literature has begun to explore the geography of firms in the OEP framework. Huneus and Kim (2018) measure firms' political connections based on the geographic proximity between their headquarter locations and politicians' districts in the US. Bisbee and You (2019) demonstrate how the geographic distribution of a firm's subsidiaries creates links between legislators and firms. These studies point to a productive new research agenda that links the politics of place with new, new trade theory and potentially deepens our understanding of the aggregation issue.

CHALLENGES GOING FORWARD

The OEP framework has evolved over the past decade in response to intellectual developments and momentous events in the global economy. Going forward, OEP will continue to face new challenges. In the near future, two developments are poised to confront the OEP paradigm: the growing power of China and the backlash against globalization and the liberal international order.

The Backlash against the Liberal International Order

In recent years, the liberal international order—a core set of principles and practices that structure international relations among democratic, capitalist, and industrialized countries—has come under increased attack (Lake et al. 2021). Since the late 1940s, the rise of free trade and international capital mobility, as well as spreading democracy, and promoting

human rights have been credited to the liberal international order. But in recent years, the liberal international order has come under growing pressure from both internal and external forces, as documented by Lake et al. (2021).

Some of the stresses on the liberal international order raise questions for the OEP framework. In the OEP framework, a one-way causal chain is implicitly assumed that begins with interests, moves through domestic institutions, and ends with international bargaining. However, international bargaining is not the end of the story, as the growing backlash against the liberal international order makes clear. Instead, international bargaining and the outcomes agreed via negotiations between states feedback into the causal chain.

Internationally negotiated outcomes shape interests. Globalization is itself an internationally agreed outcome. Over the past several decades, successful negotiations among states, including those concluded under the auspice of the World Trade Organization (WTO), have significantly lowered policy barriers to international trade. The movement of goods, capital and, in the case of the EU, people across national borders represent an internationally agreed upon outcome. These flows have, in turn, shaped peoples' views about economic openness and generated discontent with the liberal international order (e.g., Colantone and Stanig 2018a; Malgouyres 2017). Public support for economic globalization has fallen in recent years and some facets of globalization have provoked particular ire, including immigration (e.g., Goldstein and Peters 2014) and offshoring (e.g., Mansfield and Mutz 2013).

Opposition to globalization appears to be growing fastest in places most exposed to international markets. This observation emerges in a variety of countries—often using an innovative measure of globalization originally estimated by Autor et al. (2013) and commonly known as the “China shock.” Autor et al. (2013) use Chinese exports to estimate the effects of globalization because China's exports to developed countries are plausibly exogenous to subnational labor markets and allow for causal identification. China's exports increased dramatically over the period from 1991 to 2007. The increase stemmed largely from China's spectacular economic growth during this period and also from China's admission to the WTO in 2001 (Autor et al. 2013).

Because economic activities are unevenly distributed across space within countries, some areas are more exposed to imports from developing countries like China. As a result, globalization impacts different regions in countries differently, depending on their economic profiles. People living in regions more exposed to globalization, as measured by Autor et al.'s “China shock,” are more likely to express skepticism towards globalization and the liberal international order than people living in less trade-exposed regions. In the UK, for example, citizens living in areas exposed to greater inflows of Chinese goods voted to leave the EU at higher rates in the 2016 referendum (Colantone and Stanig 2018b). In France, voters in local labor markets exposed to greater Chinese imports over the period 1995–2012 voted at higher rates for the far-right, anti-globalization *National Front* party's candidate in the 2012 presidential election (Malgouyres 2017). And across 15 European countries, regional import shocks from China are associated with higher vote shares for isolationist, anti-globalization parties (Colantone and Stanig 2018a). This evidence suggests that internationally agreed outcomes, like China's accession to the WTO, can feedback into the causal chain by shaping interests.

Studies of environmental politics provide further evidence that outcomes reached via international bargaining can shape interests. Bechtel and Scheve (2013) demonstrate that the design of an international climate agreement can influence public support for a global climate agreement. Tingley and Tomz (2020) show how voluntary international commitments

on climate change can shape public opinion. They find that public support for emissions control policies is much higher in scenarios where the US government joined the Paris Agreement than in scenarios where it had not.

Taken together, these studies suggest that internationally agreed outcomes, such as trade liberalization or global climate treaties, can alter interests. Feedback from the international arena to domestic politics is consistent with the core assumptions of the OEP paradigm. However, the OEP framework typically takes interests as the starting point and internationally agreed outcomes as the end point. Modifying the OEP framework to incorporate feedback from the international arena to domestic politics, and specifically interests, seems potentially valuable and perhaps a natural evolution of the framework.

The Rise of China

The rise of China is arguably one of the most significant recent developments in the global economy. Although critics of OEP contend that the paradigm ignores the issue of structural power, OEP researchers have productively addressed at least some of the implications of China's growing power in recent years. For example, OEP scholars have become more attentive to the effects of policy-induced price distortions. Traditionally, OEP adopted the "small country" assumption of international economic theory, which posits that production and consumption in any single state are sufficiently small relative to global totals that all actions, including government policy, will have no noticeable effect on world prices (Lake 2009a). In recent years, however, OEP scholarship has relaxed the small country assumption and recognized that some countries can act in ways that affect international prices, as demonstrated by the recent US-China trade war.

The US-China trade war made it clear that a single country can, in fact, impact global prices. In 2018, China imposed tariffs on a variety of US products in response to the Trump administration's trade policy actions. China imposed a 25 percent tariff on soybeans from the US. As the world's largest importer of US soybeans, China had considerable market power and following China's implementation of the tariff, US exports to China collapsed and soybean futures fell (Taheripour and Tyner 2018). The price of a bushel of US soybeans dropped by more than a dollar in June 2018 and hit a 10-year low in September that year (Chyzh and Urbatsch 2019). US soybean producers' revenue decreased by 10 percent from what they might have expected to earn, and profits fell even further (Chyzh and Urbatsch 2019).

More work remains to be done to fully understand the functioning of China's economy and its role in the global economy. The OEP paradigm could, for example, be productively used to illuminate the identity of the winners and losers from globalization in China. In China, factor returns do not conform to expectations. Incomes for low-skilled labor have increased by roughly 100 percent, but incomes for skilled labor have increased by about 200 percent and for employees in state-owned enterprises by about 250 percent (Lake 2020). This pattern is inconsistent with economy theory, which predicts that the greatest gains from globalization should go to unskilled labor given China's factor endowments. Perhaps the relatively high returns to skilled workers reflect the structure of political power in China. OEP scholars are well-positioned to explore how the political realities in China mediate factor returns and the distributional effects of trade.

Normal Science

A final challenge to the OEP paradigm comes from its own success. Critics sometimes allege that IPE research is “boring.” This allegation may arise from the fact that much of the IPE scholarship produced today can be described as normal science—that is, research that slowly accumulates detail in accord with established broad theory, without questioning or challenging the underlying assumptions of that theory (Kuhn 1962). OEP provides an established theoretical framework. Many IPE scholars work within the OEP paradigm either deliberately or reflexively and most do so without challenging its underlying assumptions, particularly in the US. As a result, much of modern IPE research can be described as normal science. The trend toward normal science research in IPE reflects a maturing discipline where knowledge builds on earlier findings and advances incrementally.

Some critics of OEP would seem to prefer less detailed answers to bigger questions rather than tightly constructed answers to smaller questions. Of course, answering big questions is important. But normal science may ultimately produce answers to big questions via the accumulation of knowledge. To accomplish this goal, scholars must be careful to build on, rather than reproduce, existing research. The claim that IPE research is “boring” may stem, in part, from scholars’ inattention to prior scholarship. To avoid this charge and ensure that OEP research builds on—rather than duplicates—earlier findings, scholars must be careful to read, review and cite relevant prior research. Such due diligence becomes ever more important as the field matures and the body of knowledge grows. Although this responsibility falls equally on all scholars, differences in PhD training and the varied requirements for comprehensive PhD exams often means that knowledge of prior IPE scholarship varies. However, these differences are not insurmountable and scholars on both sides of the Atlantic must ensure that their research builds on earlier findings, connects with ongoing theoretical debates, and advances our understanding of the world. Only by reading broadly and engaging meaningfully with existing scholarship can completion of the bridge across the Atlantic, proposed by Lake in 2009(b), be achieved.

CONCLUSION

OEP has matured and developed over the past decade. As a framework, it has proven to be enormously productive and adaptable—integrating diverse economic phenomena under a common theoretical umbrella and providing a framework flexible enough to react to significant events in the global economy, such as the rise of China and the growing backlash against globalization and the liberal international order. The accumulating body of scholarship in the OEP tradition has moved our understanding of world politics decisively forward. Critics of OEP have yet to offer an alternative, more empirically powerful theory and as a result, OEP continues to progress as the dominant paradigm in IPE research, most notably in the US but in other parts of the world as well.

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CHAPTER 6

FEMINISM

GEORGINA WAYLEN

INTRODUCTION

IN 1997 a commentator remarked in a review essay that there was little work that could be easily categorized as feminist IPE (Robinson 1997). This is definitely not the case today. We can trace the emergence of a distinctive body of scholarship that constitutes a feminist International Political Economy (IPE)—including both feminist narratives of the global political economy and interrogations of what it means to do feminist political economy—to the beginning of the 2000s (Marchand and Runyan 2011; Cook et al. 2000; Rai 2002; Peterson 2003). Since then, an identifiable pluralist and heterogeneous feminist IPE (or IPE work informed by feminism) has been growing and thriving, as witnessed by the large body of scholarship showcased in the handbook edited by Elias and Roberts (2018a)—the largest attempt to map and interrogate the field of feminist IPE to date. While sharing important commonalities, feminist IPE is therefore not homogenous and not particularly new, with multiple diverse roots and influences (Elias and Roberts 2018b). Despite its diversity, feminist IPE scholarship does share some fundamental characteristics: a commitment to the centrality of gender for understanding the global political economy, even if there are debates about what this means and how to study it (Peterson 2005, 499). Feminist IPE scholars also have a shared commitment to counter gender inequality within the global political economy, but again they differ as to how to do this.

Yet despite feminist IPE's importance and vibrancy, feminist IPE scholars point to the continuing lack of dialogue or at best a one-sided engagement between feminist and non-feminist IPE. This remains, despite the long-standing potential for interchange, particularly between feminist IPE and constructivist analyses (Waylen 2006; Elias 2011). In considering feminism in IPE, this chapter will attempt several tasks. First, it will explore (yet again) the relationship between feminist and non-feminist IPE to assess whether levels of engagement, particularly of non-feminist IPE with feminist IPE, have increased. It will then try to explain some of the patterns that we still see today. Secondly, the chapter will outline some of the key insights that have been generated by feminist IPE since its inception. The chapter argues that feminist IPE now forms a sophisticated body of work, distinguished by its interdisciplinarity, methodological and theoretical diversity, and empirical reach, as well as its

continuing refusal to be disciplined by boundaries that Robinson (1997) noted more than two decades ago. The chapter shows that feminist IPE's important theoretical and empirical insights can and should inform key debates in IPE, for example around social reproduction (Elias 2011). It concludes by highlighting exciting new theoretical developments that an intersectional feminist IPE is contributing to, as well as recent points of coalescence and convergence that offer enhanced potential for interchange between feminist and non-feminist IPE.

FEMINIST AND NON-FEMINIST IPE—WHY DOES INSUFFICIENT DIALOGUE CONTINUE?

The initial early optimism of feminists like Sandra Whitworth (1989), who argued in the late 1980s that it was easier to raise feminist concerns in IPE than International Relations (IR) has not been realized. In the 1990s, pioneering chapters published in “critical” IPE texts by feminist scholars like Tickner (1991) and Whitworth (1994) critiqued IPE for its continuing lack of attention to gender. By the mid-2000s numerous feminist IPE scholars were still lamenting the lack of engagement of non-feminist IPE scholars with their work and the gender blindness of most IPE (Peterson 2005; Waylen 2006; Griffin 2007). Several factors were seen to contribute to this on-going gender blindness.

Building on her earlier work, Ann Tickner (2001, 66) argued that in IPE “the neo-realist and neo-liberal frameworks, with their common focus on state-centric issues of co-operation and conflict and their positivist and rationalist methodologies, do not lend themselves to investigating gendered structures of inequality.” The paramountcy of the division between state and market of much IPE also acted to preclude the easy incorporation of gendered perspectives, despite feminist IPE's important critiques of the state and market as gendered institutions. As a result, social reproduction and the private sphere were inevitably absent from most IPE debates. Much IPE scholarship, often emanating from United States using “problem solving” or open economic politics (OEP) approaches, was entirely blind to gender, not recognizing gender or gender inequality as an issue or problem relevant to IPE.

Perhaps more disappointingly for many feminist scholars, “critical” or “heterodox” IPE also remained largely gender blind, not engaging with gender as an analytical category constitutive of the global political economy (Waylen 2006; Griffin 2007). This was despite shared ontologies that gave primacy to the construction of social relations and epistemologies that were skeptical of positivism and empiricism, potentially offering more promise of dialogue and engagement. Its gender blindness continued, regardless of claims by prominent male scholars like Murphy and Nelson (2001) that “British”/critical IPE was more focused on the “contentious politics of gender” (together with race and the environment) because some early British School collections, such as Murphy and Tooze (1991) and Gill and Mittelman (1997) included a single feminist contribution (with a maximum of two). At best, much critical IPE still saw gender as simply as a synonym for women, adding women in either as victims of larger social processes, for example as exploited workers or as activists in women's movements, rather than using a gendered lens to examine key processes central to the global political economy (Waylen 2006).

This tendency to ignore or marginalize feminist IPE was further reinforced in the furore over the so-called “transatlantic divide” debate sparked by Benjamin Cohen’s 2007 article and his 2008 book. Cohen promulgated the familiar characterization of IPE as divided into two main schools: the US School dominated by positivism, quantitative methods, and empiricism; and the more critical British School that challenged orthodoxies of all kinds and was often associated with Gramscian or Marxist approaches. Cohen also structured his conceptualization of IPE around the central role of seven key scholars—“the magnificent seven” (including one woman—Susan Strange—facilitating the claim that IPE was open and women-friendly). However, this notion of “heroes”—brilliant foundational individuals who created the discipline from 1970s onwards—resonates with Peterson’s (2018) critique of IPE as embodying a masculinism (together with positivism and modernism) that acts to exclude women. As Elias (2011) argues, it is also not how gender scholars represent the emergence of feminist IPE. This debate, reinforcing the rigid division of IPE into two camps and promulgating an “origins” story of a few heroic—predominantly male—individuals, continued the exclusion of feminist IPE from IPE.

More than a decade on from the height of the transatlantic divide debate, its contemporary salience and IPE’s current state have been re-evaluated in ways that help us reconsider feminist IPE’s continuing exclusion and whether it has changed over time (Clift et al. 2022; Seabrooke and Young 2017). Several IPE scholars have argued that IPE is now more complex and in many ways more pluralistic than simply reflecting the transatlantic divide. In IPE research this is evidenced by the number of different niches and networks (around five to seven according to Seabrooke and Young (2017)) are that important, reflecting the increased diversity of both method and subject matter. Constructivist and (historical) institutionalist research in particular has become prominent recently, offering more potential for interchange. In addition, despite the assumption of a dualism, there are more intellectual similarities between the US and British IPE than have often been portrayed. Both share the origins myth of “modern IPE” that developed in the 1970s and this notion of the “seven greats” (Clift et al. 2022). Both schools emphasize the importance of the market/state dichotomy even if it is conceptualized in different ways. But, as both Seabrooke and Young (2017) and Clift et al. (2022) argue, although contemporary IPE is not just characterized by the transatlantic divide, the boundaries to IPE itself have become more narrowly drawn to create a subfield that is less interdisciplinary and increasingly dominated by IR and political science. However, as Pevehouse and Seabrooke outline in the Introduction to this handbook, this new pluralism of approach and method is not represented in the teaching of IPE. Many IPE textbooks are still characterized by a polarity, emphasizing this long-standing dualism to the exclusion of other perspectives.

The Inclusion/Exclusion of Feminist IPE in IPE Teaching and Research

Clift et al. (2022) and Seabrooke and Young (2017) argue that if we want to understand contemporary IPE and its evolution, we need to look at more than the development of its dominant ideas and the intellectual debates about what IPE is and is not. We also need to consider the practicalities of how IPE operates, namely how its boundaries are delineated and how the

networks prevalent in IPE facilitate processes of inclusion and exclusion. If we understand knowledge production as a social process, then the power dynamics at work facilitate this exclusion and inclusion. In particular, Clift et al. (2022) highlight four key mechanisms. These establish: what the field is (i.e., admissibility); how work should be conducted; the legitimate boundaries (i.e., borders); the engagement with cognate fields (i.e., external relations). These practices, frequent in both teaching and research, are hugely significant as they construct what counts as IPE, what should be on syllabi, and what is deemed to be influential, path-breaking, and noteworthy scholarship. Clift et al.'s (2022) analysis of IPE's everyday mechanisms of gatekeeping and boundary practices also has relevance for feminist IPE.

Examining these power dynamics and the four mechanisms of inclusion and exclusion can therefore give us greater insights into feminist IPE's marginalization within IPE. All four areas are significant when considering feminist IPE's exclusion—whether it is the assumptions about what constitutes appropriate and relevant work; the methods and approaches that are used; as well as the limits of feminist IPE. As we will see in the second half of this chapter, feminist IPE is characterized by broader conceptions of admissibility, a greater pluralism of method and approach and more engagement with other fields, than IPE as a whole. As a result for many positivists in particular, the subject matter, methods, and approach adopted by many feminist IPE scholars are unacceptable.

To demonstrate how these factors play out in practice, Clift et al. (2022) highlight the content of textbooks, citation practices, the role of academic gatekeepers, and what journals will publish, as important mechanisms that all reinforce what is and is not IPE. A cursory look at recent IPE teaching texts demonstrates that, as well as still foregrounding the continuing binary divide between critical/problem solving or US and British IPE, the *de facto* exclusion of feminist IPE continues. Feminist IPE's inclusion in contemporary teaching texts still ranges from nothing, to only one or two chapters at the other end of the continuum. At one extreme, Ravenhill (2016, 5th ed.) has no discussion of feminist IPE and only two references to gender in the index and Cohen (2014) only has three mentions of feminism in his section on the British School. Textbooks in the middle of the continuum, generally include a small section on feminist IPE. For example, Broome (2014) has three pages and Cohn (2016, 7th ed.) has two pages on feminism. At the other end of the continuum, O'Brien and Williams (2016, 5th ed.) still only have one chapter on gender and feminism and Stubbs and Underhill (2005, 3rd ed.) include two chapters, one of which is an updated version of Whitworth's 1994 piece. Therefore there is little improvement from the critical IPE textbooks of the 1990s and even less evidence of the absorption of feminist IPE concepts and approaches into the mainstream of IPE.

If we turn to IPE research, we can see that IPE scholars remain predominantly male. The 2006 TRIP survey showed that women comprised only 22 percent of IPE scholars (very similar to the 23 percent women in IR at that time (Jordan et al. 2009)).¹ Disappointingly, this figure fell to 21 percent in the 2011 TRIP survey at a time when the percentage of women in IR as a whole had increased to 31 percent (Sharman and Weaver 2013, 1095). However, citations and personal reporting do show that, in addition to the small number of women—like Amsden, Milner, and Sassen as well as Strange—recognized in the 1990s, more women have become prominent within IPE in the last two decades.

Examining at how these trends translate into published IPE research, we see that despite forming 22 percent of IPE scholars, in 2006 women authored only 14 percent of all articles in the top journals (Maliniak et al. 2009). Sharman and Weaver (2013, 1095) found a somewhat

better picture. Although women authored only 18.8 percent of the IPE articles published in the top 12 IR journals, they authored 22.7 percent of the articles in RIPE between 2000 and 2010 in line with their representation in the wider field. But women's citation rates are generally lower than men's as men cite female authors less often than male authors and women also cite themselves less than men do, so on average, even articles by women in top journals will be less cited than men's articles (Teele and Thelen 2017).

If we consider the substantive content of IPE research, we can see from the 2006 TRIP survey of IR scholars in both the US and Canada (the latter being historically more sympathetic to critical and feminist IPE than the former), it is predominantly women who undertake feminist IR and feminist IPE research. Although 29.8 percent of women and 16.5 percent of men reported employing feminism in their work; only 7 percent of women and 0.15 percent of men said they were primarily committed to feminism (Maliniak et al. 2013, 894). In 2006 therefore, there were relatively few feminist IPE scholars in IPE (namely those primarily committed to feminism) and, of these, the overwhelming majority were women. It is also noteworthy that in both IR and IPE women are more likely to identify themselves as constructivists than men.²

We can see how these trends play out in the publication rates of feminist IPE scholarship in the mainstream of IPE (rather than in specialist feminist journals) by examining some leading IPE journals. By 2020, no studies had specifically focused on this, and it was not possible to access data on submission and rejection rates for articles on gender and feminism. However, examining articles published in three leading journals—*Review of International Political Economy* (RIPE), *New Political Economy* (NPE), and *International Organization* (IO), we see some significant differences between them.³

Overall, all three published little feminist IPE until very recently. A search for articles with either gender or feminism in their title (a rough proxy for feminist IPE), reveals that RIPE and NPE were fairly similar.⁴ At the end of December 2019, NPE had published 11 articles with gender and three with feminism in their titles since its first issue in 1996. RIPE had published 13 articles with gender and only one with feminism in the title since 1994. These 14 explicitly feminist IPE articles comprise less than 2 percent of the articles published since RIPE's inception.⁵ *International Organization*, however, fared even worse, perhaps unsurprising given its overall orientation. Only six IO articles contained gender in their title and none contained the term feminism, and all six of these articles are better classified as IR. In terms of changes over time we see that until 2019, NPE, if anything, saw a decline in the number of feminist IPE articles it included. It published four explicitly feminist IPE articles between 1996 and 1999, another four between 2000 and 2009 but only three between 2010 and 2019. In contrast, RIPE substantially increased the amount of feminist IPE it publishes. After only two explicitly feminist IPE articles in both the 1990s and 2000s, it published eight in the next decade, with six of these appearing in 2019. This steep rise coincided with RIPE's appointment of a female editor-in-chief, and for a short period RIPE's editorial team became entirely female, highlighting the gatekeeper roles of some male academics in powerful positions as journal editors.

Yet, if we analyze the reach of the feminist IPE work published by these journals, we see that in December 2019, Isabella Bakker's (2007) article on gender and social reproduction was the third most read article in NPE and Elisabeth Prugl's (2015) article on neoliberalizing feminism was the second most cited NPE article of all time. Therefore, as we will see, even if feminist IPE does not have much traction within IPE itself, it has a broad readership,

including in a wide range of disciplines outside of IPE. Having established feminist IPE's position in IPE, we can now examine its intellectual content and development.

FEMINIST IPE THEORIZING AND ANALYSIS

In an important recent contribution to these debates, Elias and Roberts (2018b, 4) elaborate on what they consider to be feminist IPE. Because there is no "singular or straightforward way of 'doing' feminist IPE," they deliberately do not define what it is or should be. Instead, using the terms feminist approaches and gender approaches interchangeably, they (2018, 6) understand feminist (and gender) approaches in IPE "to be ones that critically interrogate the way in which gender operates as a relation of social power." In their view, gender operates both as a discourse and a structural relationship of inequality within the global political economy. Feminist political economy is a "political project that seeks to *understand* and *challenge* (my emphasis) how the categories of masculine and feminine have been established and maintained, and to document and undermine the privileging of the former" (Elias and Roberts 2018b, 4). They emphasize that there is not one single feminist agenda, even though there is an explicit commitment to feminist praxis (i.e., linking feminist theory and practice).

As Elias and Roberts (2018b) rightly point out, because it is characterized by diversity, there is no universal linear story about rise of a body of work considered feminist IPE. This diversity has enabled it to avoid some of the "problematic reifications that plague the rest of IPE" for example around "the political, the state, and the international" (Elias and Roberts 2018b, 2). For many feminist IPE scholars, this diversity is evidenced in three ways. First, feminist IPE is an interdisciplinary enterprise that goes beyond established disciplinary boundaries with multidisciplinary origins that are much wider than IR broadly defined; second, feminist IPE has a broad empirical scope; and third, feminist IPE's development has been informed by different feminist methodological and theoretical traditions. So we could more rightly be talking about *feminisms* in IPE rather than a singular feminism. We will consider each of these characteristics in turn.

Feminist IPE's Methodological and Theoretical Traditions

Feminist scholarship as a whole has long been recognized for the pluralism and heterogeneity of the approaches and methods used to reveal unequal power dynamics, as well as its shared commitment to challenging masculine biases in knowledge construction (Ackerly and True 2013). Feminist IPE is no exception. The methods, whether quantitative or qualitative, used by feminist IPE scholars vary, depending in part on their theoretical approach and the questions they are addressing.

Like feminist scholarship in general, contemporary feminist IPE has roots in the pioneering feminist scholarship of differing intellectual origins and theoretical beliefs in a range of disciplines, including feminist economics, development studies, geography, sociology, and anthropology as well as international relations and political science. In particular, the early structuralist analyses of socialist feminists (indebted to Marxist theorizing) have

been centrally important to the development of feminist IPE. From the 1970s onwards socialist feminists highlighted Marxism's sex blindness due primarily to its concentration on class (Hartmann 1979). They argued that using a gender lens illuminates the often-obscured centrality of women's oppression to capitalism and the global political economy, that contributes to capitalist relations of production. For socialist feminists, these relations are often ones of gendered as well as classed (and racialized) inequalities. Socialist feminists also highlighted how processes of social reproduction in both the household and in the public sphere played a fundamental role in maintaining those capitalist relations of production. As we will see, debates around the importance of social reproduction continue to have salience.

Liberal feminism too, has had a role in the development of feminist political economy. The women in development (WID) approach has been particularly influential, emphasizing that it was women's lack of proper access to the essentially beneficial processes of (capitalist) modernization and development that marginalized them economically and socially, increasing their dependence on men (Jaquette 1982). Ester Boserup (1970), for example, operationalized these arguments for agricultural development, particularly in Africa, highlighting the negative impact of women's exclusion from cash crop production. The policy implications were that women had to be brought more effectively into processes of (capitalist) development. Again feminist IPE debates about policy proposals emanating from this liberal feminist framework continue to this day.

The subsequent development of feminist theorizing in feminist IPE has also mirrored the development of feminist theorizing more generally. From the 1980s, feminist theorizing became more sophisticated and complex as poststructural, postcolonial, and intersectional feminisms, together with a greater focus on sexualities and body politics, became increasingly important. These new approaches undermined simplistic notions that it is possible to talk of "woman" as unitary and a historical category, rendering the concept of "women's interests" untenable in the uncomplicated ways often assumed by many second wave feminists.

In particular, postcolonial feminism challenged much white Western feminist work, arguing that their analyses were imbued with racist and ethnocentric assumptions characteristic of a universal over-homogenizing feminism that generalized white feminist experience to black women, ignoring their concerns and turning women from the Global South into a non-Western "other" (hooks 1981; Mohanty 1988). These critiques formed part of an intersectional feminism pioneered by Kimberle Crenshaw (1989). An intersectional approach recognizes that an understanding of multiple and intersecting sources of oppression such as race, gender, class, sexuality, and disability must be central to feminism and feminist analyses. For example an intersectional feminist IPE analysis of the 2007/2008 global financial crisis highlights that poor African American women were significantly more likely to have been sold unsustainable mortgages in the US sub-prime mortgage market that played such an important part in the crisis (LeBaron and Roberts 2010).

At the same time, as part of the move away from the materialist emphasis that characterized socialist feminism, poststructuralist feminism also highlighted the importance of identity and the role of language and discourse in the construction of differing identities, even if, as Griffin (2018) argues, poststructuralism made little headway in IPE as a whole. Poststructuralist feminist IPE scholars deconstructed neoliberal discourses of globalization and restructuring to reveal their gendered dynamics (Griffin 2009). Finally, issues surrounding sexuality, intimacy, and body politics also became increasingly central,

encapsulated in queer theory and the recognition of non-normative sexualities as well as dominance of heteronormativity (Bedford and Rai 2010). Kate Bedford (2009), for example, examined how heteronormative assumptions about household and families impacted on the activities of the World Bank.

The Empirical and Analytical Scope of Feminist IPE

These theoretical approaches and insights have enabled feminist IPE to show empirically how gender and gender relations, often ones of exploitation and inequality, are key to understanding many aspects of the contemporary global political economy, whether it is global flows of goods, services, and people; global economic governance; as well as finally how the most recent feminist IPE theorizing can improve IPE's analyses of the contemporary global political economy. We will consider each of these in turn.

Much feminist IPE has examined the increasing global flows of goods, people, and finance, or what Bedford and Rai (2010) have termed the gendered regimes of production and consumption and the gendered systems of exchange associated with globalization. Four decades ago, Diane Elson and Ruth Pearson (1981) highlighted how much export-led production, seen as the engine of economic growth in East Asia, was based on the employment of women in the new factories. Industries such as garment production and electronics relied on young women's labor, utilizing their "nimble fingers"—considered an innate characteristic rather than a socially learnt skill—together with their supposed docility, both traits associated with certain constructions of femininity. Similar trends have been identified in the growth of production of high value agricultural exports such as fruit, flowers, and "exotic" vegetables for consumption in the Global North (Barrientos 2019).

Feminist IPE scholars have also used a gender lens to examine processes of migration, illuminating how men and women migrate in different ways, for different reasons, and to different forms of employment. For example, as the COVID-19 pandemic has highlighted, predominantly poor, often women of color from the Global South and other peripheral areas, migrate to provide care, often on an unregulated basis, within households as domestic servants in the private sphere or in hospitals and care homes in the public sphere, as part of global care chains (Kofman 2004). In contrast men more often migrate to work in industries such as construction. The hugely important financial sector and increasing financialization of many aspects of life, including the household, have also been interrogated by feminist IPE scholars. Griffin (2013), for example, has analyzed how the financial services industry is a deeply masculinized sector in which particular masculinities (often dubbed "hegemonic") predominate. After the global financial crisis of 2007/2008, feminist IPE scholars problematized how the crisis was gendered, dissecting some of the dominant explanations and solutions, including the popular discourses of "Lehmann Brothers versus Lehmann sisters" and "women to the rescue" (Prugl 2012; Hozic and True 2016).

Feminist IPE has also interrogated global economic governance. Initially scholars focused on two areas—the policy outcomes of international feminist institutions (IFIs) and the gendered ideas/discourses and narratives of global economic governance. By the end of the 1980s gender scholars had started examining IFIs' general policy prescriptions. Most notably they promulgated important gendered critiques of the neoliberal structural adjustment programs (SAPs) implemented by the International Monetary Fund (IMF) and World Bank (Bakker

1994; Elson 1995). From the 1990s onwards, as the World Bank's gender policy interventions—primarily gender mainstreaming—became more commonplace, they too, were critiqued by gender scholars as primarily technocratic rather than transformative, as well as under-resourced and largely ineffective (Bedford 2009; Griffin 2009; Moser and Moser 2005).

At the same time, feminist IPE scholars began to analyze an important narrative of what gender equality meant to IFIs like the World Bank developing within global economic governance. It crystallized in the “business case for gender equality” or “gender equality as smart economics”—namely that economically empowering women, for example through increased labor market participation or increased access to financial services, was good for economic growth (Roberts and Soederburg 2012). This discourse of gender equality as smart economics became even more widespread after the global financial crisis when it seemed to offer policymakers a partial solution to economic stagnation and recession that followed the global financial crisis.

Feminist IPE scholars argued that the World Bank's “gender equality as smart economics” discourse did not emerge in isolation, but is a gendered dimension of the broadening of global economic governance to include a wider range of actors and institutions. Roberts (2015, 115) outlines how a loose constellation of forces, including corporations, banks, and financial firms like Coca Cola and Goldman Sachs, often in partnership with IFIs, converged to promote a gender equality agenda. Dubbed transnational business feminism, feminist IPE scholars have investigated public/private partnerships such as Nike's Girl Effect Initiative (Calkin 2016; Prugl and True 2014). Feminist IPE critics argue that, with echoes of the liberal feminism of the 1980s, transnational business feminism co-opts feminist ideas for predominantly instrumental purposes (Prugl 2015). Others go further, arguing that transnational business feminism is a new form of neoliberal feminism, co-opting a version of feminism to disguise the continuing domination of neoliberalism (Fraser 2009).

Recent Theoretical Developments

Bringing together all the insights of feminist IPE, over the last decade important new theorizing by feminist IPE scholars has increased our understanding of the global political economy in the related areas of social reproduction, the household, and the everyday. This work challenges further the states and markets approach which has dominated so much mainstream and critical IPE (but which Bedford and Rai (2010) argue cannot answer Enloe's question: “where are the women?”). Building on its socialist feminist roots and the feminist analyses of the public/private divide described earlier, recent feminist IPE work on social reproduction has explored in greater depth how gendered and racialized hierarchies have been (re)produced at various sites and scales in the maintenance and development of capitalism (Luxton 2018). For example, extending Elson's analysis of the neglect of social reproduction, Rai et al. (2014) argue that the three main sites of social reproduction—individuals, households, and communities—can experience a serious depletion when a critical gap opens between the domestic, affective, and reproductive outflows and the inflows that sustain health and well-being. This depletion in social reproduction results in gendered harm as individuals' physical and mental well-being decreases, and the sustainability of households and communities declines (Rai et al. 2014, 87). As a result, processes of depletion increase economic vulnerability and in times of crisis exacerbate the social costs of market failure.

The feminist IPE work on social reproduction complements the recent interest in IPE more generally on the everyday and the localized cultural practices that sustain (or subvert) the ongoing processes of marketization and neoliberalization (Elias and Roberts 2016, 788). Indeed Elias and Roberts (2016) argue that this feminist scholarship predated and paved the way for the increased emphasis on the everyday in IPE, even if much of this subsequent work has remained gender blind. Crucially, examining this together with social reproduction, identifies and analyzes the structural links between the global political economy and the everyday. It is not possible to understand either global governance or the everyday without the other, as everyday gendered social relations and gendered power relations are co-constitutive. Gendered analyses of social reproduction are therefore crucially important, even if they remain a missing element of mainstream IPE's analyses of the everyday and the global political economy more generally. Finally, in dialogue with, and as part of, the development of important queer and decolonial critiques of IPE, feminist scholars are queering social reproduction and the history of capitalism, as well as ensuring that the study of postcoloniality and racial capitalism takes on board social reproduction and queer politics (Bhattacharyya 2018; Gore 2021; Rao 2020; Smith 2020). All these developments significantly enhance both feminist IPE and IPE as a whole.

CONCLUSIONS

Feminist IPE has developed hugely over the last two decades into a vibrant and flourishing interdisciplinary and pluralist body of scholarship that gives us important insights into many aspects of the global political economy. This chapter has shown that, despite the important strides made by feminist IPE, a lack of engagement by many IPE scholars continues, and argued that this lack of engagement has harmed the field of IPE as a whole. However, there is evidence that there are now greater opportunities than ever for interchange. The decline in the dominance of the two camps and the growing heterogeneity of the multiple niches and networks in IPE (epitomized, for example, by the large body of scholars and work identified as constructivist) could and should facilitate more attention to feminist scholarship. The increased interest within IPE in the everyday, the role of the household, and social reproduction and how these are raced, classed, and gendered—and whose importance has been so vividly demonstrated in the COVID-19 pandemic—also offers the prospect of greater collaboration, particularly as more scholars identifying as “critical” now include social reproduction as an important element of their analyses.

NOTES

1. This figure had risen to 33% by the 2014 more global TRIP survey.
2. See the various TRIP surveys for details of this.
3. RIPE was established in 1994, NPE in 1996, and IO in 1947.
4. I realize that there are likely to be many other articles with content that would count as feminist IPE but for this purpose, I was looking for articles that explicitly signal that they are feminist IPE through the use of the two key words—gender/feminism.

5. I estimate that RIPE published around 750 articles in total from 1994 to 2020 and so 14 would comprise around 1.87% of the total.

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CHAPTER 7

RACISM AND PATERNALISM

J. P. SINGH

IMAGINING the “real world” shapes the theories and practices in all scholarship. The Introduction to this handbook cites Stephen Krasner who notes that “the study of international relations has never been divorced from contemporary events,” though there are often disagreements on the meaning of contemporary (or past). International Political Economy (IPE) emerged as a sub-discipline to explain the globalization and interdependence processes in the postwar era. As the international liberal order deepened, IPE was at the forefront with its theory of international regimes explaining its norms, rules, principles, and decision-making procedures, and the role of hegemony (Krasner 1983; Keohane 2005). Susan Strange (1982, 479) criticized IPE’s fascination with such regimes as a “fad,” “imprecise,” “value-biased,” “static,” and “narrow.” Neither Strange nor Krasner doubted that IPE does not track the current contours of the world economy, but there was a crucial difference. Stopford et al.’s (1991) approach sought to change the historical status quo to lessen power asymmetries between the North and the South through diplomacy and other means. Krasner (1985) viewed postcolonial demands such as a New International Economic Order as seeking an authoritative non-liberal re-allocation of resources. The implication was to manage the South rather than questioning preponderant power.

The preceding paragraph alluded to a period of theoretical disagreement rooted in interpretations of history. This chapter tracks a burgeoning agreement in IPE over another historical blind spot in post-Second World War international relations, namely the racism and paternalism that the Global South has experienced, which did not become a major issue of concern in IPE until recent “current events.” This is surprising because International Relations (IR) as an academic discipline came of age at a time in the 1930s and 1940s when issues of race were at the forefront of global politics. *Racism* was introduced to the English vocabulary in the 1930s and referred to anti-Semitism and the *Judenrein* project directed at exterminating Jews from Germany (Rattansi 2020, 4). Colonial writers pointed out the antecedents. Aimé Césaire wrote: “He [Hitler] applied to Europe colonialist procedures which until then had been reserved exclusively for the Arabs of Algeria, the ‘coolies’ of India, and the ‘niggers’ of Africa” (Césaire 2001, 36). A few decades after Césaire’s original work, Said (1978) provided an exhumation of these “procedures” as they

developed through Western intellectual traditions during the colonial and postcolonial era. Robert Vitalis (2015) documents the intellectual history of the “birth” of IR to show that rather than overlook questions of race, White scholars in the US consciously marginalized such questions and intellectuals, such as the Nobel Laureate Ralph Bunche, who tried to ask them.

While a select few IR scholars analyzed racism in the postcolonial era, the current interest in racism is mostly due to developments in the global political economy since the 1990s. As with the 1930s, race became an important subject of study when its manifestations grew important in Western political economies, though racism is not limited to the Western world (see, e.g., Twine 1998; Pandey 2013). Of particular note here is the treatment of the “other”—meaning the immigrant or minority population—in the Western world. Roxanne Doty (1993)’s seminal essay on race in IR, begins with two motivations: (1) violence against immigrants from the Global South in Europe, and (2) the fact that IR was behind other social sciences in its consideration of race issues. Doty (1993) is careful to point out precursors in IR scholarship, from the work of William Du Bois in the 1910s to that of James Rosenau in 1970s, that dealt with issues of race. Nevertheless, these were isolated endeavors in the annals of international relations replete with terms such as power and interdependence (e.g., see Keohane and Nye Jr. 1977).

Since the 1990s, IPE scholarship on race has tracked the rise of far right and populist politics in Europe and North America. However, a full-fledged reckoning with race issues awaited recent events such as the 2016 Brexit vote in the UK, and the election of Donald Trump as President of the US, when racist rhetoric and policies began to be rolled out from the top echelons of national governance unheard of since the 1930s.¹ This has led to an exhumation within IR and IPE for the neglect of race and racism in the disciplines (Zvobgo and Loken 2020; Singh 2020b).

The rise of sociological perspectives in IR and developments in economics since the late 1980s also provides theoretical tools for “noticing” race. IPE had largely accepted the tenet of neoclassical and Marxian economics that interests and preferences of human actors are shaped through material conditions and incentives. The dynamics of Marxian economics explain change through the relationship between modes of production and class interests. Neoclassical economics holds incomes, tastes (culture), and technology as given while observing the effects of preferences in economic behavior (Singh 2020a). Neo-classical trade economists endogenized technology or made it internal to the preference formation of firms starting in the 1980s. Borrowing from sociology, theorists began to speak of adaptive preferences (Elster 1982), preference falsification (Kuran 1987, 1997), and meta-preferences to reveal social pressures and cultural influences in preference formation (Throsby 1994; George 2001). International relations did the same with the underlying sociology of preference formation. In the last decade, a few IPE theorists have outlined socio-tropic preferences (Mansfield and Mutz 2009). Collectively, sociology and economics opened a window to examine the non-material sources of interest formation or their intersection with them. Scholarship examining the intersectionalities of race, class, and gender are an example (Gill and Mittelman 1997; Chowdhry and Nair 2003).

While the bulk of the following essay deals with current issues of race in IPE, the next section² further details IR’s silence on race until recently to provide a context. Subsequently, four subsections outline current thinking on issues of race in trade, international development, foreign aid, and immigration studies.

RACISM IN INTERNATIONAL RELATIONS

Racism is a set of social beliefs that hold groups of people as inferior to enable discriminatory practices. Racism in international interactions may be overt or latent: beliefs that assign superiority to one group of people may result in security, economic, or human rights practices ranging from overt condemnation and discrimination to being paternalistic and infantilizing developing countries while appearing to be charitable. While most of the literature thus far focuses on the West, racism can be found anywhere—within India's caste system or Chinese racism toward Africans. While global racism is critiqued and analyzed (Evrigenis 2007; Kaufmann 2018; Norris and Inglehart 2019), the discipline's history also shows oversights entailing the hesitance and inability to address race issues. Reckoning with the shortcomings and the strengths of current perspectives is instructive toward presenting analyses and solutions.

Ironically, a history of the discipline provides a remarkable obsession with race questions, albeit from a position of superiority in the West, at the beginning of the twentieth century to their near absence at the end of it. Intellectual histories note that until the early twentieth century, international relations meant international race relations (Vitalis 2015). Most scientific studies in Europe assigned an inferior status to colonized people, believing racial differences to have a biological basis. The dominant strain in international governance was of civilizing the colonized (Shipman 2002; Crawford 2002). A predecessor to the journal, *Foreign Affairs* was known as *Journal of Race Development* between 1910 and 1919, though it did feature anti-racism contributors such as W. E. B. Du Bois (Henderson 2013).

During the interwar period, academic writings in IR did not address racism even during the Nazi era. The general blind spot reflected the budding discipline's focus on describing international interactions on a pendulum between power and idealism and a bias against noticing racial dimensions that had been entrenched in Western thinking since the 1930s (Carr 1964; Hobson 2012). Foreign policy practitioners also delivered. Woodrow Wilson's idealism cleverly disguised his fear of the colored races that had to be checked through an international order. Cordell Hull, FDR's Secretary of State and a chief architect of the postwar liberal international order, also offers a glimpse in his memoirs: the two volumes hardly mention the nationalist movements in the colonies, and Hull may have privately believed that civilization belongs to the Europeans (Hull 1948). Meanwhile other biological and social sciences debated race (Barkan 1992; Appiah 2015). These carried over into organizations such as UNESCO in the postwar era. Anthropologist Claude Lévi-Strauss led the UNESCO studies on the race question (Lévi-Strauss 1952).

In the postwar era, international relations also overlooked notable public scholarship on race from leaders such as Steve Biko, Amílcar Cabral, Aimé Césaire, Franz Fanon, Mahatma Gandhi, and Martin Luther King. The practice of US foreign policy borrowed its vocabulary from realism that "transcended" racism (Morgenthau 1967) and devalued anti-colonialism as merely a triumph of Western liberal ideals (Hobson 2012). While many intellectual architects of US foreign policy had fled Nazi Germany, they viewed containment and power as the response to horrors like Nazism (or, later, the Soviet Union) and were disillusioned with Enlightenment idealism (Molloy 2000). An exhumation of the postwar liberal international order now reveals racialized fissures. Historian Mazower (2012) explains that many

creators of the postwar liberal order, including Jan Smuts of South Africa, were well-known racists and created the United Nations (UN) to continue the work of “civilizing inferior races.” UNESCO was not an outlier: its first Director-General Julian Huxley was a eugenicist, a pseudo-science that could be traced to white anxieties about loss of status in the 1930s (Singh 2011).

International institutions with racialized origins need not continue to be racist, but IR needs to show how these organizations overcame racism (counterfact is the bedrock of sound reasoning). Decades after the Second World War, the main axes of IR models were power and interdependence, but these “Western” concerns disguised an underlying racism, one in which the postcolonial states were to be “provided for” and never deemed equal even in international organizations such as the UN where each country had one vote.

INTERNATIONAL TRADE

There is a disconnect between colonial era histories of racist and violent exploitation, and the postwar writing in IR that seldom refers to race. A brief history of these elisions from mainstream IR and IPE writers is important.

The exploitative nature of European trade with its colonies is frequently acknowledged in the literature. Imperialism provides the clue to understanding the racist underpinnings of this exploitation, which included forced slavery and keeping production patterns in the colonies in a “primitive” agricultural state. Human bondage and resource extraction were ruthless drivers of colonial exploitation. 300,000 slaves were shipped from Africa to the Caribbean and Latin America in the sixteenth century, and this number was 6 million in the eighteenth century (Acemoglus and Robinson 2006, 251). The growth of anti-slavery movements in Europe and the civil war in the US are often credited for the abolition of slavery, for example in Britain in 1833 (Crawford 2002). There may have been another reason, namely: discovery in Europe that sugar could be home-produced from beet, which meant that slave trade for the British to its colonies had effectively ended by 1807 (Mintz 1985, 68). Persaud (2014) provides a stark history of racist, gender, and sexual violence that accompanied sugar plantations in the Caribbean. Sugar is but one instance of colonial “trade” relations. In general, the colonies were primary commodity producers and industrialization was discouraged. After slavery ended, the surviving sugar plantations continued the exploitative path dependence and doubled down on human bondage and exploitation. Brennan and Packer (2012) make the case for reparations to the developing world for the racism and human rights violence that accompanied the sugar trade.³

The continuity with exploitative colonial practices in the postwar trading order can be seen in the discussion over imperial preferences in the negotiations that eventually led to the postwar trade organization, the General Agreements on Tariffs and Trade (GATT) in 1947. Imperial preferences were tariff reductions between Britain and its colonies. The issue was one of the most difficult to negotiate between the US and Britain in the lead-up to GATT. There was an economic basis to British interest in preserving what the Atlantic Charter of 1941 called “existing obligations”: in 1938, nearly 50 percent of British exports and 40 percent of the imports were with the colonies (Feis 1945). The Tories also viewed these preferences as a continuation of the empire (Irwin et al. 2008). The Americans resisted

imperial preferences because they traded distortionary but they were not that interested in colonial questions.

While Britain demanded imperial preferences in trade, the developing world's major interest was in getting infant industry protections to jumpstart manufacturing and to overcome being primary commodity producers. The developing world received begrudging support from the US and the UK for infant industry protections, with provisions only for exceptional circumstances. Many countries sent highly educated Keynesian economists or diplomats, such as Uruguay's Julio Lacarte to these trade talks. The American negotiators rebuffed their infant industry demands and the American negotiator Clyde Wilcox wrote in a confidential memo: "The Indians came with a chip on their shoulder. They regarded the Proposals as a document prepared by the U.S. and the U.K. to serve the interest of the highly industrialized countries by keeping the backward countries in a position of economic dependence" (quoted in Irwin et al. 2008).

After GATT's creation, the story of North-South trade relations took on a paternalistic flavor in which a knowledgeable colonial power "infantilized" and seemingly looked after the former colony, positing imperial preferences as a concession. In practice, the colonial power primarily preserved its trading privileges, namely cheap raw materials for Europe. When negotiations commenced between the burgeoning European Economic Community (EEC) and Africa, Ghana's President Kwame Nkrumah called imperial preferences "collective neocolonialism" and "collective imperialism" that preserved Europe's indirect rule over the colonies (Zartman 1971, 21). Eventually these preferences started to be enshrined in international trade law through non-reciprocal tariff reductions and a limited amount of quotas for the developing world's products through trade measures such as the Special and Differential Treatment through GATT that was in place by 1971. The developing world is often posited as asking for these paternalistic privileges, especially through the figure of Argentinian economist Raúl Prebisch who founded UNCTAD in 1964 to assist the developing world in correcting its adverse terms of trade. The story is complicated. Many developing countries continued to view GATT as the chief forum for their trade interests and UNCTAD as a way to bring pressure (Hudec 2011; Scott and Hannah 2017). While Prebisch advocated for mechanisms that led to Special and Differential Treatment (SDT) in the 1960s, the underlying political economy arose from the need for economic diversification and declining terms of trade for the developing world (Margulis 2017). Further SDT was debated and also generated controversy in the developing world. The developing world's advocacy was for market access and infant industry. Many trade economists from the developing world denounced SDT as breadcrumbs and an instrument that would cast the developing world in an inferior status (Srinivasan 2019).

Paternalism is "a patronizing discourse resulting from a position of economic and political strength and cultural distance" from the developing world (Singh 2017, 1). It assigns benevolence and oversight to the paternal powers while turning the colonized into supplicants. Paternalism is sweet talk atop underlying racism. My analysis of paternalism in North-South trade relations found through quantitative and qualitative (process tracing, structured focused comparisons) that paternalism is strongly correlated to trade concessions across the entire gamut of trade issues from agriculture to high-tech services. Further, a content analysis of 1,465 USTR press releases from 1982–1993, roughly the beginnings and end of a multilateral trade round, showed that 93 percent of the paternalistic references the US made were toward the developing world. Paternalistic statements from the developed world are

moralistic and preachy, touting benefits of measures such as SDT exhorting the developing world to open their markets while effectively keeping developing country exports out of these markets. Another aspect of paternalism is not acknowledging the developing countries as equal even when they negotiate hard. Amy Skonieczny (2001) offers excellent content analysis of the media reports on negotiations leading up to the approval of the North American Free Trade Agreement (NAFTA) with Canada and Mexico in 1993 showing that Mexico was portrayed as discursively inferior and as a “dependent other” (451). Alexandra Guisinger (2017) shows that support for trade protectionism in the US often has a racial component: as in American discourses about social welfare, those deserving of trade protectionism are often portrayed as “White.”

An important contribution of the literature examining the links between race and trade is the exploration of trade policy preferences at social and cultural levels. A notable piece of this scholarship comes from Mansfield and Mutz (2009) who argue for socio-tropic preferences. They find high degree of support for protectionism among those who are nationalistic or xenophobic. Peter Navarro’s book on China is a testament to the way xenophobia affects trade preferences not in the empirically erroneous claims made against China but in the realization of those claims in American trade policy a decade after the publication of the book when Navarro became an advisor on trade policy to President Trump (Navarro 2006).

INTERNATIONAL DEVELOPMENT

Issues of international development within IPE attract scholars from several social sciences. Many work at the intersection of international and comparative political economy, and conduct field research in particular regions or countries. A point of convergence for these scholarships is the study of institutions at the domestic and international levels to show how they have affected development, variously understood from measures such as economic growth to broad notions of human well-being. Institutions include formal organizations such as international organizations, governments bureaucracies, or political parties. They may be abstractions such as class, patriarchy, or state. Liberal institutionalists study the lack of formal and informal institutions that further development, while critical and radical theorists often emphasize factors such as class or international division of labor that distort “local” or “domestic” development. Neither set of scholars grappled with the question of race in examining institutions until fairly recently.

The intellectual history of Eurocentric conceptions of politics referenced above applies equally to understanding the blind spots in development. Liberal institutionalist theory carried Eurocentric conceptions of the preferred path to development, namely the modernization approach, and ignorance or underappreciation, despite extensive field research, of the international context that would thwart industrialization and modernization even if developing countries had agency to move in that direction. Modernization theory derived its rationale from the European industrialization experience, best captured in Rostow’s five stages of growth from a “traditional society” to the “age of high mass consumption” (Rostow 1960). Lerner (1958)’s *Passing of the Traditional Society* was another classic: mass communications such as radio and urban industrialization were the solutions to overcome tradition viewed in negative terms. Political scientists took up the stages of growth argument

with an eager zeal and began to outline political modernization. Gabriel and Coleman (1960) were careful to show that traditional and modern elements coexisted even in the most industrialized states, and Huntington argued for an appreciation of the cultural context (Huntington 1971). However, the Eurocentricity was baked in: to develop, all societies must modernize and the West was far ahead. When there was an appreciation of the local cultural context, it was merely to show how backward it was: this spawned writings in that period about the slow Hindu rate of growth or the Confucian “mindset” that was deemed anti-modernist (Nayar 2006).

Political modernization theorists were blind to the international context that thwarted economic diversification or political institutionalization in the developing world. The moves from developing countries for infant-industry protections pushed back at the international level were mentioned above. By the 1960s there was a mountain of evidence showing that the terms of trade were arrayed against the developing world and its industrialization: an important group of economists showed how tariff differentials favored raw materials and primary commodity exports from the developing world but any manufacturing value-added resulted in higher tariffs for their exports (General Agreement on Tariffs and Trade 1958, Balassa 1965). These international trade strategies kept the former colonies poor and in the agricultural fields. Further, many of the “Third World dictators” around the world, that modernization theorists decried, were propped up to support the West in the Cold War. Finally, there was an international developmentalist mindset from agencies such as the World Bank that put itself in a paternalistic position of advocating what was best for the developing world regardless of local contexts.

The racialized underpinnings of economic and political modernization strategies were laid bare in other social sciences. Imagining three-quarters of the world population, formerly colonized, as docile and awaiting superior knowledge from the West reeks of Orientalism (Said 1978). Anthropologists critiqued the modernization narrative for assuming that there were “backward societies” and “underdevelopment” and that populations were in need of the strategies advocated from technocrats housed in international institutions who propagated the narrative to the “native” governments (Escobar 1995; Ferguson 1990).

Racism in these approaches is multilayered and pathological. Paulo Freire (2018) describes the pathology of the oppressed as lacking the cultural voice to name their world, and the relationship with the oppressors in sado-masochistic terms. The modernization approach was ahistoric and defective, but even those countries that chose this path faced roadblocks from the West while the latter preached the benefits of the very phenomenon they blocked. Modernization was the desired goal but the West effectively thwarted infant-industry strategies and manufactured exports from the developing world. In current writings, systemic racism refers to phenomena where the discrimination is deeply situated and diffuse so as to be barely noticeable. However, the noticeability was ever present: from radical writers such as Steve Biko and Frantz Fanon who pointed out the psychological aspects of racist oppression, to neoclassical economists who deepened the unequal terms of trade arguments.

The radical and critical theorists, especially the dependency school, also pointed out the core capitalist (colonial) processes that distorted development in the periphery (Cardoso and Faletto 1979). Most of these analyses ignored race until IPE theorists began to show that imperialism and postcolonial capitalism have a racist underbelly in the exploitation of labor (Persaud 2014; Chowdhry and Nair 2003; Anievas et al. 2014). Several of these theorists also hold up the gendered dimensions of racist exploitation. Despite the intersectionality

inherent in claims about class, race, and gender, most of these analyses are materialist conceptions of history in which class is the primary explanatory factor.

Paternalism is now widely critiqued and denounced in development studies. Two types of literature are symbolic of this move. Postcolonial studies that include many IPE scholars have forwarded an agenda of decolonizing the imagination of development including questions of race and paternalism (McEwan 2018 offers an excellent introduction]. Second, ideas of participatory development provide a countermove to top-down paternalism (Mansuri and Rao 2013).

FOREIGN AID

Scholarship in IPE on foreign aid investigates both donor interests and effects of foreign aid (Alesina and Dollar 2000). The literature on the effectiveness of foreign aid dominated these debates until the late 1990s, but the recent literature on donor interests demonstrates interesting empirical claims on three counts: colonial ties, paternalism, and strategic interests. Colonial ties and strategic interests are not surprising and easily empirically variable. For the purposes of this chapter, the paternalism claim warrants further detail.

Two representative works provide some depth to the claims for or against paternalism or racism in foreign aid. On the former, Baker (2015) demonstrates using experimental methods that foreign aid is correlated with perceptions of the racial other, seen to be an inferior position. Baker's results show that when shown pictures of people who were Caucasian or Black, but in similar economic positions, people are more likely to give aid for the latter. These results find confirmation in other studies (VanDeVeer 2014). The results also highlight the "moral chip on the shoulder" that donors often wear about the recipients of foreign aid: for example, while the official development assistance from the US is about 1 percent percent of the GNP, the median answer in a poll from the University of Maryland was 25 percent (the Borgen Project n.d.). The case for the underlying paternalism has long been denounced in economics from the early studies of Peter Bauer et al. (1976) in the 1950s to current ones such as that from William Easterly (2013). In the last decade the racism and paternalism in foreign aid was highlighted in a popular book from Dambisa Moyo (2009).

Michael Barnett carefully documents the case *for* paternalism (Barnett 2011, 2012). In this case, paternalism speaks to the caring for people across borders with a history of interventions dating back to the nineteenth century. Barnett's case intersects with scholarship, which shows that ethical and humanitarian considerations matter in global politics (Klotz 1999; Crawford 2002). Barnett is aware that paternalism can easily be correlated with racism and neocolonialism (Barnett 2020) and documents the pathologies that inform international organizations in tasks such as humanitarianism and development that they take up (Barnett and Finnemore 1999). He also addresses the religious origins of paternalism that have increasingly become secular (Barnett and Stein 2012). Nevertheless, awareness is not the same as counterfact for the evidence against the various ill-effects of paternalism, especially those beyond humanitarianism. The pro-paternalism scholarship also sits uncomfortably with views from realist scholars that posit benefits for the weak at the bottom of international hierarchies (Lake 2009; for a critique, see Singh 2017).

IMMIGRATION

Immigration issues in the Western world pointed IPE scholars toward making race salient in the discipline's queries starting in the early 1990s. Identity issues and a racial backlash also came to the fore after the signing of the Treaty of Maastricht in 1992 that created the European Union (EU), which began to allow free movement of people as a result of services liberalization. With the accession of East European states, some of the racism was directed at East Europe, epitomized in the fears surrounding the "Polish plumber" who would migrate to Western Europe and lead to labor surpluses and unemployment in low-skill services occupations. As the EU became stronger questions of identity conflicted with the vision of a legal order: "From Sarajevo's no man's land to Paris's suburbs, even our Kantian island Europe seems more than ever caught up in struggles about identity and their recognition rather than simple bargains over interests" (Nicolaïdis 2007, 693). The 2015 refugee crisis in Europe when Germany's Chancellor Angela Merkel allowed over a million Syrian refugees into Germany led to widespread fears and increased the voting share of Germany far right party AfD in the Bundestag and many of the *länder* elections. At the Brexit referendum in 2016, UK's far right parties and a few Tories brought up the specter of unfettered migration and refugees, especially in their visual images, as a reason for leaving the EU.

A few statistics offer context (International Organization for Migration 2020). The International Organization for Migration calculates that the number of migrants in the world increased from 173 million in 2000 to 272 million in 2019, an average increase of 2.2 percent a year. Of the 272 million, 83.6 million migrants are in Asia, 82.3 million in Europe, and 58.6 million in North America. The increase in numbers notwithstanding, the ratio of migrants to the total population in the world has remained stable at 2.2 to 3.5 percent of the world's population. In 2019, there were 50.6 million migrants in the U.S., 9.6 million in the UK, and 13.1 million in Germany.

Public debate about the "burden" of migration can often be one-sided and lack historical context. For example, the 50.6 million figure for migrants in the US can be analyzed in various ways. First, on current costs and benefits most studies document the benefits of documented and undocumented migrants to the US (Omelaniuk 2017). Second, among the historical "costs" in the US, 12 million indigenous people may have been killed between 1492 and 1900 in the areas that are now US. Only 600,000 of the 12 million Africans shipped to the New World were enslaved in North America (Wikipedia 2021). After killing and subjugating the indigenous population, the European migrants fought a civil war over the status of relatively few slaves from Africa.

Two strands of IPE literature deal with racism and migration issues. One tries to understand the attitudes and preferences toward migrants. Contrary to assertions about the threat migrants pose for residents' economic security, Hainmueller and Hopkins (2014) find that European attitudes toward immigrants in Europe are not related to the Europeans' economic or material standing. Instead, socio-tropic or cultural concerns shape preferences. Rosenberg's migration deviation descriptor shows that the actual number of migrants from majority Black states are far less than would be expected if racially blind measures were used (Rosenberg 2019). A second strand of literature, at a broader level, correlates the rise of populism to the fears stoked by politicians about the migrants. The most notable studies

are those from (Norris and Inglehart (2019); Inglehart and Norris (2016)). Each documents the psychological fears, xenophobia, and racism directed at non-European migrants. Mutz (2018) also deals with the loss of status felt by White Evangelical men in the US that prompted the vote for Donald Trump in 2016. Miles Kahler (2020) presents the cultural war between the “cosmopolitans” and “parochials” as mediated though local demographic, cultural, and socioeconomic factors. Taken together, these studies move understanding in IPE beyond an obsession with material factors and distribution effects.

CONCLUSION

IPE scholarship has moved to consider race issues since the 1990s but especially after the global political tumults following the Brexit vote to consider questions of race. The 2015 Annual Convention of the American Political Science Association on the theme “Diversities Reconsidered” did not feature a single paper in its IR sub-sections that was on race or racism. International relations is no longer tone deaf to racism, especially as it examines the intersection of domestic and international politics, and includes racism beyond the Western world. Racism dominates politics in Western and non-Western worlds (such as Brazil, India, or Turkey). Historically, racism from China and Japan has been an important factor in patterns of domination in East Asia. Zoltán Búzás (2017) provides an excellent annotated bibliography on the extensive literature on race in international relations and in global politics everywhere.

Our debates on race need to concentrate on ontological blind spots, methods, and evidence. This entails an exhumation of the blind spots since the Second World War that kept issues of race and culture out of mainstream explanations and foreign policy endeavors. Neither IPE nor IR can overlook the Western civilizational codes that are embedded in their understandings.

Re-imagining the world is important for theoretical and empirical development. IPE is beginning to re-examine its models of preference formation to include cultural factors such as race and letting go of an over-reliance on materialist explanations of history. Another way for IPE to overcome its relative isolation from questions of race is to engage with other disciplines that have studied race rigorously. The case of international development scholarship is instructive in this regard with its new foci on issues of participatory development and decolonizing the narrative of development.

NOTES

1. US-led international events with racial dimensions include building a wall along the US–Mexico border, the trade war with China, and President Trump speaking disparagingly of Africa or calling COVID-19 “China virus” or “Kung Flu” (Wikipedia 2020). Similarly, the pro-Brexit campaign in the UK frequently employed racialized anti-immigrant imagery and rhetoric.
2. This section adapts text from Singh 2020b.
3. In general, the case for reparations from colonial exploitation is also made in popular literature (Tharoor 2018).

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CHAPTER 8

HISTORICAL MATERIALISM

ANGELA WIGGER

INTRODUCTION

THE current conjuncture, probably the most pervasive crisis in capitalism's history ever, challenges the discipline of International Political Economy (IPE) to rethink the ontological grounds of its theories and conceptual tools. Now, more than ever, IPE is tasked with asking "the big questions": global warming and the loss of biodiversity uncompromisingly remind us that the imperative of relentless capitalist growth cannot be realized within our planetary boundaries; and the COVID-19 pandemic has not only led to dramatic loss of human life and a public health crisis across the world, but also exposed and exacerbated multiple asymmetries and social inequalities. Global value chains and trade have been disrupted, and informal sectors, mostly in the Global South, have collapsed, leaving millions of people in dire straits about their future survival. At the same time, global debt levels have reached historically unseen records—debt levels which, in the context of the global recession and the costly pandemic relief measures, are heralding the advent of a crisis that is far more dramatic than that witnessed in 2008. Not only are big picture analyses urgently needed, we also need coherent visions for a systemic change.

The discipline of IPE has been struggling to engage with the big picture questions that lie at the heart of capitalism's multiple and looming crises. Although many IPE scholars are devoted to identifying "empirical" blind spots and finding new empirical alignments that contribute to eradicating social injustices tied to gender, race, and North-South divides (LeBaron et al. 2021), questions of ontology are frequently ignored. As will be shown here, without a sustained ontological enquiry into the contradictions of global capitalism, our understanding of the contemporary global political economy and transformative praxis remains limited. Grand theories that offer a comprehensive understanding of global capitalism and that are capable of remedying the absence of big picture research are few and far between. Historical materialism is probably the only theory equipped to analyze the totality of social life alongside the dynamics of global capitalism within its ontological realm. As a theory of history, a theory of power, and a theory of emancipatory praxis, historical materialism is capable of understanding the interplay of agents shaping and contesting the current capitalist order, the institutional strongholds enabling or hindering them, and the underlying ideational and material (production) structures from which these agents

emanate. While most other theories usually privilege either agency or structure and the ideal or the material, historical materialism seeks to cover all four ontological dimensions at once, without regressing into meaningless eclecticism, reductionism, or structural determinism. Importantly, historical materialism not only explains but also critiques the existing social order, and through exploring and elucidating the theme of human emancipation, it seeks to animate radical imagination about alternative futures. As Barry Gills (1987, 268) has noted, historical materialism “may in fact prove to be, intellectually if not politically, the strongest conceptual approach” as it comprehends and critiques society “on the basis of a holistic analysis of forms of political repression, forms of economic exploitation and the interaction of these forms in history.”

This chapter seeks to highlight the relevance of historical materialism for contemporary IPE scholarship and to demonstrate that historical materialism is an open and continuously evolving approach where critique from within and beyond its own theoretical confines has been a key stimulus for theoretical “progress.” To be sure, historical materialism is a broad church, fractionated into multiple schools and schisms. Although historical materialism usually subsumes Marxist or Marxist-inclined approaches, or Marxists who define themselves as historical materialists; not all historical materialist research is strictly Marxist (see Braudel 1980). Historical materialism is also sometimes labeled as “post-Marxist” (see the Introduction of this volume); however, when considering that Marx and Engels only laid the foundations which have been elaborated further by generations of theorists after them, any approach developed post-Marx and Engels is ultimately a reconstructed historical materialism. While there may be no such thing as a pure, prototypical, or authentic historical materialism, there are a range of ontological and epistemological premises that lay at its core. The first section of the chapter offers a primer of such key ontological and epistemological premises. The second section outlines how historical materialism theorizes capitalism and recurring capitalist crises. The third section contextualizes historical materialist research within the discipline of IPE and showcases historical materialist work where critique has spurred theoretical advancement, notably in the theorization of the state-economy and state-society relations, and the production-reproduction relation under neoliberal capitalism. The concluding section suggests dialogue and critique as the way forward for teasing out productive tensions and advancing the ontological reach of IPE scholarship, and in the spirit of emancipatory praxis, it reasserts the importance of radical imaginaries for an alternative, post-capitalist future. As a disclaimer, it should be noted that the chapter does not attempt to offer a canonical and genealogical “state-of-the-art” overview of historical materialist approaches, nor does it engage with internal and idiosyncratic debates that have taken place within historical materialism. For comprehensive overviews that go beyond the brief outline of basic theoretical premises, see Anderson (1976), Joseph (2006), Keucheyan (2013), Bieler and Morton (2018), or Callinicos et al. (2021).

WHAT IS HISTORICAL MATERIALISM? A PRIMER ON ONTOLOGY AND EPISTEMOLOGY

Historical materialism is based on a *material* conception of history and social power relations, which is rooted in an essentialist understanding of social reality: in order to

survive, humans need to produce and reproduce; after all, we all need food, clothing, shelter, sanitation, and much more (see also Bruff 2009, 247). These material and biological life requirements are satisfied through interacting with nature, and at the same time transforming nature. In the fulfilment of wants and needs, humans (and also some animals) use instruments and importantly labor, even if the instrument is just a hand and if the labor consists of picking an apple. Importantly, the necessary labor that guarantees human survival is not conducted individually but collectively, and how humans organize this collective endeavor leads to social power relations. A central premise of historical materialism is therefore that the collective modes of (re-)production of human life, in and through nature, create “the material basis for all forms of social existence, and the ways in which human efforts are combined in productive processes affect all other aspects of social life” (Cox 1987, 1). With the social relations of production as a key focus, historical materialism seeks to expose such relations even if they are not imminently visible. Produced goods and services may appear as neutral objects but they are in fact fetishized illusions inscribed by these relations. Historical materialism seeks to expose also the hidden relations between people that lie underneath the level of appearances. As Marx (1991, 956) reminds us, if everything was as it appeared on the surface, there would be no need for science, and historical materialism has the task “to appropriate the material in detail, to analyse its different forms of development, to trace out their inner connexion” (Marx 1996 [1867], 19).

The modes of production, and the social relations emanating from these modes of production, change over time and space, hence the *historical* in historical materialism. The past influences the present, albeit in a non-deterministic way. As Marx (2008 [1852], 398) famously stated in *The Eighteenth Brumaire of Louis Bonaparte*: “Men [*sic*] make their own history, but they do not make it as they please; they do not make it under self-selected circumstances, but under circumstances existing already, given and transmitted from the past.” Historical materialism assigns genuine importance to transformative agency, understood as overcoming and acting against social structures, rather than reproducing them. Agency can change the course of history and the future is perceived as open-ended. Even though structures cannot be changed easily or immediately in the foreseeable future, neither the realms of structure and agency, nor the ideational or the material are seen as fixed. Historical materialism leaves no room for a reductionist understanding of humans as mere bearers of a cause-effect relationship; agents are neither puppets on a string nor omnipotent puppet masters that pull all the strings (Archer 1995, 71). Historical materialism rather interrogates the dialectical interplay between the material conditions of social life over time and space, the prevailing and contesting ideas, and the agency articulating and acting upon such ideas in political struggles.

The emphasis on agency in dialectical interplay with material and ideational structures has ontological and epistemological consequences. It implies that dogmatic orthodoxies or a closed teleology in the form of pre-established logics or assumed, law-like generalizations, or universal truths are by definition ruled out. It implies furthermore that a “plug-and-play”-approach that gives primacy to mere theory testing, or what is sometimes also referred to as theoreticism, is not possible either. Marx himself repeatedly emphasized that he only provided some guidelines for conducting research and suggested the method of abstraction as a pathway to move from a concrete description of the social phenomenon to the abstraction of possible causes. As he argues in *Grundrisse* (1973 [1857], 101), “the method of rising from the abstract to the concrete is the only way in which thought appropriates the

concrete, reproduces it as the concrete in the mind. But this is by no means the process by which the concrete itself comes into being.” The method of abstraction involves the act of re-concretization with respect to the phenomenon under investigation, whereby abstraction does not refer to universal truths or the possibility to make generalizations. Abstract theory needs constant adjustment and re-evaluation in the light of a changing social reality (Cox 1986, 209) and is itself part of a specific spatiotemporal conjunction. Historical materialism is therefore never static and in itself genuinely historical.

By virtue of its holistic yet open-ended ontology, and its essentialist understanding of production and social power relations, historical materialism lies at the foundation of IPE as a discipline. Historical materialism is ontologically equipped to study the (re-)production of everyday life in every system throughout history; however, as the next section demonstrates, it excels in the theorization and critique of capitalism as a historically contingent mode of production and reproduction.

THEORIZING THE DYNAMICS AND CONTRADICTIONS OF GLOBAL CAPITALISM

Within historical materialism, deconstructing and demystifying the asymmetric social power relations arising from the dynamics and contradictions of capitalism as a historical mode of production takes center stage. Even though capitalism is perceived as dynamic, variegated, and historically contingent, a range of abstract and recurrent contradictions, structural violence, and crisis tendencies can be identified that can be transhistorical, and to some degree also objective to capitalism. The following unpacks a few ontological core dimensions that constitute the skeleton of a historical materialist understanding of global capitalism, notably the exploitation of labor and nature, class and class agency, surplus accumulation, competition and expansion, uneven and combined development, inequality and inbuilt crisis dynamics, and the role of the capitalist state.

In capitalism, production and the exchange of goods and services relies on fundamentally skewed social power relations: the vast majority of people sell their labor power in return for a wage while social reproductive labor is mostly unwaged, and a minority, owning the means of production, extracts surplus value from labor in the form of a non-compensation of labor time. Only through the exploitation of labor within and upon nature, can surplus value be produced and accumulated. The distribution of accumulated surplus and the social organization of capitalist production more generally give rise to opposing interests, and thus conflict. The concepts of *class*, *class agency*, and *class conflict* lie at the heart of capitalist social relations of production; yet, the crude dichotomy of exploiters (capital) and the exploited (labor) should not be understood rigidly, nor is class the sole category where we see a division of labor. To begin with, class is not a clear-cut seamless monolithic entity. Capital and labor are internally fractionalized in multifaceted ways alongside various axes and stages in the capitalist cycle, leading sometimes to contradictory interests and social antagonism and then again, unified positions—positions that are articulated and contested at the level of *ideas*, discourses, and ideologies. It is through the articulation of common positions, identities, and demands where a *class*

by itself becomes a *class for itself*, or what is referred to as class agency. However, under the imperatives of *capitalist competition*, which pits not only capital against capital and capital against labor but also labor against labor, unified class positions are difficult to achieve. Usually, class consciousness and political alliances across class fractions, and thereby a strategic orientation and purpose, arise only in confrontation with other social groups and classes (Van Apeldoorn 2004). Importantly, in addition to class, the accumulation of capital creates and thrives on various other power asymmetries. Forms of domination and repression also pertain to sex, gender, age, race, ethnicity, or people with different abilities or sexual preferences. The exploitation of labor taking place outside the immediate circuit of commodity production, trade, and finance, such as child, health, and elderly care, education, family life, and sexuality, is moreover equally constitutive to the accumulation of capital. As Cammack (2020) demonstrates, Marx also saw the various forms of social reproduction, including activities associated with the maintenance and re-production of peoples' lives on a daily and intergenerational basis, not as separate but as part of a historically evolving totality in the accumulation of capital.

The accumulation of surplus capital is furthermore never linear, exponential, or unproblematic but pervaded by a range of contradictions and ultimately crises. Accumulated surplus capital, or what has not been paid out in wages, ultimately needs to be reinvested to create more surplus capital, as without the relentless accumulation of surplus capital, capitalism would cease to exist. Such reinvestments take place in a competitive fashion, and those capitalists who accumulate more quickly usually tend to drive out of business those who accumulate at a slower rate. The competitive battle can intensify and trigger a further exploitation of labor and nature, which is why capitalist competition is essentially antagonizing. The presence of an industrial army, or surplus labor, consisting of latent, floating, or stagnant layers of unabsorbed labor, serves as a dynamic lever to discipline labor to accept intensified forms of exploitation. The competitive accumulation of capital forces the owners of capital, against their "ill or goodwill," to replace human labor with machines in search of greater productivity, a process which reproduces the reserve army of labor.

Capitalist competition also erects hierarchies of wealth and power within and across geographical regions. The imperialist division between core and periphery, or what Trotsky (1977 [1932]) coined as *uneven and combined development*, is both an outcome and a prerequisite for the continuation of capital accumulation. There are structural limits to renewal and the expansion of capital within a confined geographical realm. Whenever surplus capital cannot be recapitalized and thus reactivated through profitable reinvestment in the sphere of production, other possible outlets and thus new spheres of exploitation need to be found (Harvey 2006; Clarke 2001). What has also been referred to as the structural problem of overaccumulation constitutes a foundational and recurring crisis of capitalism, which can be overcome through enlarging capitalist logics to new geographical realms or through deepening capitalist logics through the commodification of ever more social relations. Capital usually flows where rates of return are highest, or where a stable income stream can be generated. Investments in fictitious capital, so-called interest-bearing assets that themselves are not productive, such as built environment or infrastructure, including land, real estate, and housing, or the sphere of financial circulation, also referred to as the rentier-type of accumulation, can provide such an outlet. As Marx (1975 [1847], 134–5) observed almost 170 years ago, "[t]here are even phases in the life of modern nations when everybody is seized with a sort of craze for making profit without producing." A crisis emerges when these

various forms of rentier-type of accumulation get overly out of sync with the accumulation in the production sphere.

Last but not least, historical materialism seeks to understand structural features of capitalist dynamics, such as its crisis tendency or uneven and combined development, as well as the various forms of exploitation, through an enquiry into the governance strategies of capitalist states. The accumulation of capital relies on states providing the legal prerequisites; however, states can only temporarily stabilize capital accumulation. States are never neutral arbiters but codify, legitimize, or represent social and spatial inequalities, and at the same time reproduce state power by guaranteeing the continued accumulation of capital. As an asymmetrical institutional terrain, states are however not unequivocally predisposed to the interests of prevailing capital fractions and can make material concessions to pacify opposing class fractions, including labor and other social groups.

This portrayal, albeit brief and incomplete, shows that historical materialism seeks to understand social power relations as an integral part of the nature and limits of capital accumulation within global capitalism. As the next section shows, rather than fitting into the disciplinary molds that are circumscribed by closed theoretical models, historical materialism, by virtue of its transdisciplinarity, openness, and its pretension to comprehend global capitalism holistically, is perfectly equipped to fill ontological voids and to conquer terrains that other IPE theories cannot.

HISTORICAL MATERIALISM IN IPE: (NON-)ENGAGEMENTS, DIALOGUES, AND CRITIQUE

The discipline IPE has long been overshadowed by Cold War ideologies whereby analyzing capitalism was considered hopelessly unscientific and imbued with a communist rhetoric. Rather than capitalism, the seemingly euphemistic and more prosaic term “market economy” came to prevail, or at best, capitalism was mentioned in passing only. As a result, the laws of motion of capitalism were generally not theorized outside historical materialism. Today, historical materialism forms part of the official canon of grand theories within IPE, or at least, no self-respecting textbook can sideline historical materialism as part of the standard theoretical repertoire. Notably the seminal work of Cox (1981, 1986, 1987, 1996) has been crucial for introducing the intellectual work of Gramsci into the field of IPE, and Gramscian historical materialism is often seen as “the most important alternative to realist and liberal perspectives in the field today” (Griffiths 2000, 118). The pioneering work of Cox has moreover leveled the road for a wide range of theories that have adopted the prefix “critical” within IPE since the 1980s, including feminist, reflexive, postcolonial, postmodern, or poststructuralist studies, and studies committed to a post-positivist epistemology more generally. Nonetheless, this does not take away that historical materialist research has long been marginalized, silenced, or simply gone unmentioned by the academic mainstream, and this continues to pervade academia as the spectre of theoretical pluralism is often confined to textbooks only. What also may be seen as a “constructive non-engagement” whereby historical materialist perspectives are tolerated as part of a division of labor in academia, is

however a great loss, as there is no other theory than historical materialism that is able to “conceive of capitalism as a global, gendered, racialized and ecological class antagonism in which struggles over wages and working conditions are organically linked to struggles over dispossession, social reproduction, ecology, imperialism and racism” (Pradella 2021).

One of the reasons for non-engagement may lay in the fact that historical materialism is often poorly understood and often misconstrued as “structuralism” and having a deterministic understanding of social reality, or it is caricatured alongside the political failures to establish communist regimes. Without doubt, historical materialism gives ontological primacy to social structures; after all, upon birth, humans enter a materially and ideationally pre-structured world. This should, however, not be mistaken with the absence of agency or the perception that historical materialist scholars always prioritize structures above agency (Knafo and Teschke 2020). Furthermore, historical materialist research is sometimes rebutted as normative or biased, and thus accused of lacking the necessary objectivity and scholarly distance to the research object (Cohen 2008, 92). These charges are often rooted in the belief that researchers can effectively distinguish between facts and values, and as such reflect the widespread misconception that perceives positivist epistemologies as synonymous with “normal” science (Kurki and Wight 2007). Historical materialism is not more or less normative than any other theory. Theories that do not question existing unequal social relations of power, implicitly or explicitly legitimize, reproduce, and reaffirm the position of predominant forces. Every ontology is normative, and also political, as there simply can be no objective or neutral theory, or research for that matter (Wight 2006). As Ferguson (2011, 12) has nicely put it, theories are like filters that select, eliminate, and highlight certain aspects of social reality, and thereby inevitably create and distort this reality; theories make it possible to move from the scale and detail of the empirical to the condensed, focused space; they are the price we pay for creating order.

The non-engagement may also find its roots in that historical materialist scholars often do not seek to push Marxist perspectives into the field of IPE, kicking and screaming. Historical materialism is not confined to the discipline of IPE but essentially transdisciplinary in nature, or what Jessop and Sum (2001) have labeled pre-disciplinary: the ontological foundations of historical materialism date back to a time before economics, sociology, political science, and by extension IPE, became separate disciplines. In addition to having multiple disciplinary homes, historical materialists also have thriving research communities of their own, with their own institutionalized scholarly networks, conferences, debates, specialized journals, and book series. This may be seen as self-segregation whereby scholars reject conventional IPE journals and rather seek dialogue and engagement among congenial colleagues (Seabrook and Young 2017, 310–11). Indeed, there is a certain resignation among historical materialists as ontological divides cannot simply be bridged or reconciled; after all, the contradictions of capitalism are not another paradox, blind spot, or puzzle that can be explained away, or in a Popperian sense, falsified on the basis of hypothesized cause-effect relations. At the same time, we should not underestimate “practices of gatekeeping that in turn are the product of power relations within academic fields” (Clift et al. 2020). These processes are intimately linked and self-reinforcing. Notwithstanding this, historical materialists are regularly in dialogue with scholars outside of historical materialism in the form of critique—critique understood as an enquiry into what is and what is not covered ontologically, how truth claims are reached and legitimized as a naturalized state of affairs, as well as how such truth claims authoritatively inform social practices. Critique is the key

touchstone of the dialectical research method that underpins historical materialism (Wigger and Horn 2016). Through critique, historical materialists have conquered new theoretical grounds, refined their concepts and re-searched their answers. In addition to self-critique, theoretical advancements were also achieved through critiquing the academic mainstream and other critical approaches.

By way of example, historical materialists have challenged social constructivist, poststructuralist, and postmodernist approaches, or what has also been referred to as the cultural or ideational turn, for perceiving the “real” as a social construction in the eye of the beholder, and language as the foundation of social life (Harvey 1991; Wood 1995; Palmer 1993; Bieler and Morton 2008). In particular, postmodern and poststructuralist scholars rejected truth claims and metanarratives, and focused their research on the various discursive identities and fluctuating subjectivities that people adopt. While historical materialists would share the assumption that social reality is intransitive and knowledge fallible, the focus on postmodern fragments at the expense of capitalist unity and historicity is incompatible with historical materialist assumptions and understandings about class and its materialist moorings (Wood 1995, 2). Most notably, Gramscian approaches insisted that the ideational cannot be understood as fully socially constructed, independent, and thus free-floating of the historically specific, material context in which ideas have emerged. As Bruff (2011, 397) has put it: “the ideas we hold are inextricably linked to our conditions of existence.” It is through critiquing the ideational turn, and the subsequent challenge to theorize the ideational-material axis more explicitly, that particular strands of historical materialism have emerged, such as the “Amsterdam Project” or Cultural Political Economy (see Overbeek 2004; Van Apeldoorn 2004; Jessop and Sum 2006).

Historical materialists have also consistently challenged neo-Weberian understandings of the state as a neutral institutional terrain and critiqued methodological nationalism, and the reification of states as giant fictitious andromorphogenized macro-agents (Robinson 2006). In contrast to approaches that perceive states as containers of a unitary national interest, historical materialists insist on understanding the state-society complex as an uneven terrain of class struggles which is historically embedded in, and internally related to, capitalist social relations. Gramsci’s (1971, 207–08) conception of the “integral state” and the subsequent refinements by Poulantzas (1978), but also Jessop’s (1999) strategic relational approach, are key theoretical sources in this respect. Poulantzas (1978, 132, original emphasis) defined the state as “the condensation of a relationship of forces between classes and class fractions, such as these express themselves, in a necessarily specific form, *within the state itself*.” Political struggles of the past are inscribed in the state as a site and center of the exercise of power (Poulantzas 1978, 148).

Theoretical contributions on the state-capitalism and state-society nexus particularly have received a boost alongside the rise of coercive state practices around the world in the aftermath of the 2008 crisis. Drawing on Poulantzas’ (1978) concept of authoritarian statism, Bruff (2012, 2014) coined the term “authoritarian neoliberalism” to refer to the seemingly contradictory development whereby as a result of the imposition of harsh neoliberal adjustment measures, states have appeared simultaneously stronger but also as more fragile, contested, and delegitimized entities. As Bruff demonstrates, while neoliberalism was never about markets being freed from the visible hand of the state and always relied on the suppression of democratic interference, authoritarian neoliberalism is characterized by the fact that even fewer attempts are being made to achieve consent from contesting groups through policy and/or material concessions. Bruff’s work has laid the groundwork for multiple analyses

and conferences within and beyond historical materialism, thereby elevating discussions on neoliberalism to the next level (Tansel 2017; Clua-Losada and Ribera-Almandoz 2016; Bailey et al. 2018). Historical materialists, like Worth (2019), have also sought to understand the rise of neo-fascist and radical right-wing movements and political parties around the world against the backdrop of the increasingly authoritarian state practices of imposing neoliberal solutions. Worth's research stands in stark contrast to "populism" research in the academic mainstream which disregards notions of class and the growing socioeconomic inequalities under neoliberal capitalism. As Palmer (1993, 147) had already observed in the 1990s, the term populism serves as "an interpretative container where anything can be poured into it, and so much else shut out by arbitrary adjustment of the lid."

Historical materialism is also uniquely situated to discuss the crises of social reproduction that have come to fore and that have also been exacerbated by the COVID-19 crisis. Feminist historical materialists have critiqued IPE approaches that rely on a separation between public and private, and between political spaces and the household. They have also critiqued historical materialist approaches that do not transcend class as a fundamental social category. Ever since, feminist historical materialists have made invaluable contributions theorizing how value extracted from unwaged labor in the household enters global value chains (Federici 2012; Fraser 2013; Bhattacharya 2017; Wöhl 2014). Historical materialists have also put much effort into remedying the ontological fallacies of globalization studies that have too hastily claimed the retreat, or hollowing out, of the state. Pradella (2017), for example, has sought to understand capitalism as a global manifestation without eclipsing interstate conflict and links the competitive accumulation of capital and economic crises to imperialist expansion, while at the same time bringing migration and the creation of surplus labor to the fore. In a similar vein, Yurchenko (2018) and Bieler and Morton (2018) theorize and analyze the dialectical interplay between inter-class and interstate antagonisms, showing that imperialist tendencies, militarism, and warfare are intimately related to intensified accumulation rivalries that find their origins in the structural problem of overaccumulation.

Last but not least, historical materialists have pointed to the "capital surplus absorption problem," or overaccumulation to explain the deeper roots of the financial and economic crisis of 2008 (see Overbeek 2012; Fernandez and Wigger 2016), and the growing prevalence of the rentier's type of capitalism in contemporary societies. Christophers (2020), for example, theorized and analyzed rent-seeking accumulation patterns revolving around making money from the control and ownership of scarce assets without producing anything in the process. He shows how rent extraction from the ownership of land, natural resources, infrastructure, intellectual property, and digital platforms alongside economic stagnation and underinvestment in the production economy is creating ever more polarized societies. In a similar vein, Soederberg (2014), who has coined the term "neoliberal debtfare state," has advanced the theorization of debt expansion in neoliberal capitalism, the profound social inequality that underpins creditor-debtor relations, and the state-institutionalist moorings that enable and legitimize debt-led accumulation patterns. Soederberg demonstrates how the state-financial capital nexus facilitates the "poverty industry" under the pretext of "financial inclusion," the "democratization of credit," and "consumer protection," and reveals the structural violence that comes with state mediation. A key concept to understand the rentier's type of capitalism is "secondary exploitation," an indirect dispossessive strategy in the sphere of exchange, such as interest-bearing debt or consecutive rental payments, which is laying a claim to the appropriation of a portion of the production of future surplus value,

and hence, future labor (Soederberg 2020; Wigger 2020). The strength of historical materialist work is that the sphere of circulation is theorized in relation with the sphere of production, which offers a more holistic account of social reality vis-à-vis approaches that focus on for example financialization or housing as if these were isolated domains.

CONCLUSIONS

This chapter has attempted to show that historical materialism is a vibrant, open, and dynamic perspective that has evolved through critique and transdisciplinary engagements. As such, it offers the IPE scholarship the opportunity to engage in critical discussions about global capitalism, as well to exert critique on historical materialist theorizing and analyses. Engagements with historical materialism moreover also offers the diverse IPE community an anchor point to discuss and contribute to transformative change. As Cox (1996, 90) reminds us, an integral part of critical scholarship is not only to explain and criticize the existing structures of social inequality but also to formulate coherent visions of alternatives to transcend them. Historical materialism after all propagates the unification of theory and praxis. Scholars should hence not only point to (structural) oppression, exploitation, and social inequality but defy them in struggles and actions. Most studies, including studies grounded in critical theory (broadly understood), however, often neglect to spell out their radical purpose or fail to define the conditions for emancipation (Farrands and Worth 2005). To be sure, while the emancipatory commitment to create a more just and egalitarian society is historical materialism's linchpin, reflections about post-capitalist futures and how to get there often remain untouched among historical materialists themselves, and if addressed, they tend to be kept utterly short and vague. Marx's (1845) reminder that "philosophers have hitherto only interpreted the world; the point is to change it" sometimes seems to abate silently. Leaving the comfort zones of mere capitalist critique and envisaging a non-capitalist future may seem to be a dauntingly naive endeavor, especially as the deeply ingrained macro-structures of global capitalism cannot easily be changed. Alternative visions are always incomplete and imperfect, and historical materialists, including Marx, are replete with contradictions. Yet, the mere possibility of envisioning a different world already holds a pre-sentiment of how to get to the envisaged future society.

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PART II

METHODS

CHAPTER 9

FORMAL MODELS OF INTERNATIONAL POLITICAL ECONOMY

Looking Back and Moving Forward

LESLIE JOHNS

SCHOLARS of International Political Economy (IPE) have made important findings about the interaction of economics and politics in regulating the global economy. What are the major insights provided by formal models of IPE, and how can future scholars profitably move scholarship forward? This chapter provides an overview of IPE scholarship, which is organized by the substantive topics that have been addressed by formal models. These topics include how states promote international trade, protect foreign investment, and stabilize and grow their economies. For each of these topics, I begin by looking back at past findings from formal models. I then discuss how IPE scholars can profitably move forward in their future research. Based on this discussion, I conclude by asking what explains the relative paucity of formal models of IPE, and when are formal models more or less useful for understanding IPE?

PROMOTING INTERNATIONAL TRADE

States pass many kinds of policies to regulate the sale of goods and services across borders. These include both tariffs—which are taxes on a good moved across a border—and non-tariff barriers—which include quantitative restrictions on the amount of goods and services that can be traded, and regulations and standards that restrict trade.

Economists have long argued that the main reason why states trade is comparative advantage. This comparative advantage can come from variation across states in technology or in productive assets known as factors, like capital, labor, and land. Conventional accounts of comparative advantage argue that states benefit in the aggregate if each state specializes in producing the good or service in which it has a comparative advantage, and then removes

barriers to trade across borders. In early formal models of international trade, comparative advantage leads to trade across industries. For example, a state that is abundant in land may specialize in agriculture, while a state that is abundant in labor may focus on manufacturing.

Looking Back

IPE models of trade politics focus on the distributional (rather than the aggregate) effects of trade. Traditional models of trade politics focus on three key actors. First, consumers in these models benefit from low tariffs at home, which reduce the price of imported goods and services. To the extent that these consumers are also workers, most formal models assume that they can easily shift their labor in response to market demand. Second, exporting firms benefit from low tariffs abroad, where they sell products to foreign consumers. Third, import-competing firms benefit from high tariffs at home to limit foreign competition. Trade policy therefore pits the interests of consumers and exporting firms, who favor free trade, against import-competing firms, who oppose free trade.

Economic theory suggests that the aggregate benefit of international trade, particularly for consumers, should cause states to reduce trade barriers. Yet governments often neglect consumer interests when making trade policy (Betz and Pond 2019b). Instead, governments are more responsive to firms who lobby against free trade. Such firms can use the tools available to any special interest group to achieve their goals. For example, Ehrlich (2007) shows that trade lobbying is affected by access to policy-making. Similarly, Ballard-Rosa et al. (2016) demonstrate that political pressure to protect domestic firms rises and falls over time in response to economic crises.

Why can't consumers also lobby lawmakers in favor of free trade? Formal models usually highlight two causal factors. First, collective action problems make it difficult for larger groups to mobilize politically. Smaller, concentrated groups are therefore more likely to secure their preferred trade policies than larger, diffuse groups (McGillivray 2004). Second, information asymmetries affect who knows what about trade policies. Firms that actually sell goods and services across borders are believed to be more informed about trade policies than consumers, who cannot easily understand whether high prices are caused by trade policy or other economic factors (Kono 2006; Mansfield, Milner, and Rosendorff 2002).

Additionally, trade policy lobbying is affected by the design of domestic political institutions. Lohmann and O'Halloran (1994) argue that executives have strong incentive to protect consumers because they represent the whole electorate. Yet legislators, who usually have more power over trade policy, are subject to local interests that can lead to protectionism. Similarly, Rogowski and Kayser (2002) argue that electoral rules (e.g., majoritarianism, proportional representation, etc.) affect how politicians trade off consumer and firm interests. Additionally, governments with more veto players may find it more difficult to negotiate and ratify treaties that liberalize trade (Mansfield, Milner, and Pevehouse 2007; Milner and Rosendorff 1996).

Formal models in IPE usually represent these microfoundations using the Prisoners' Dilemma. The Nash equilibrium of this static game is for both states to choose Protectionism. Yet both states would be better off if they could jointly agree to Free Trade. The central theoretical question in most trade models is therefore how states can credibly commit to free trade, despite the temptation to protect their markets from foreign competition.

Most IPE models argue that international institutions provide a solution to the Prisoners' Dilemma. For example, Mansfield, Milner, and Rosendorff (2002) argue that institutions like the World Trade Organization (WTO) mitigate information asymmetries between consumers and import-competing industries. They construct a formal model in which WTO lawsuits act as fire-alarms that alert consumers to protectionist policies. Leaders therefore benefit from signing trade agreements because the possibility of being caught restricting trade allows them to resist pressure from import-competing industries. Consumers accordingly view trade agreements as a credible signal that their leader is committed to free trade.

Hollyer and Rosendorff (2012) use a complementary model to argue that trade agreements reduce the variance of trade policy. By reducing uncertainty about future domestic policies, consumers can make better economic decisions that increase their well-being. They argue that democracies, which are more responsive to the average voter's well-being than autocracies, are therefore more likely to sign more trade agreements. Carnegie (2014) argues that a similar mechanism exists at the firm level: when firms have confidence that their government is committed to free trade, they are more willing to make long-term investments in the production of goods and services for export.

These political dynamics also affect how states design trade agreements (Johns and Peritz 2015). One source of variation in trade agreements is the *depth* of cooperation—how much states are required to reduce trade barriers. Mansfield, Milner, and Rosendorff (2000) argue that ratification dynamics shape the depth of tariff concessions. They argue that when two democracies negotiate, each side must make deep commitments to ensure that the agreement will be ratified by their trading partner's domestic legislature. However, when a democracy negotiates with an autocracy, it need not make such deep concessions because an autocrat does not need support from an independent legislature.¹

A second source of variation in trade agreements is *flexibility*—how much states tolerate violations of trade concessions. IPE scholars argue that leaders often face unexpected economic and political pressure to restrict trade, and trade agreements must be designed to manage such pressure (Johns and Rosendorff 2009; Rosendorff and Milner 2001). One tactic for managing temporary, stochastic pressure is for injured states to tolerate occasional unilateral violations of trade agreements in exchange for compensation (Rosendorff 2005). This mechanism allows for the efficient breach of trade agreements. An alternative tactic is for law to explicitly allow appeals to exception, whereby states can make principled legal arguments that external circumstances allow them to restrict trade (Pelc 2009). If a neutral actor, like an international court, can observe and verify these external circumstances, then cooperation can be stable over time despite occasional violations.

While most IPE scholars have examined such design attributes in isolation, trade agreements are designed with multiple dimensions in mind. Recent formal models show that when states negotiate deeper trade concessions, they should incorporate more flexibility (Johns 2014). Depth and flexibility therefore act as complementary attributes of legal agreements (Johns 2015).

Finally, political factors also affect trade disputes between states. Many recent formal models have moved beyond the fire-alarm metaphor to examine trade dispute politics. One strand of research examines the timing of disputes. Rosendorff and Smith (2018) argue that when a state's government changes, the coalition of interests seeking trade protection also changes. In their formal model, government changes trigger policy changes, which in turn trigger trade disputes. They argue that the effect of government turnover is strongest

within autocratic states because autocrats are more likely to narrowly target trade protectionism, leading to more dramatic changes in trade policy when governments change. Similarly, Chaudoin (2014) argues that states are most likely to challenge trade policies when their trading partner will be most responsive to the demands of domestic consumers, such as during elections held at times of low unemployment. However, he argues that such circumstances also make a government less likely to violate trade agreements in the first place.

Another strand of research emphasizes multilateralism in trade disputes. Johns and Pelc (2014) argue that litigants strategically decide whether to encourage broad participation in trade disputes. They argue that broad participation both lowers the benefit of winning a trade dispute—because concessions must be shared amongst more states—and lowers the cost of losing a dispute by politicizing the outcome. These two factors affect pre-trial demands and settlements. Overall, complainants have incentive to promote participation when their case is weak, and to limit participation when their case is strong. Similarly, Johns and Pelc (2016) argue that strategic incentives affect state decisions about whether to participate in disputes. While participation gives states access to private benefits, it also reduces the likelihood of settlement. Accordingly, affected states may refuse to participate in trade disputes to encourage pre-trial settlements that expand trade. Finally, Johns and Pelc (2018) argue that free-riding incentives affect initial decisions about whether to file trade disputes. They argue that trade violations are less likely to be challenged when their impact is spread out over many trading partners.

Moving Forward

Recent developments in economic theory—called “new new trade theory” (NNTT)—have dramatically changed our understanding of international trade. NNTT was motivated by changing empirical patterns in global trade that began in the 1980s. Economists noted that instead of trading goods across industries, most states had shifted to intra-industry trade. Additionally, firm-level variation within industries (in employment, productivity, profits, etc.) suggested that traditional formal models of cross-industry trade were no longer adequate.

In a seminal formal model, Melitz (2003) constructed an alternative economic theory of international trade that matched new empirical patterns. In this model, firms within a state vary in their productivity at producing goods and services, and firms produce differentiated products, which are unique varieties of a good or service. So, for example, instead of making wine, firms produce many different varieties of wine, each with their own unique characteristics. If consumers prefer variety, then a market will contain many firms that vary in their attributes (employment, productivity, profits, etc.).

Many of the key political features of the traditional trade model still hold under NNTT. In the Melitz model, consumers still benefit from international trade because they can buy a broader variety of goods at lower prices, and can change jobs easily if their own employer is put out of business by foreign competition. Additionally, exporting firms prefer low tariffs abroad, while import-competing firms prefer trade protection at home (Gilligan 1997; Kono 2009).

One path forward for IPE scholarship is to ask: how does NNTT affect trade politics?

The basic assumptions and insights of formal models of trade policy should continue to hold because the basic preferences over trade policy still hold. However, the Melitz model suggests that political cleavages will no longer be based on industries; instead, cleavages can occur within industries. Future scholarship should use the insights from NNTT to probe three questions more deeply: who supports or opposes trade liberalization; how does product differentiation affect tariffs; and how does NNTT extend to non-tariff barriers?

First, NNTT does not tell us which specific firms will support or oppose free trade. Firms that face discrimination at home—such as women- or minority-owned businesses—may benefit most from international trade (Osgood and Peters 2017). In contrast, firms that already export goods may benefit from restricted trade abroad (Osgood 2016). Productivity does not imply support for free trade. Supply chains—in which goods are bought as inputs for making other goods—can also affect trade policy preferences (Osgood 2018).

Second, product differentiation may allow governments to narrowly target trade protection. For example, instead of lobbying on wine tariffs, vintners may lobby on tariffs on cabernet, chardonnay, and merlot. Gilligan (1997) and Kim (2017) suggest that product differentiation transforms tariff lobbying into a private good, reducing collective action problems and increasing protectionism. However, NNTT also suggests that stronger consumer preferences for differentiated goods may reduce trade barriers over time (Osgood 2019).

Third, scholars should consider how NNTT affects non-tariff barriers, which can raise costs for both domestic and foreign firms. For example, Kennard (2020) examines firm-level lobbying for climate change policies that increase production costs, and Perlman (2020) examines firm-level support for safety-based product bans. While such policies and bans directly reduce firm profits, they also reduce economic competition, which can indirectly raise profits for some firms. Similarly, Gulotty (2020) argues that supply chains have dramatically changed the politics of non-tariff barriers like labeling and licensing rules.

An alternative path forward is to ask: how can we understand the recent backlash against international trade? In recent years, voters have opposed free trade in the US and other developed democracies (Johns, Pelc, and Wellhausen 2019). Yet existing trade theory suggests that consumers benefit (in the aggregate) from free trade. What explains this backlash? Why do our theoretical models not match the real-world voter preferences?

The most likely answer is that trade theory largely ignores labor market adjustment costs. For example, Melitz (2003) assumes that labor can be freely redeployed across both firms and industries. So a steel-worker whose firm is harmed by foreign competition can freely transfer to another steel mill or work in another industry, like software design. Clearly, this assumption is unrealistic.

Policymakers understand that trade liberalization harms workers who cannot easily change jobs. Accordingly, they often provide trade adjustment assistance, which gives compensation and education to displaced workers. Yet such programs have not quelled political opposition to free trade. Kim and Gulotty (2019) argue that governments have not provided adequate trade adjustment assistance because doing so signals to voters that trade policy may harm them. Politicians may therefore want the illusion that everyone benefits from free trade. Future IPE scholars should consider this mismatch between the illusion and the reality of adjustment costs. Doing so will require new theoretical models of both the economics and politics of labor markets.

PROTECTING FOREIGN INVESTMENT

Foreign investment can come in three forms. First, portfolio investment consists of the purchase of publicly-listed stocks and bonds by foreigners. Second, sovereign debt consists of loans to governments from private or public actors. Third, foreign direct investment (FDI) involves an investment by a firm in one state into business activities that take place in another state.² For this investment to qualify as FDI, the firm must have long time-horizons and be directly involved in business activities abroad.³ FDI usually involves the purchase of immobile assets, which cannot be easily redeployed for other activities. Both portfolio investment and sovereign debt have received almost no attention from formal IPE scholars. But a growing body of scholarship uses formal models to assess FDI politics.

Looking Back

Most scholars argue that developing states want FDI, but face a commitment problem. They argue that strategic interactions between a foreign firm and host government unfold sequentially over time, as shown in Figure 9.1. First, the firm must decide whether to invest in the host state. If the firm invests, then the host government must decide whether to take the investment. Such “taking” can correspond to outright expropriation, severe contract violations, or changes in the regulatory environment that substantially decrease the value of the foreign firm’s investment while providing an economic or political benefit to the host government.

From the firm’s perspective, the investment is only profitable if its property rights are respected. The worst outcome is accordingly to invest and have its assets taken. A better outcome is to not invest in the first place (and pursue other investment opportunities). The best outcome is to invest and not experience expropriation. From the host government’s perspective, the worst outcome is to not receive the investment. But if the firm invests, then the host government will be tempted to take the investment.

If the host government faces no penalty from taking the foreign investment, then it cannot credibly commit to property rights. Early IPE scholars described such situations as an “obsolescing bargain” because favorable conditions that were promised to investors *ex ante* often were not realized *ex post* (Vernon 1971). These scholars argued that foreign firms should anticipate such situations and under-invest in developing states. This behavior is shown in Figure 9.1(a). However, if the host government could credibly commit to not take the investment, then both the firm and host state would benefit. Namely, if taking imposes large exogenous costs ($c > 1$) on the host government, then investment will occur, as shown in Figure 9.1(b). Thus, large exogenous costs solve the commitment problem.

But how are such exogenous costs generated? One possible mechanism is reputation. If other investors believe that they too will have their assets taken, they may refrain from future investment, thereby harming the host government in the long term. For example, Jensen and Johnston (2011) argue that states with more natural resources can offer higher investment returns than states without them. Therefore, they argue that states with natural resources suffer a lower reputation cost from taking, and are accordingly more likely to expropriate.

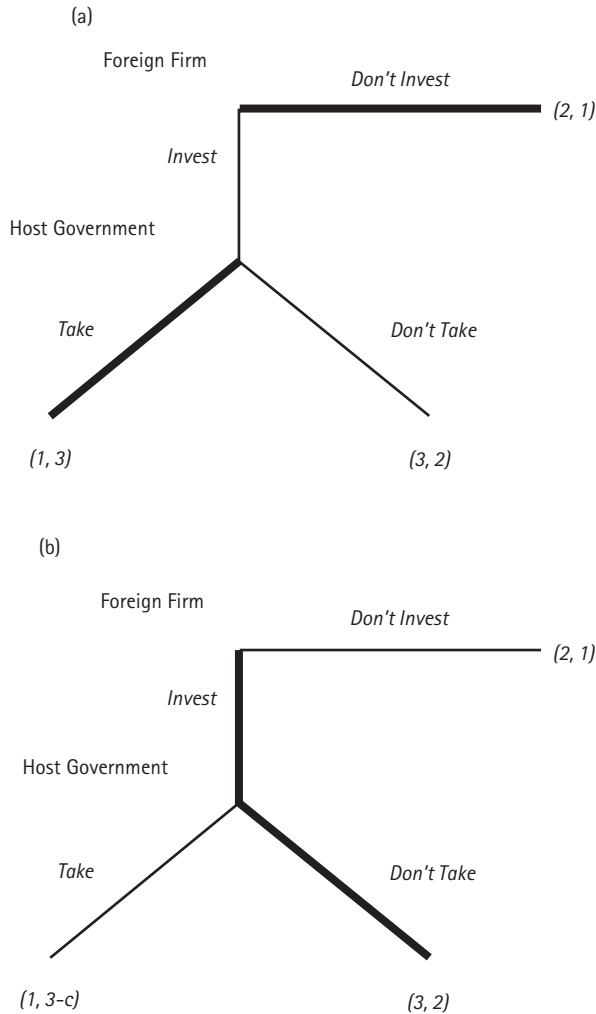


FIGURE 9.1. Foreign direct investment as a commitment problem. (a) No cost from taking the foreign investment; (b) High cost from taking the foreign investment ($c > 1$).

An alternative (and complementary) argument is that domestic political institutions and voter accountability constrain the host government. This literature echoes earlier arguments about how democratic institutions secure property rights (Weingast 1997). For example, Albornoz et al. (2012) argue that governments consider the impact of expropriation on political constituencies. They argue that democracies are less likely to expropriate foreign investment in manufacturing than in land because manufacturing disruptions will have a broader impact on voters.

How can states better promote FDI? Many IPE scholars argue that international law solves commitment problems by raising the cost of expropriation. For example, Arias et al. (2018) argue that autocracies benefit more than democracies from joining international investment agreements because they face fewer domestic constraints. They argue that such agreements

will have a larger effect on government survival in office when states are more autocratic. Investment agreements can also induce domestic firms to form financial relationships with foreign firms to gain access to international legal protection (Betz and Pond 2019a).

International law may also increase reputation costs. International investment agreements create legal standards and allow investors to sue host states using investor-state dispute settlement (ISDS). These procedures coordinate beliefs about appropriate behavior and compensate investors when their rights are violated (Johns, Thrall, and Wellhausen 2020; Johns 2019). When an international tribunal finds a host state guilty of breaking international law, future investors can observe this public signal and adjust their own behavior. International law can therefore coordinate reputation costs for legal violations (Johns 2012).

Another IPE literature examines the impact of domestic policies on foreign investment. Many economists argue that high trade barriers stimulate “tariff-jumping,” in which foreign-owned firms produce their goods in the country-of-sale to avoid tariffs (e.g., Brander and Spencer 1987). Trade restrictions can therefore promote FDI. Once such foreign firms invest, they have incentive to lobby their host government to maintain or even increase trade barriers (Blonigen and Ohno 1998). FDI investments can therefore cause the political interests of foreign firms to align with those of domestic firms. This literature largely overlooks commitment problems by examining states with strong domestic institutions, like the US.

Similarly, some IPE scholars have examined the impact of labor policy on FDI. Pinto and Pinto (2008) argue that changes between pro-labor and pro-capital governments affect the allocation of FDI across different industries. In particular, governments that support labor are more likely to encourage FDI, which benefits their domestic constituents (Pinto 2013). These models show that government ideology shapes FDI profitability, and hence the risk tolerance of foreign firms. Alternatively, Payton and Woo (2014) argue that stricter labor laws decrease new FDI.

Moving Forward

Most previous formal IPE research focuses on how host governments can solve commitment problems by building strong domestic institutions or joining international investment agreements. One path forward is to examine how foreign firms mitigate political risk through their business activities. Johns and Wellhausen (2016) argue that supply chains with local businesses protect the property rights of foreign firms. When a host government contemplates mistreatment of a foreign firm, it must consider the negative spillover effects on domestic firms. Supply chains thus link foreign and domestic firms through their economic activities, making them partners in the protection of capital. Johns and Wellhausen (2019) show that high entry costs also protect FDI by deterring the entry of new foreign firms into markets.

An alternative path is to examine FDI’s impact on domestic politics in host states. For example, Bhagwati et al. (1987) argue that foreigners may invest in host states to buy political influence, rather than economic profit. More recently, Tomashevskiy (2015) argues that foreign portfolio investment increases political contributions for right-wing parties. Similarly, Tomashevskiy (2017) shows that FDI can prevent coups by giving autocrats revenue to buy

elite political support. These findings suggest that formal models of IPE could examine domestic politics outcomes from FDI.

Finally, future research could examine how FDI relates to broader financial liberalization. For example, FDI can only enter a state if a government first decides to open its market. Pond (2018a, 2018b) argues that the relationship between financial liberalization, expropriation, and taxation are interwoven and dependent on a state's regime-type. Additionally, international law affects whether foreign investors can repatriate FDI profits. While veto players constrain a government's decision about whether to take foreign-owned assets, Graham et al. (2018) argue that veto players do not necessarily constrain profit repatriation. A full understanding of FDI may therefore require more examination of capital restrictions.

STABILIZING AND GROWING ECONOMIES

One final category is financial and monetary policies for stabilizing and growing economies. Many scholars of comparative political economy study topics like central bank independence and exchange rates, and many empirical IPE scholars examine financial and monetary policies. Yet relatively few formal IPE models examine these topics. After briefly describing the existing literature, we ask how future scholars can profitably use formal models to contribute to these topics.

Looking Back

Traditional macroeconomic theory suggests that monetary policy—which affects the supply of money in the domestic economy—has contradictory effects on employment and inflation. Economists argue that expanding the money supply increases employment, production, and spending while also increasing inflation. In contrast, contracting the money supply reduces inflation while also reducing employment, production, and spending. This relationship suggests a trade-off between inflation and unemployment: monetary policies to lower unemployment will also cause higher inflation.

Most research on monetary policy focuses on domestic influences. However, a few scholars have examined how international politics influences monetary policy. Suzuki (1994) constructs a formal model in which the monetary policy of one state affects the relative value of its currency, which affects inflation for its trading partners. Suzuki argues that these externalities in monetary policy create a Prisoners' Dilemma in which states defect when they restrict their money supply. He argues that economic interdependence (i.e., higher externalities) has mixed effects, increasing both the temptation to defect and benefit of cooperation.

A related IPE literature examines monetary integration—the adoption by multiple states of a single currency. To integrate, states must delegate their autonomy over monetary policy to an international organization. For example, in the 1990s, some European Union members decided to adopt the Euro as a common currency. Euro-adopting states gave the European Central Bank—an international organization—authority to decide whether to expand or contract the Euro supply. Both economists and political scientists debated this integration,

with some scholars grounding their claims in formal models. For example, Alesina and Grilli (1993) questioned whether partial (or “multispeed”) integration would lead to full integration over time. They argued that if states with similar policy preferences—such as prioritizing low inflation—integrated first, they would choose an international bureaucrat who reflected their preferences. When a new state contemplated joining the currency, it would have different preferences—such as prioritizing low unemployment. While this state might have benefited from monetary integration, it would have little impact on the international bureaucrat. Such a state might therefore keep its own currency so that it could maintain control over monetary policy. In contrast, Plümper and Troeger (2008) focused on integration’s impact on non-members. They argued that monetary integration reduced autonomy for states outside the Eurozone that traded predominantly with states within the Eurozone. This research on monetary integration connects to a broader literature on multilateralism, which examines how the composition of international organizations drives their effectiveness and the selection of international bureaucrats (Gilligan and Johns 2012, 231–3; Johns 2007).

A related topic in IPE is capital mobility—how easily capital can be moved across borders. Some states allow capital to move freely, meaning that domestic firms can easily invest their money abroad and bring their profits home, while other states restrict mobility. Capital mobility is closely linked to monetary policy and exchange rates because large flows of capital into or out of an economy affects both exchange rates and a government’s ability to shape employment and inflation. Capital mobility is also linked to international trade and investment because the free movement of capital is necessary for investment to occur, which can affect trade.

High capital mobility makes it easier for foreign investors to enter a market to produce and sell their goods. Most IPE scholars therefore hypothesized that more foreign investment should make trade policy less effective in protecting domestic firms from foreign competition. Capital mobility, they believed, reduced pressure for trade protection. However, Hiscox (2004) uses a formal model to show that if capital is specific (e.g., can only be used in a particular industry, geographic location, etc.), then mobility and high tariffs can coexist. For example, if the US government makes it easier for foreign firms to open automobile factories in Alabama, then the price of local labor will increase. Higher labor costs may then cause an Alabama textile manufacturer to seek higher textile tariffs. Similarly, Pond (2018c) argues that labor unions will pressure governments for asymmetric policies on capital mobility. While capital inflows can increase employment and wages, capital outflows have the opposite effect. Therefore, Pond argues that states with stronger labor rights will have more inflow openness, but less outflow openness. Finally, Farías and Arruda de Almeida (2014) argue that capital mobility encourages illicit activities, like money laundering and tax evasion, which harm economic growth.

One final topic in IPE research is unearned international income, such as foreign aid and remittances. Political scientists have long argued that rich states can use foreign aid to buy policy concessions from poor states. Lundborg (1998) offered an early formal model of aid competition between the US and USSR. More recent research by Bueno de Mesquita and Smith (2007, 2009) examines how domestic politics in the recipient state changes how states use aid to purchase policy concessions. They then extend this model to examine competition between multiple donor states (Bueno de Mesquita and Smith 2016). Arena and Pechenkina (2016) apply these insights to show that foreign aid can prevent international conflict.

However, Smith (2008) argues that aid also prevents democratization by giving the recipient government resources to suppress internal dissent.

Similarly, many political economists have begun to examine how remittances—which are payments that workers who live abroad send back to their home state—affect domestic governance. Abdih et al. (2012) show that remittances provide household income that reduces pressure on a government to provide basic services. They argue that remittances therefore enable corruption. However, Desierto (2018) argues that political competition constrains whether governments can divert resources in this way. Similarly, Pfütze (2014) shows that remittances make individuals less reliant on government patronage, thereby increasing electoral competition.

Moving Forward

Few formal models of IPE examine economic stabilization and growth. This neglect is probably caused by the weak microfoundations for traditional macroeconomic theory. For example, many contemporary economists question the central assumption of monetary policy—that there is a trade-off between employment and inflation—based on policy responses to the Global Financial Crisis of 2008–2009. Until this debate is resolved, IPE scholars will probably contribute little to scholarship on monetary policy, monetary integration, and capital mobility.

The best path forward is probably to focus on topics with well-established economic microfoundations. IPE scholars can then focus on how international politics interacts with economics. For example, few formal theorists have considered the International Monetary Fund (IMF), which issues loans to states during financial crises. Dreher (2004) provides a compelling model in which an IMF loan is a costly signal to voters about government preferences. Similarly, Fang and Stone (2012) examine government decisions to delegate policy-making to IMF bureaucrats. Such papers demonstrate the value of using formal models to study the IMF.

Other neglected topics include sovereign debt—the issuance of loans to governments by private and public actors—and migration—the movement of individuals across borders. Ballard-Rosa (2020) provides a compelling argument about how conflicts between rural and urban areas affect sovereign debt repayments during economic crises. Similarly, Sellars (2019) offers a nuanced theory of how outward migration stymies political mobilization against a government. Her argument suggests that pro-emigration policies may help leaders to stymie internal dissent, an insight supported by Miller and Peters (2020).

CONCLUSION

These examples illustrate that formal models of IPE have been motivated by substantive research questions, rather than methodological debates.⁴ Contemporary research is motivated by important substantive topics, like how states promote international trade, protect foreign investment, and stabilize and grow their economies. While this chapter has focused on the substantive arguments put forth in this literature, many scholars assess the plausibility of

their formal models using both quantitative and qualitative methods (Lorentzen, Fravel, and Paine 2017).

These examples also illustrate that relatively few IPE scholars have used formal models. What explains the relative paucity of formal models of IPE? When are formal models more or less useful for understanding IPE? Formal models are usually most useful when they address relatively rare or isolated interactions amongst a small number of elite actors. However, many structural aspects of IPE topics depart from these criteria.

First, most IPE interactions—such as the trade of goods and services, the investment of capital abroad, etc.—are infinitely repeated, rather than rare or isolated. Such infinitely repeated interactions generate major conceptual challenges for formal models because they generate an infinite number of equilibria. Consider the example of a host government that expropriates a foreign investment. A well-defined equilibrium would need to specify how all potential investors respond to all possible actions. Should an investor refuse to invest for one period? For two periods? Forever? What if she ignores the expropriation of another firm and invests anyway? All of these behaviors can be supported in an equilibrium. So how can formal theory help us to understanding observable behavior?

Second, economic markets are made up of a huge number of players, leading to complex strategic possibilities. Microeconomic theory usually simplifies these possibilities by assuming that firms are “price-takers,” meaning that each firm cannot change the overall market demand or price of goods. Yet in the modern global economy, large and powerful firms often do shape demand and prices, challenging many theoretical assumptions about how economic markets work. Additionally, large firms are not “politics-takers”—their actions affect the political environment in which they operate. Formal models of IPE must therefore balance competing assumptions about when individual economic actors can change a strategic environment, and when they cannot.

Finally, many IPE topics do not involve elite actors. Formal models almost always assume that actors can understand their strategic environment and anticipate the possible consequences of their actions. Yet many IPE topics, like political support for trade liberalization, involve mass publics with limited cognition about the consequences of their actions. For example, empirical research shows that voters understand little about the consequences of trade agreements for their own economic well-being (Blonigen 2011). As IPE research moves forward, formal models will be most useful when they address topics that avoid these three conceptual challenges.

NOTES

1. In contrast, Kim, Londregan, and Ratkovic (2019) argue that autocracies balance the benefits of trade against the negative security externalities from interpersonal contacts enabled by trade. They argue that the depth of cooperation is less relevant for unstable autocracies than the initial decision about whether to open trade.
2. Throughout this chapter, the terms “firm” and “investor” refer to individuals, businesses, partnerships, corporations, etc. that engage in business activities.
3. Simply purchasing company shares on a stock exchange would not qualify as FDI since stock-holders are typically not involved in the day-to-day management decisions of companies.
4. See the chapter by Jon C. W. Pevehouse and Leonard Seabrooke in this volume.

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CHAPTER 10

EXPERIMENTS IN IPE RESEARCH

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WHILE experiments are no “new” method in the study of International Political Economy (IPE), within the last decade experiments have developed from a marginally employed to an increasingly standard method. Articles based on experimental research designs have their steady place both in the major journals of our discipline and in the program of the major political economy conferences, such as the International Political Economy Society (IPES) or the Political Economy of International Organization (PEIO) conference.

The reason for this surge in studies relying on experimental methods is probably threefold (see Jensen et al. 2014; Morton and Williams 2010; Mutz 2011): First, IPE research in general has become increasingly interested in the micro-level underpinnings of its macro-theoretical frameworks, which is at the heart of the so-called “open economy politics” (OEP) framework (Lake 2009). The OEP framework considers economic policy as a governmental response to the interests and preferences of different societal actors, such as firms, voters, and interest groups. This approach has not only been the basis for innumerable macro-level studies modeling, economic policymaking, such as trade policy, monetary policy, etc., as a function of pivotal societal preferences (e.g., Hiscox 1999; Broz and Frieden 2001; Mansfield et al. 2002; Rogowski and Kayser 2002) but has also sparked interest in survey research to better understand the precise microfoundations of these theoretical models (e.g., O’Rourke and Sinnott 2001; Scheve and Slaughter 2001a, b; Mayda and Rodrik 2005). Following this general turn toward a better understanding of the microfoundations of economic policymaking, the field has witnessed since then a proliferation of experimental research focusing on the precise causal relationships that determine which actor holds which kinds of preferences.

This leads to the second reason for the increase in experimental research in IPE, namely the growing importance of causal identification (Pepinsky 2019). This quest for identifying causality, as described in more detail later when discussing the potential outcomes framework, is present in political science research more generally and has clearly strengthened the importance of the experimental method as an integral part of the research agenda in IPE.

The third reason for the rise in experimental research has to do with the immense technological progress in conducting experiments, especially online survey experiments (for an

overview see Mutz 2011). On one hand, the technical hurdles to implement experiments due to the existence of advanced and easy to program experimental software are relatively small. On the other hand, the costs of conducting experiments have strongly decreased, *inter alia* due to platforms such as Time-Sharing Experiments for Social Sciences (TESS) in the US, with similar platforms having been released over the last years also in other countries, and due to commercial platforms, such as Amazon Mechanical Turk.

This chapter intends to provide an overview of the variety of experimental research in IPE. After a short introduction to causal inference and the potential outcomes framework, I outline why experiments are often seen as a solution to the fundamental problem of causality. The chapter continues with outlining different experimental methods. The remainder of the chapter discusses a multitude of experimental IPE studies. In doing so, I follow the example of Druckman et al. (2006) who provide an overview of experimental research in political science by relying on Roth's (1995) three roles of experiments, namely searching for facts, speaking to theorists, and whispering in the ears of princes. In this discussion, I strive to include experimental studies from various subfields in IPE to illustrate the diversity of experimental research in our discipline. The chapter concludes with a critical assessment of both the advantages and disadvantages of the experimental turn in IPE.

CAUSAL INFERENCE—THE POTENTIAL OUTCOMES FRAMEWORK

The core challenge for most scholars in applied IPE research, as in applied social science research more generally, is the pursuit of causal inference. While this pursuit of causal inference is, of course, nothing new, with the increasing popularity of the experimental method a convergence can be observed toward the specific framework under which this strive for causality is conducted, namely the so-called Neyman-Rubin potential outcomes framework (Neyman 1923; Rubin 1974). This potential outcomes framework sees the identification of a causal effect as a comparison of the effect—given a condition or treatment has happened—with the non-effect in the absence of this condition (Neyman 1923; Rubin 1974). To make a causal statement thus ultimately boils down to the question whether one would observe the same result (or effect) if the respective cause (or treatment) would be different. The fundamental problem with this approach is that it hinges on a counterfactual ideal: A researcher can never observe the same object of analysis simultaneously once with and once without the manifestation of the respective cause (or treatment). Stated simply a scientist can in no case empirically compare an object of investigation at the same time with itself yet given a different cause (or treatment) (see Holland 1986; for an introduction, see Sekhon 2008).

This fundamental problem of causal inference might become clearer if one considers it in the light of a real-world, IPE question. One question at the center of IPE research on foreign direct investment (FDI), for example, is whether firms prefer to invest in democratic as compared to autocratic countries (Jensen 2008). Since democracies differ from their autocratic counterparts on many other grounds too, empirical studies try to accommodate these differences either by controlling for the most important confounders in a regression

framework or by comparing countries that are as similar as possible but for their political system in a case study framework. Yet, these approaches cannot completely eliminate the possibility that some differences, either observable or non-observable, exist that make the democratic countries receiving FDI different from the autocratic ones. The problem would not exist if one could simply observe the same country twice at the same time point. Since, however, the question of whether country *x* would have received the same amount of FDI if it had been democratic instead of autocratic is purely counterfactual, researchers need to opt for a second-best approach: either compare countries with different political systems at the same time or investigate a country over time that has changed its political system (or a combination of the two approaches).

Irrespective of whether an analysis compares the same country to itself at different points in time or different countries at the same time—longitudinal versus cross-sectional research design—the risk remains that an observed difference was not triggered by the postulated cause, but is due to other dissimilarities. Hence any conclusion that the observed difference is indeed causal rests on uncertain grounds and the aim of any research design is thus to exclude as many alternative explanations as possible to ensure internal validity.

Conducting an experiment provides a way out of this fundamental problem of causality. In such a setting, the researcher, by manipulating the presumed cause (treatment) and randomly assigning the cause or its absence to the participating objects of the experiment, can exclude any potential alternative explanations.¹ In particular, it is the random allocation of the treatment which breaks the link between any potential confounder and the outcome and thus ensures that the treatment is the only systematic difference between the different treatment groups (Fisher 1935). As a consequence, differences in the observed effects between the various groups should be due solely to the manipulated cause.

VARIETIES OF EXPERIMENTS

Experimental designs differ along various dimensions. First, one can distinguish experiments according to where they are conducted. There are experiments done in the laboratory versus experiments run in the field. Yet the most common method by now, which rarely existed a decade ago, are experiments as part of surveys. Within this broad classification of experiments, one can further distinguish between experiments implementing games derived from game theoretic models, direct informational treatments, in the form of framing or priming experiments or experiments conveying information via audio or visual techniques, vignette experiments, such as conjoint experiments, list experiments, etc. Furthermore, experiments differ as to whether they are one-shot experiments or consist of multiple repeated interventions over time and as to whether they were implemented online or via direct face-to-face contact with respondents. Also the type of the respondent can differ. While most experiments are interested in ordinary citizens as their unit of analysis some experiments intend to elicit the preferences of other types of actors, such as firms, organizations, etc. Finally, the outcome variable in experiments can differ. While most studies, especially survey experiments, use some kind of attitude as their main outcome variable, some experiments are interested in real behavioral change, which can range from sending a text message to political representatives to allocating and receiving money in

laboratory experiments. I will discuss these varieties in experimental design in the following paragraphs.

Type of Experiment

Laboratory experiments, as their name suggests, take place in a laboratory and thus in an environment that allows for comprehensive control. In IPE research, such experiments mostly take the form of games deduced from game theoretic models and are implemented on computers. Respondents can either “play” against the computer in a randomized version of the game theoretic model under investigation or respondents can play against each other.² In such settings, the researcher fully controls who participates and who receives which kind of treatment. While this aspect might sound trivial at first encounter, other types of experiments, such as field or survey experiments, sometimes do not find it easy to ensure either of these components. For example, in a survey experiment the person targeted might not be the person actually responding but may be the younger brother opening the invitation email on the targeted person’s computer. In field experiments, it is often not easy to ensure that the intent to treat and actual treatment correspond since respondents might simply not have opened the respective text message sent as part of the treatment. Or they might have forwarded their treatment information to someone who was not part of the initial treatment group thus violating the Stable Unit Value Assumption (SUTVA);³ an assumption necessary for causal identification under the potential outcomes framework (Sekhon 2008). This advantage of laboratory experiments often comes at the expense of reduced external validity. Since most laboratory experiments are conducted with a limited number of college students the question arises of how well their findings generalize to the other parts of the population (Mutz 2011).

Field experiments, on the other hand, allow for an estimation of a causal effect in an empirically relevant or “natural” environment and thus can often have higher external validity. However, in field experiments a researcher’s ability to ensure randomized treatment allocation and whether the treatment has actually been received by the treatment group only, is often strongly reduced thus potentially decreasing internal validity.

One way to ensure internal validity without necessarily jeopardizing external validity is the *survey experiment* (Jensen et al. 2014; Morton and Williams 2010; Mutz 2011). As the name suggests, survey experiments consist of (typically) information-based experiments integrated into a survey. By randomly allocating which survey respondents receive what kind of information, survey experiments are usually characterized by strong internal validity. Furthermore, since survey experiments are frequently integrated into national representative surveys, this implies an oftentimes higher external validity than is the case with laboratory experiments.

Survey experiments can take various forms. Most typical are what Mutz (2011) calls direct treatments. These can be, for example, so-called vignette treatments, in which respondents see different versions of a hypothetical scenario. An example of such an experiment is Chaudoin (2014) who provides respondents with the hypothetical situation of a US government considering an increase in trade barriers. Respondents, given the various informational vignettes, need to answer whether they would like their government to take the described action or not. Other informational treatments consist of individuals receiving

different types of informational content or receiving the same content but framed in different ways (e.g., positive versus negative wording). One such example are experiments making some individuals aware of their personal economic implications of trade liberalization and others not (Rho and Tomz 2017; Jamal and Milner 2019; Schaffer and Spilker 2019).

There exist also some more subtle or maybe more creative ways of providing parts of the respondents with information that other parts of the respondents do not receive. For example, Dietrich et al. (2018) investigate whether the branding of development aid projects can be a way for donors to enhance their reputation. Their treatment as part of an online survey consists of showing a short video describing a development project in Bangladesh. The experimental difference arises because some respondents see the logo of USAID as part of the video while others do not. Interestingly, their results show only a limited effectiveness of such branding. Another example of a visual treatment based on a video is Hopkins (2015). Studying the effect of language acquisition on migration attitudes, he shows respondents randomized video clips, in which an immigrant either speaks perfectly English, broken English, or Spanish. His results challenge a common assumption in immigration studies, namely that the general public prefers well-adapted immigrants. Instead, participants in the experiment are most positive toward immigrants who speak broken English. Hopkins (2015) interprets this as a sign that citizens reward immigrants for their attempt to learn English more than for being able to actually speak the language well. Visual experiments, however, do not need to consist of videos. For example, Naoi and Kume (2015) use pictures of a work environment (e.g., a factory) versus a consumption environment (e.g., a supermarket) to prime respondents toward either their role as workers or their role as consumers in the economy.

By controlling the informational environment in which respondents answer specific questions, experimental research designs also often provide a solution to the so-called “attitudes-on-attitudes” problem (Fordham and Kleinberg 2012). This problem arises in regression analysis if researchers rely on one attitude to predict another and essentially pertains to the question of causal order: In a non-experimental framework, such as a regression analysis using regular survey data, the researcher simply cannot ensure that the specific attitude she is interested in as the predictor variable has indeed been developed prior to the relevant attitude to be predicted. For example, investigating whether individuals with high social trust see trade openness more positively, Spilker et al. (2012) regress one attitude, that is, social trust, on another one, that is, trade attitudes, without being able to ensure that indeed a person’s level of trust has been developed before she formed an opinion on trade. The only way to circumvent this problem is by using an experimental set-up as in Nguyen and Bernauer (2019) who find some support for the relationship to be causal indeed.

Other techniques of survey experiments have their origin in the methodological literature that strives to improve measurement (Mutz 2011). If researchers believe that respondents might not answer truthfully, they can opt for experimental techniques to circumvent these problems. One such technique is the list experiment. In such an experiment the researcher provides respondents with a list of alternatives for which the respondent does not need to say which of the alternatives are true but simply how many of them. The experimental component implies that some respondents see a list with an additional sensitive item included and others see a shorter list without this sensitive item (Mutz 2011). As a consequence, the researcher can estimate the proportion of respondents having experienced the sensitive item by calculating the difference in the number of experienced items between treatment and

control group.⁴ An example is Swedlund's (2017) study on the effect of government repression on donors' willingness to lend to developing countries as she believes that simply asking experts working for development organizations directly on the relationship between repression and allocation of development aid might result in socially desirable answers.

Another way to deal with situations of social desirability are so-called conjoint experiments (Hainmueller et al. 2014). These survey experiments provide respondents with two profiles at once, for example, of potential immigrants, climate treaties, or economic bailout packages, that vary along a multitude of dimensions (Bechtel and Scheve 2013; Bechtel et al. 2014; Hainmueller et al. 2014). For example, in the case of immigrants these dimensions could be age, gender, education, or the job of the potential immigrant. Respondents seeing these profiles are asked to make a choice between each pair of profiles they are seeing as to which one they prefer. Since these profiles vary randomly researchers can estimate the causal effect of each dimension, for example, age versus gender of an immigrant, on the likelihood of respondents favoring particular immigrants. Furthermore, since profiles vary in multiple ways respondents typically cannot systematically assess these variations so as to optimize their answers in a socially desirable way. An additional advantage of conjoint experiments is their usefulness in studying any type of higher dimensional research question. Hence research not only relies on conjoint experiments to study migration but various other IPE phenomena, as distinct as trade agreements (Spilker et al. 2016, 2018) or bailout packages (Bechtel et al. 2014).

Varieties in Implementing Experiments

Next to the specific design of experiments, there exist further differences. One such difference, which to some extent overlaps with the type of experiment, is the way the experiment is administered to the population. While most laboratory experiments are implemented via computers and many survey experiments are conducted online, this is typically not the case for field experiments and also for some survey experiments. Especially, if survey experiments are conducted in developing countries, online implementation is often not possible. Instead, the experiment is administered face-to-face either on pre-prepared and printed questionnaires (e.g., Spilker et al. 2016) or on tablets (e.g., Findley et al. 2017). While the latter approach is similar to experiments administered on a computer, implementing experiments on pre-prepared questionnaires requires careful planning and execution. The most difficult part is to guarantee proper randomization into treatment and control groups, which experimental software easily achieves due to its built-in randomization function. However, if a researcher cannot rely on such software when implementing the experiment, other ways to ensure randomization have to be found. Spilker et al. (2016), for example, in their study conducted in Costa Rica, Nicaragua, and Vietnam on which type of preferential trade agreement (PTA) member countries citizens prefer created all conjoint profiles randomly on a computer before going to the field. These printed questionnaires were then randomly distributed to respondents when actually conducting the interviews.

Another difference pertains to timing. While most experimental studies are one shot endeavors, that is, the experiment is implemented only once, some experiments are implemented over a repeated timespan. For example, Goldstein and Peters (2014) use repeated waves to assess US citizens' attitudes vis-à-vis immigration over the course of the

great recession. Due to this repeated design, they are able to show that high-skilled US citizens become less positive toward high-skilled immigrants once they feel more economic pressure. Another example is the study by Spilker et al. (2020) who are interested in opinion change in the context of trade attitudes. By studying information-updating processes in the context of the negotiations of the Transatlantic Trade and Investment Partnership (TTIP), they need a baseline from which to evaluate opinion change. To do so, they opted for a two-wave design, in which they asked respondents for their general opinion on TTIP in wave one and provided them with specific randomized information on TTIP's potential consequences in wave two in order to investigate under which circumstances respondents change their mind on trade.

Yet another difference concerns the type of actor studied. While most experimental studies in IPE rely on citizens as their target group some research intends to capture the preferences of other types of actors, such as experts and firms. Of course, in reality, it is at the end of the day always a specific person who takes part in the respective experiment. Yet, the intention of some studies is to understand something about preferences of firms, organizations, or experts and not the individual as such.

Several experimental studies investigate firm-level preferences. For example, the field experiment by Findley et al. (2015) investigates compliance with international law. In particular, they wrote emails to more than 1,700 incorporation service firms to ask for confidential incorporation. The experimental component of the study consisted of mentioning in some emails that such incorporation would be against international law while other emails even offered to pay a premium for incorporation. One of their many findings is that mentioning international rules has no effects on compliance for those firms who received the respective treatment. Another example of an experimental study at the firm level is Malesky and Mosley (2018). They investigate whether firms are willing to upgrade their labor rights in Vietnam if this allows them higher markups when selling their products to foreign markets. Similarly, Kim et al. (2019) also investigate firm-level preferences in the context of trade. By relying on conjoint experiments, they assess what types of provisions in preferential trade agreements are crucial for what kinds of firms.

Next to firm-level analysis, there exist a few experimental studies, mostly in the context of development aid, that focus on expert opinion. One example is the above-mentioned study by Swedlund (2017) studying the opinion on development aid of top-level donor representatives in 20 African countries. Another example in the context of development aid is the study by Findley et al. (2017), which, next to an experiment with Ugandan citizens, consists of a survey experiment with Ugandan members of parliament. By comparing the preferences of ordinary citizens with preferences of members of parliament, they can show that while members of parliament tend to prefer projects run by the government, citizens prefer projects funded by international donors.

The final difference I would like to highlight concerns the outcome variable in the respective experiment. Most experimental studies in IPE, especially survey experiments, assess the effect of their treatment(s) on some sort of attitude. This can be whether a person prefers trade openness or protectionism, who should finance a specific development project, whether migration should be restricted or what kind of trade agreement is preferred. Laboratory experiments, especially if they consist of an implementation of a game theoretic model, rely on some form of economic gains to measure their outcome. These virtual gains arising from the laboratory experiment are usually converted to actual money and

paid out at the end of the experiment. However, some survey experimental studies, mostly in addition to assessing an attitude, also rely on some behavioral outcome variable. The above-mentioned study by Findley et al. (2015), for example, is interested in whether an incorporation service replied to their email as one type of outcome measure. Spilker et al. (2020), in addition to asking individuals about their opinion on TTIP as discussed earlier, ask whether respondents would be willing to sign a petition in favor or against TTIP. Signing a petition or writing an email to political representatives are approaches to test whether the experiment mainly assesses merely costless attitudes or whether such attitudes would also translate to real-world behavior.

ROLE OF EXPERIMENTS

To complete this chapter's overview of experiments in IPE, I follow the example of Druckman et al. (2006) who—based on Roth (1995)—outline three non-exclusive roles experiments can play in social science research. These three roles—speaking to theorists, searching for facts, and whispering in the ears of princes—allow me to further outline the variety and diversity of experimental studies in IPE. However, given the strong increase in experimental studies in all fields of IPE, the discussion is not an exhaustive summary of the experimental literature in IPE. Rather, my goal is to highlight different experimental approaches and findings, to illustrate some controversies and to point to some noteworthy studies.

Speaking to Theorists

One important role of experiments is to assess the assumptions or predictions of established theories (whether they are formal theories or not). Thus, the goal in such a setting is to design experiments trying to test whether specific theories or parts of specific theories are valid or not.

To illustrate such an approach, I outline in the next paragraphs a long-standing controversy in the context of the research on trade preferences. With its strong focus of understanding the micro-motives of macro-level phenomenon, as already mentioned, research on trade preferences is strongly characterized by the use of experiments. Experimental research on trade has started as an advancement of the many survey-based but non-experimental studies on testing the two workhorse models used in trade preference research.⁵ In most of these studies people's economic self-interest is centerstage and the scientific debate mainly targeted the question what determines this economic self-interest: is it a person's skill level (as in the factor-endowments, i.e., Heckscher-Ohlin, model) or rather her sectoral employment (as in the specific factors, i.e., Ricardo-Viner, model)? Both approaches have in common, however, that they assume that individuals are motivated by whether they personally gain or lose from trade (Oatley 2015). Hence under both approaches, studies start with the premise that changes in trade policy will have an effect on individuals' trade attitudes by affecting individuals' employment and income. In particular, if trade leads to the loss of one's job or a lower income, an individual should oppose trade openness whereas if individuals experience higher incomes under trade liberalization, they should support trade openness.

The difference between the two approaches—HO versus RV—arises due to the assumption of whether factors are completely mobile or not (Oatley 2015). While the HO model assumes that individuals can move frictionlessly from one sector to another the RV model assumes that this is not easily possible as someone working in the automotive industry might not find it easy to get a job in the health sector. As a consequence, the prediction of who wins and who loses from trade liberalization differs between the two models. Under the HO model, highly skilled individuals as well as capital owners gain from trade liberalization in industrialized countries whereas low-skilled individuals lose from trade openness. Under the RV model, it is individuals who work in export-oriented sectors who gain from trade openness while individuals working in import-competing sectors lose out.

Yet, Mansfield and Mutz (2009) challenge the decisiveness of economic self-interest and argue instead that individual-level trade preferences should be based on collective-level information, that is, on individuals' perceptions of how a given policy has affected the nation as a whole. This does not necessarily need to imply that such "*sociotropic*" preference formation occurs because people are altruistic. Sociotropic preferences can also arise because citizens tend to have difficulties linking their own personal economic situation to public policies and thus use more easily available country-level information, such as unemployment numbers, as information heuristics instead; an important point given the complex nature of trade liberalization (Guisinger 2009; Hiscox 2006). To illustrate, an unemployed person might thus not blame trade liberalization directly for his or her personal situation. Instead, people who notice an increase in unemployment in their communities might hold the government's trade policy accountable for this development, independently of their own employment status (Mansfield and Mutz 2009, 12).

In their own, non-experimental research, Mansfield and Mutz (2009) find strong support for the assertion that sociotropic considerations are important determinants of individual trade preferences. Later experimental studies have tried to test the presumed causal mechanisms underlying this relationship but have come to different conclusions than the original study. In particular, later studies intended to differentiate between the two potential reasons for sociotropic preferences: real altruism and heuristic use of sociotropic information to compensate for individual-level information. Relying on framing-based survey experiments, both Rho and Tomz (2017) and Schaffer and Spilker (2019) can show that the respective wording of how trade is supposed to affect individuals strongly matters for their support or opposition to trade. While this clearly suggests that individuals seem to find it hard to decide whether they themselves belong to the winners or losers from globalization, the findings of both studies clearly show that this is not due to individuals forming such preferences in an altruistic way.⁶ Rather both studies find strong support for the fact that once individuals become aware that they personally win or lose from trade they tend to adjust their support for trade liberalizations accordingly.

Experimental research on *migration* preferences follows a similar trajectory to research on trade attitudes. Also in this field, a strong focus has been placed on better understanding the micro-level foundations of macro-level theories. In line with the standard approach in trade, the literature has theorized that individuals form their views on migration via immigration's impact on wages and employment of native workers. High-skilled immigrants arguably compete with high-skilled native workers and low-skilled immigrants with low-skilled native workers (Scheve and Slaughter 2001b). This type of labor market competition should

then lead to a rejection of equally skilled immigrants by natives because they threaten their economic benefits.

However, non-experimental evidence strongly suggests that both high- and low-skilled workers tend to favor high-skilled over low-skilled workers thus providing limited support for above argument (Hainmueller and Hiscox 2010; Mayda 2006). Using a survey experiment with US employees in 12 US industries, for example, Hainmueller et al. (2015) are able to show that this seems to be indeed a causal relationship: while with increasing levels of education—the typical measure for skills—support for immigration in general increases, still both high- and low-skilled workers prefer high-skilled to low-skilled immigrants.

Two reasons are currently on offer to explain this finding. One follows the same logic as the sociotropic argument mentioned earlier, namely that natives do not form their opinion toward migration on the basis of economic self-interest but rather based on whether the country would benefit from the type of migrants. To the best of my knowledge no experimental test of this sociotropic argument for migration attitudes currently exists.

The second argument for why both high- and low-skilled natives prefer high-skilled migrants might have to do with the fact that high-skilled workers might simply not be exposed to the theorized threats due to economic competition with high-skilled immigrants. If one considers trade and migration effects jointly (Peters 2015), one can derive an explanation for why high-skilled natives might not feel economically threatened by high-skilled migrants. As introduced above, under the HO model high-skilled workers profit from trade openness in industrialized countries since they are the abundant factor in such economies (Oatley 2015). As a consequence, trade creates a situation in which high-skilled workers are in great demand on the domestic labor market. Their relaxed job market conditions might explain why high-skilled workers do not perceive high-skilled immigrants as labor market competition. And indeed, using repeated survey experiments conducted at several time points before and during the great recession, Goldstein and Peters (2014) can show that if high-skilled natives feel more economic pressure their positive attitudes vis-à-vis high-skilled migrants tend to become more negative exactly as one would expect given such argument.

Another example of how experimental studies try to better understand causal mechanisms, pertains to *audience cost* theory (see Tomz 2007; Chaudoin 2014). Audience cost theory argues that citizens punish their elected leaders for actions that are inconsistent with their (international) obligations or promises. If a government leader, for example, promises to intervene in a foreign country if a certain red line is crossed and later does not intervene despite the line being crossed, audience cost theory would suggest that the general public should punish the leader for breaking this commitment. However, what audience cost theory tends to ignore is the fact that citizens also have a substantive policy interest. Chaudoin (2014), in his experimental study on trade barriers, shows that this preference for consistency as implied by audience cost theory only exists for citizens with little substantive policy interest, at least in the field of trade policy. Using a framing experiment embedded in a US survey, Chaudoin (2014) provides some of the respondents with the information that the US government plans to erect trade barriers to protect specific sectors from foreign competition. In an additional treatment, some of the respondents read that this violates the US' obligation under the World Trade Organization (WTO), which is Chaudoin's way to test for audience cost (which is in line with previous studies such as Tomz 2007). By differentiating between respondents with strong interest in trade matters

and those with only little interest, he is then able to show that whether such obligations are in line with WTO rules or not only seems to matter for those with little interest. Chaudoin's study thus is a successful way of refining the causal mechanisms of existing theories using an experimental set-up.

Searching for Facts

In experiments that strive to search for facts researchers try to “isolate the cause of some observed regularity, by varying details of the way the experiments were conducted” (Roth 1995, 22). These types of experiments often intend to assess whether a specific factor indeed influences the outcome in question and if so what the magnitude of this effect is.

Again, I begin illustrating this kind of research by experimental studies on trade policy. In addition to research aiming to better understand the micro-level assumption of the HO and RV models on trade, as discussed in the preceding section, various experimental studies focus on the impact of *non-economic factors* on how people perceive trade liberalization. In this context, experimental studies typically serve to evaluate whether a specific relationship found in previous non-experimental research is indeed causal.

One example are studies investigating trade preferences' cultural and ideological determinants. While observational research has shown that *nationalist and chauvinistic* individuals tend to be more supportive of protectionist measures potentially because of their heightened national pride and feelings of national superiority, this research suffered from the attitude-on-attitudes problem discussed earlier (O'Rourke and Sinnott 2001). Thus, without an experimental research design, the conclusion whether it is indeed nationalist attitudes that are causally affecting trade preferences rested on uncertain ground. However, several experimental studies were able to increase our confidence in this result. For example, Margalit (2012) can show, using a survey experiment, that heightened cultural concerns intensify people's view of trade liberalization as harmful. Similarly, Mutz and Kim (2017), by elaborating on the impact of in-group favoritism, can show by means of a population-based survey experiment that Americans prefer outcomes that maximize in-group well-being over maximizing total gains. Thus, most Americans tend to value the well-being of other Americans more than that of people outside their own country, a tendency which is exacerbated by a sense of national superiority and which sometimes can even become strong enough to make individuals forgo potential economic benefits.

Another example in which experimental research was able to confirm prior purely observational research concerns the impact of *social trust* on individual trade preferences (Kaltenthaler and Miller 2013; Spilker et al. 2012). By combining an interactive voluntary contribution game in a natural field setting in Vietnam with a survey tapping individuals' trade attitudes, Nguyen and Bernauer (2019) can show that, in line with previous research, social trust indeed tends to increase public support for international trade.

In contrast, experimental studies so far have not been able to confirm the observational result that citizens with strong *environmental attitudes* tend to be more protectionist (Bechtel et al. 2012). In their experimental study conducted in Vietnam and Nicaragua, Bernauer and Nguyen (2015) do not find empirical evidence for a perceived trade-off between free trade and environmental protection among the surveyed citizens. Rather, it is observed that people who are aware of and concerned about environmental problems are more supportive

of free trade. This is supported by research of Spilker et al. (2016, 2018) based on conjoint experiments in Costa Rica, Nicaragua, and Vietnam.

Another example is research trying to better understand the so-called *gender gap* on trade attitudes. Existing literature offers considerable evidence that women tend to be more opposed to trade than men even after controlling for various socioeconomic characteristics (e.g., Mayda and Rodrik 2005; O'Rourke and Sinnott 2001). Several potential explanations for this so-called gender gap in trade preferences exist, yet, non-experimental studies face difficulties understanding the exact mechanisms. Two experimental studies have so far tried to shed some light on this. Both, Mansfield et al. (2015) and Guisinger (2016), are able to rebut one mechanism previously found in the literature, namely that women might be more averse toward trade liberalization due to their lower levels of economic education (Burgoon and Hiscox 2008). In contrast, these experimental studies suggest that a mixture of economic self-interest, immobility, risk aversion, and a more negative view on international affairs in general are probably responsible for the gender gap (Guisinger 2016; Mansfield et al. 2015).

Two examples from the rather rare research on foreign investment preferences also illustrate this role of searching for facts. Jensen et al. (2014), relying on survey experiments in the US, can show that politicians can claim credit for inward foreign investment choices and thus can turn such investment decisions into electoral gains. Using various survey experiments, Chilton et al. (2017) investigate the role of reciprocity in foreign direct investment. They show that both citizens in the US and in China are more likely to oppose a foreign investment if the other country has recently forbidden a merger of their home country with one of its domestic firms. The role of reciprocity is also highlighted in research on agricultural subsidies. For example, Jensen and Shin (2014) find that the approval rating for agrarian subsidies increases if the respondent is previously informed that this type of subsidy is less than in comparable foreign countries.

Whispering in the Ears of Princes

This final role of experiments is concerned with the dialogue between experimental studies and policymakers (Druckman et al. 2006). In designing experiments in ways that resemble closely real-world environments, researchers can increase the usefulness or credibility of their research for policymakers. Two examples already discussed that serve this purpose are the studies by Dietrich et al. (2018) on the branding of development aid and by Findley et al. (2017) on the preferences of ordinary citizens versus members of parliaments in developing countries with regard to whether financing comes from international versus national donors.

Also, the various conjoint experiments mentioned throughout this chapter can be seen to serve this role. Introduced in the context of migration, the general idea underlying conjoint experiments is that real-world choices are often not one-dimensional, that is, pro/contra migration or pro/contra trade openness, but rather multidimensional instead. Thus, the relevant question is often not so much whether a person prefers migration or trade openness but what kind of migration or trade liberalization.

In the context of migration, Hainmueller and Hopkins (2015) can therefore show that US citizens prefer migrants who are highly educated and who have jobs with a good reputation to migrants with low levels of education, little command of the English language, and

no clear plans to work. In the European context, Bansak et al. (2016) find that vulnerable migrants who are Christian instead of Muslim and who have a higher likelihood of being employable are the most positively evaluated asylum seekers. In the context of trade, Spilker et al. (2016) exchange potential migrant or asylum seeker profiles with potential trade partners. They can show that it seems to be more important, at least for citizens in Costa Rica, Nicaragua, and Vietnam, whether a country is democratic and culturally similar to the home country than how economically important the potential trade partner is.

Other studies have applied conjoint experiments to the study of policies or international agreements respectively: Bechtel and Scheve (2013) in the context of potential international climate agreements, Bechtel et al. (2014) on Eurozone bailout packages, and Spilker et al. (2018) on potential trade agreements.

CONCLUSION

As the discussion in this chapter illustrates, the use of experiments in IPE research has not only become an integral part of the discipline, it is also a lively and very diverse field. A multitude of experimental techniques are employed varying with respect to the type of experiment, the actor under study, the potential outcome variable, or the role the respective study can play. Despite this variation in experimental studies, they all share a specific approach to tackle research questions. All of the studies subscribe to a clear causal-analytical research framework without which an experimental research design is probably futile.

Although the discussion in the chapter was not intended to be an exhaustive examination of all experimental research existing in IPE, the prominence of some fields, such as trade, migration, or development aid, in contrast to other fields, such as investment or monetary politics, clearly reflects actual variation in how common experiments are in the different subfields of IPE. This has probably not so much to do with a disposition of some subfields in IPE toward experiments which other subfields do not share but rather with a more general characteristic of experimental research. Namely, that only specific questions lend themselves to being examined via experiments. Only if the causal mechanism to be tested can be formulated in a way that can be translated into an experimental context at the individual level are experiments a useful method of analysis. And while this is true for some subfields of IPE, for example, trade, this is not so much the case for other subfields, for example, investment.

This leads me to some more general observations concerning the advantages and disadvantages of the experimental turn in IPE (for a criticism of this turn see Pepinsky 2014). Beginning with the advantages of experimental research in IPE, one clearly needs to stress the ability to establish whether an observed relationship is causal. This advantage of experiments alone makes them an invaluable contribution to the field. In addition, conducting an experiment asks the research to be precise and detailed about the specific relationship to be tested thus making this kind of research both theoretically and empirically very transparent and traceable.

However, there are also certain disadvantages attached to this experimental turn. One disadvantage is what Pepinsky (2014) refers to as the question of aggregation or rather what the right microfoundations are. The plethora of experimental results on topics, such as trade or immigration preferences, raises the question of what all these, sometimes even

contradictory, experimental results imply for our understanding of the macro-level IPE theories. Do we need to discard specific theories, for example, on trade policymaking, if we know from our experimental results that individuals find it difficult to identify their economic self-interest or if factors, such as ethnocentrism, seem more important than economic self-interest? And how to integrate the numerous different experimental studies on similar questions? Looking at the subfield of trade, it sometimes seems as if the many experimental studies each provide some very interesting, yet, often rather detailed contributions to the literature but the bigger picture of what all this implies seems to be currently missing.

Looming behind these questions is a probably even bigger question of what researchers in IPE really mean when they talk about microfoundations of their theoretical models (Pepinsky 2014). To illustrate this point, I would like to return to the example used at the beginning of this chapter to elaborate on the potential outcome framework. The question whether democracies in contrast to autocracies attract more foreign investment has no clear *individual-level* microfoundations although it clearly rests on a theoretical foundation that is less aggregated than the macro-level since it spells out assumptions on firm-level behavior. Of course, one could design an experiment to test whether individuals prefer their home country firms to invest in democracies rather than in autocracies (for non-experimental studies in this direction see Kaya and Walker 2012 and Pandya 2010) yet this would not be the “correct” microfoundation in this context. Rather, one either has to accept that experimental research is not the right tool in certain contexts or researchers need to invest resources in experimentally investigating the preferences of firms, which are the relevant actors in this case. As valuable as the latter type of research is, as studies such as Malesky and Mosley (2018) illustrate, it is also clearly much more difficult, time-consuming, and probably also more labor-intensive to implement experiments in the context of surveys with firms, organizations, or experts. Additionally, some research questions, such as whether the WTO or preferential trade agreements increase trade flows (Rose 2004; Tomz et al. 2007), might simply be unsuitable for experimental research.

This leads me to a last observation trying to connect the content of this chapter to the question raised in the introductory chapter of this handbook as to whether the field in IPE is one or rather two? In my opinion, IPE scholars using experiments by subscribing to a common causal-analytical framework can be attributed to what the authors of the introductory chapter call the American school. The reason for this is that experimental research designs really only make sense if subscribing to a causal-inference perspective. As a consequence, however, not only does the field of experimental research in IPE have a tendency to focus on the microfoundations without constructively engaging in how to aggregate the different experimental findings in order to come to a broader macro-level argument, as criticized by Pepinsky (2014, 2019). But with its strong focus on causal-inference and the therefore almost exclusive use of experimental methods in the American school tradition, the tendency for “constructive non-engagement” with scholars of different IPE traditions might be even more pronounced.

NOTES

1. In this chapter, I follow Morton and Williams (2010) and Margetts and Stoker (2010) and consider such research designs to be an experiment, in which the researcher has control

- over the data generation process. In addition, I follow Margetts and Stoker (2010) and consider only experiments in which intervention by the researcher took the form of a randomized allocation into different groups. Hence this does not, e.g., cover so-called natural experiments (Bechtel and Hainmueller 2011; Kern and Hainmueller 2009).
2. E.g., to understand fairness concerns in climate change politics, Gampfer (2014) implements a version of the dictator game, in which individuals are randomly allocated to be either the proposer or the receiver of a deal that allocates the potential costs of climate change. In this setting, participants play against each other and Gampfer (2014) can draw conclusions on what is considered fair in climate politics by analyzing which kind of proposals the receivers accept and which not and under which circumstances.
 3. SUTVA, as Morton and Williams (2010) discuss, is not one single but rather a collection of assumptions. In particular, they summarize, in accordance with Rubin (1980), the following two assumptions to be crucial: “(1) that treatment of unit i only affects the outcome of unit i (thus it does not matter how many others have been treated or not treated) and (2) that, for ATE and ATT, the treatment is homogeneous across voters” (Morton and Williams 2010, 71).
 4. Similar to the list experiment, endorsement experiments strive to measure support for specific actors, e.g., terrorist groups, for which honest or unbiased answers are unlikely to be obtained. In such experiments, the researcher compares support for different policies, statements, etc. once mentioning a specific endorser, e.g., the terrorist group, and once not mentioning this endorser. By comparing the rate of support of the policies once with and once without endorsement the researcher can estimate support for the respective endorser. One example is Lyall et al.’s (2013) study on the support for the Taliban in Afghanistan.
 5. For an overview of the literature on trade attitudes more generally see Nguyen and Spilker (2019).
 6. This is in some contrast to Jamal and Milner (2019) who show for Tunisia that individuals can understand fairly well the impact of trade liberalization for their own economic well-being.

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CHAPTER 11

NETWORKS IN INTERNATIONAL POLITICAL ECONOMY

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THE use of network analysis in International Political Economy (IPE) scholarship is a variegated phenomenon that has a long history, owing largely to the interdisciplinary nature of the field. Networks have been used to study economic organization at the international level at least since the early twentieth century, in the form of the first corporate interlock studies. The use of network analysis to the study of the changing organizational environment arose again in the 1970s, as mathematical graph theory was further developed and the re-emergence of global finance gave rise to greater institutional complexity.¹ Fennema and Heemskerk (2018) argue that the use of computer-assisted network analysis emerged independently in a variety of small groups around 1970 as an instance of Mertonian multiple discovery. This occurred, they argue, in two sites in the US (California and Stony Brook) and in the Netherlands (Amsterdam). The effort, Fennema and Heemskerk (2018) argue, was deeply politically motivated in each case, by the desire to push for a research agenda on corporate power and to create a terrain of legitimate research. Today the use of network analysis is ever more widespread, including a range of different topics and modalities of using network science for the study of IPE. As such, a range of network methods have been developed to analyze many topics in IPE scholarship, from transnational trade and financial networks among national economies, to ties between individuals in corporate, institutional, and formally political settings.

The past decade in particular has seen a proliferation of IPE research that utilizes network science methodologies. There are two reasons for this. The first is straightforwardly technological: developments in network science have established a richer suite of empirical techniques than was available previously, while advances in computing power have allowed researchers to implement them even in large-*n* contexts favored by many IPE researchers. Prior to these developments many network analyses were limited to descriptive explorations of limited data, but now a full toolkit of network methods—inferential as well as descriptive—are well-implemented in statistical software. The computational demands

are still intense, but the proliferation of supercomputers on many university campuses, plus cloud computing, make complex analyses much more feasible than in the past. Just as early advances in computing technology led to a surge in statistical regression analysis, more recent advances have opened up opportunities for more complex modeling of networks.

The second reason is related to developments within IPE itself.² Since the global financial crisis that began in the American real estate sector in 2007, there has been an increased demand for theoretical and methodological approaches that can investigate structures of interaction, and explore complex interdependencies, in the global economy (Winecoff 2020; Lake 2021; Cohen 2009; Keohane 2009; Oatley 2011, 2019; Oatley et al. 2013; Winecoff 2015). While a variety of theoretical and empirical tools have been used to explore these “new structuralisms” and “new interdependencies” (Farrell and Newman 2019; Keohane and Victor 2011; Winecoff 2017b), network analysis is particularly well-suited for this task. Network models do not require the assumptions of independent and identically distributed observations that are central to linear regression models, yet network methodologies satisfy many contemporary demands for empirical rigor in modern social science. Relatedly, the use of network analysis incentivizes scholars to shift their analytical focus to global structures and interpersonal relations, rather than modeling the global economy as a set of discrete national economies occupied by atomistic firms and individuals. Indeed many of the key concepts that IPE scholars use—such as structural power, complex/weaponized interdependence, embeddedness, regime complexes, and institutional proliferation, as well as prominence and status—can be analyzed using modern network analytical tools, allowing researchers to employ a methodological (and theoretical) toolkit that more closely aligns with the core assumptions and arguments of traditional concepts and theories in IPE than traditional statistical methods that assume observational independence.

In what follows below we describe the usefulness of network science to the study of IPE, before turning to a variety of topics where network analysis has been deployed within IPE. We focus in particular on the use of network analysis within the study of international trade, finance, networks of ownership and control, and global elite networks.

THE UTILITY OF NETWORK SCIENCE TO THE STUDY OF IPE

Network analysis is often characterized as one particular quantitative method, which co-exists amidst a suite of methods that scholars may apply to various empirical research problems. Network analysis is more than that, however, in ways that are particularly meaningful to the present and future of IPE scholarship. The value of network science comes from its emphasis on structural interdependence in addition to the characteristics of the units (people, firms, nation-states) being analyzed. Indeed, the discipline of IPE itself has been analyzed as a network: of collaborations, citations, and pedagogical strategies (Seabrooke and Young 2017; Clift et al. 2022).

It is hard to think of any topic in IPE that does not involve meaningful interdependence between units. Market competition is, after all, an interdependent process, and joint membership in international organizations is definitionally relational. Linkages across national

economies—trade, investment, multinational production, migration—are not only ubiquitous but increasing in importance as the global economy evolves (Oatley 2019). Climate change and global governance are of collective importance and require collective action. The connections between individual people—on corporate boards, via revolving doors, as members of epistemic communities—are of increasing salience in an era of rising inequality.

Network analysis is well-suited for analyzing these communities and systems. By enabling the researcher to explicitly model complex systems, including those with high levels of endogenous feedback, network analysis provides us with a way to capture phenomena that we are often able to describe but not systematically measure: *political-economic structures*. By “political-economic structures,” we refer to the patterns of agential interaction that collectively create larger, emergent systems that condition actors’ subsequent behaviors, which then potentially change the structure. Such systems are known to be “complex adaptive” (Miller and Page 2007). The “adaptive” part refers to agency, as actors tailor their actions according to the structural context within which they exist (Winecoff 2017b). This structure is “complex” if the distribution of linkages in the system has some impact on the performance of the system (by, e.g., incentivizing some forms of agent behaviors while dissuading other forms) (Oatley et al. 2013).

Thus, network analysis allows us to analyze agents and structures in a holistic framework. This has long been a priority for social science, which recapitulates the same analytical issue: how to make sense of the role of agency within structural constraints, and the reciprocal relationship between the two. The agent-structure dynamic is fundamental to nearly all social science (Wight 2006). Network analysis does not fully resolve it, but it does provide a set of tools that facilitates a rigorous exploration of the unit-system interplay both theoretically and empirically. As a result, in many situations network analysis provides analytical leverage for researchers who would rather explore the agent-structure interaction directly, rather than assume away one of the two—usually structure—to “unpack a black box” at a single level of analysis.

Doing so allows researchers to create more complex research designs, including models that include endogenous growth mechanisms (something commonplace in macroeconomics since Romer (1994) at least, but still foreign to much IPE), as well as non-linear dynamics such as those associated with new trade theory (Krugman 2008), and preference/productivity heterogeneity—which may often be attributable to heterogeneity in network position—associated with “new new trade theory” (Kim and Osgood 2019; Winecoff 2017b). Such designs have the advantage of being capable of exploring how interdependent systems emerge; how they affect actions by, and outcomes for, agents; or both, in a holistic manner. These tools are more and more essential for IPE scholars, as the organization of global economy becomes more complex, more interconnected, and more feedback-driven than ever before.

While it is important for IPE to consider the applicability of the wealth of network theory (both formal and informal) from sociology and systems science for IPE research, the ability for network analysis to empirically render interdependent structures makes it appropriate for already-existing IPE foci on, for example, complex interdependence, core-periphery dependency, structural power, hegemony, zones and roles in the world economy, production and investment chains, epistemic communities, business-government linkages (via interpersonal ties, campaign donations, lobbying, or other interactions), institutionalization generally and “regime complexes” specifically (such as those governing the environment

and trade). These can all be empirically rendered using network analysis, in ways that other methodologies arguably cannot match.

Many concepts that are the bread and butter of IPE research can be represented through network concepts and measures. For example, “hegemony” can be defined as structural prominence within networks. The network concept of “homophily”—the tendency for like to connect to like—is analogous to, but often more analytically precise than, arguments about why regime-type similarity should predict trade patterns. The phenomenon of “preferential attachment” is a concept related to two central phenomena occurring simultaneously—increasing returns and path dependence, which can generate “rich get richer” dynamics—that are fundamental to the study of the organization of cumulative advantage at the levels of state power, wealth accumulation, multinational industrial organization, etc. The tendency for real-world networks to contain high levels of “transitivity”—the tendency for a friend of a friend to become a friend, a process that produces network “communities” of tight interdependence—can help inform IPE scholarship on how and why particular patterns of economic activity occur at global, regional, and sub-regional strata. Units in the system that provide these pathways for k -adic interaction often occupy “bridging” or “gatekeeping” roles that can confer contextual forms of power and influence. These are merely a few ways in which network analysis can help scholars analyze interdependence, a fundamental concept to all IPE scholarship, and the very thing being modeled and measured in network analysis.

As with all quantitative methods, network analysis includes both descriptive and inferential tools.³ Until recently the descriptive methods were predominant, as they were less computationally intense to calculate but could still reveal interesting patterns of structural prominence, the existence of hubs and communities, and k -adic properties (such as the prevalence of triads in the network). Nevertheless, these methods were not very commonly used in IPE by comparison with other quantitative approaches that emphasize inference via statistical hypothesis testing; as such, it was not uncommon to hear network analysis described as “pretty pictures”: perhaps interesting, but from which it was difficult to learn much about the relationships between variables of interest.

By the early 2010s, however, inferential models were well-implemented in commonly used statistical software (R and Python in addition to specialized network software such as UCINET and Gephi), and computational power was much more accessible: personal computers could run these models on small samples of data, while university supercomputers and commercial services (such as Amazon Web Services) could handle larger datasets. As a result, the use of these methods increased exponentially in the ensuing years, and now constitutes a substantial body of literature (much of which is surveyed below).

While a heavy toolkit of network methods already exists, there are new developments and frontiers yet to explore in both the descriptive and inferential domains. Descriptively, work proceeds on developing optimal ways to partition networks into communities with internal dynamics that are distinct from the network as a whole, using blockmodeling and other community detection algorithms.⁴ New algorithms for detecting node prominence in very large networks are also being developed (Young et al. 2020), as are methods for uncovering the generative models of network evolution, including complex adaptive processes (Denny 2016). In all of these cases methods exist for networks with both binary and valued links, with links that are directed or undirected, and both static and time series networks. The looming

frontier is the analysis of multiplex networks—a.k.a. “networks of networks”—which exist in multiple dimensions, which has yet to be pursued to a substantial degree in IPE (although implementable methods do now exist in software).⁵

A wide range of inferential network models also already exists, and many more are in development. All of these allow for the inclusion of node-level attributes (e.g., a country’s political regime type, level of GDP, or strictness of regulation; an individual’s level of education, exposure to trade, or geographical location) in addition to network characteristics such as homophily (“like attracts like”), triadic closure (“friend of a friend becomes a friend”), density (observed vs potential overall dyadic connectiveness), preferential attachment (“rich get richer”), etc. The simplest inferential network model is a traditional regression with the inclusion of descriptive network statistics (e.g., node centrality) as regressors. While this is arguably better than neglecting interdependence as a relevant variable entirely, it is also difficult to capture complex structures of interdependence in a regression framework (which assumes observational independence). For that, we need methods designed for that purpose. The exponential random graph model (ERGM) family is among the most used and the most flexible. This model class is analogous to logistic regression when applied to networks with binary ties, but differs from logit in that interdependence exists in the functional form of the model (so that the outcome is not the collection of [0,1] outcomes, which are assumed to be independent from each other, but the realization of the entire network). ERGM models have been extended to time series networks (Temporal ERGM, or TERGM) and non-binary links (Generalized ERGM, or GERGM), and many other data forms.⁶ Latent space models, meanwhile, estimate the likelihood of two entities becoming connected based on how far apart they are in the latent space (including node-level covariates). The Quadratic Assignment Procedure (QAP) is not a distinct model per se, but a test for complex dependence within a regression framework, which adjusts for network interdependence of dyads. Cranmer et al. (2016) elaborate on these models, comparing their strengths and weaknesses.⁷

Innovation in network science is occurring at a rapid pace. At this stage, inferential network methods are sufficiently well developed to satisfy the demand for empirical rigor in contemporary IPE. Implementation of these methods—descriptive and inferential, cross-sectional and longitudinal, complex and multiplex—into IPE is still at a very early stage, however. There remains much low-hanging fruit yet to be picked, and many advances yet to be made, notwithstanding the excellent research which we survey below.

THE USE OF NETWORK ANALYSIS IN IPE

Quite a lot of IPE research using network theory and/or method has involved the exploration of cross-border economic flows (e.g., trade, investment), corporate ownership structures, and participation in international institutions (especially intergovernmental organizations).⁸ In this section we survey many of the key contributions in these domains. One of the repeated findings in these domains is that outcomes in the observed world are often highly sensitive to network interdependencies. If these are not properly accounted for in empirical analyses the result will often be inferential bias, in an unknown direction and to an unknown degree, particularly in quantitative research designs (Oatley 2011; Ba 2021;

Winecoff 2017a, b). Taken together, this research suggests that much more network analysis of many traditional IPE topics is needed.

We begin by a review of some of the IPE scholarship on international trade, before turning to the financial system and then other areas where network analysis has started to make a mark on IPE research.

TRADE AND TRADE INSTITUTIONS

Some of the earliest transnational political economy networks to be analyzed involve the global structure of trade. In 1942, the League of Nations published a report on *The Network of World Trade*, which was expanded upon by Hilgerdt (1943), in which it was noted that “International trade is much more than the exchange of goods between one country and another; it is an intricate network that cannot be rent without loss” (League of Nations 1942, 7). The analysis was purely descriptive and showed that even by that point global trade was not an unconnected collection of dyadic exchange relations, but a structure of mutual interdependence.

In more recent years the level of economic complexity in the world economy has increased remarkably, so it is not a surprise that studies of the political economy of trade in IPE and cognate fields have used network analysis at an increasing rate. For example, Lazer (1999) argued that the process of triadic closure had a major impact on the proliferation of trade openness in Europe in the 1860s, which set the course for the rise and fall of most-favored nation agreements until the Great Crash in 1929. De Benedictis and Lucia Tajoli (2011) render the trade network over time, and implement network statistics into the classic gravity model of trade from international economics. Ward et al. (2013) go further, combining the gravity model with latent space network model, and uncover dependencies in the trade system that the classic gravity model would miss. Gray and Potter (2012) showed that countries at the core of the global trading system experience less macroeconomic volatility than those in the periphery. More recently, Garcia-Algarra et al. (2019) present a stochastic generative model of world trade, while Winecoff (2020) used value-added trade data to descriptively analyze changes to the structure of world trade, finding that China’s rise to structural prominence is real but limited, and has mostly come at the expense of Europe and East Asia rather than the US, so the probability of a hegemonic power transition in the global economy may not yet be imminent.

A substantial body of research has also examined the network of preferential trade agreements (PTAs). The first wave of this research was published in the mid-2000s, as Hafner-Burton and Montgomery (2006, 2009) link network prominence—in IGO membership generally and PTAs specifically—to distinct forms of power than influence the prevalence of conflict and sanctions activity (respectively) in world politics.⁹ Manger et al. (2012) examined the development of the PTA network over time using a SIENA model,¹⁰ finding that the PTA structure exacerbated inequalities in the international political economy, as it had developed into a hub-and-spoke system (with a sharp core-periphery structure). While developments in the subsequent years have changed that picture somewhat, there remains strong positive feedback in the PTA network, in addition to hyperdyadic processes (Brazys and Winecoff 2016). Park and Kim (2020) show that network positioning within PTA

systems is an important driver of export growth, particularly by enabling access to global value chains. More recently, global value chains themselves have been analyzed as networks; Alves et al. (2022) show that the 2007–2009 global financial crisis led to a substantial re-wiring of global value chains, with China emerging as the new core (partially displacing the US). As the global system of production and exchange continues to become more complex, studies in this vein will become even more vital for understanding the dynamics of change in the politics of the global economy. Indeed, as we write this essay the global economy is reacting to supply chain disruptions following from the COVID-19 pandemic, while policymakers become increasingly concerned about the return of inflation after decades of “great moderation” (at least in some economies). These outcomes are unexplainable without some conception of the networks that organize and structure the global economy.

INTERNATIONAL FINANCE AND INVESTMENT NETWORKS

On February 24, 2009, at the height of the crisis that began in the American mortgage market, President of the United States Barack Obama addressed a Joint Session of Congress to detail his plans for getting out of the crisis. In that speech he said that “The concern is that if we do not re-start lending in this country, our recovery will be choked off before it even begins. You see, the flow of credit is the lifeblood of our economy” (Obama 2009). The metaphor is a common one in political economy, where “flow of funds” analyses consider the continual flow of money and credit to be essential for economic growth. It is easy to understand how finance could be productively studied using network methods, which are ideal for capturing resource flows across interdependent units.

Financial institutions have tended to be key organizational vehicles by which corporate elites connect to one another. The flip side of this is that financial institutions play critical roles within inter-firm networks. Since the global financial crisis, many economists and central bankers have been intensively studying financial networks, in recognition of financial interconnectedness. Concepts used by financial regulators around the world such as “too interconnected to fail” and “systemically important financial institutions” all have an implicit network-relational component. Some attempts to model systemic risk along the lines of the “financial ecosystem” (Haldane and May 2011) represent recognition that financial instability does not simply emerge from formally contracting firms but rather are informed by the social relationships within which those firms are embedded. Earlier attempts to bring complexity science into the study of finance represent important precursors (New York Fed 2007). The reference to elements of the financial system as a financial “ecosystem” have become more and more commonplace (Wilkins 2016). This takes the form of urging “ecosystem thinking” (O’Kelley and Farah 2017) for and by financial industry voices, conceived of as a network of interlinked firms (Accenture 2019, 8).

Thus it is not a surprise that IPE scholars who research the global financial system would use network analysis extensively. A recent stream of such research began following the subprime crisis, which contributed to the severity of the Great Recession. This event caused significant introspection within the field of IPE,¹¹ and scholars seeking to move beyond the

“reductionist” mode of Open Economy Politics (Lake 2009; Oatley 2011) were attracted to network science as a tool for understanding global interdependencies. This generation of scholars was increasingly attached to *complex* network science—which emphasizes the importance of hubs, communities, bridges, and endogenous structural processes (e.g., preferential attachment) in guiding network evolution—in addition to traditional network considerations like homophily, reciprocity, and triadic relations.

Some of these studies were framed in terms of understanding the subprime crisis, and its aftereffects, as potential for a structural reorganization of the world economy and the power distribution within it (Ba and Winecoff 2019; Bauerle Danzman et al. 2017; Fichtner 2016; Oatley et al. 2013; Winecoff 2015). Others began applying similar logics to the global monetary system (Khanna and Winecoff 2020), the development of roles theory as a tool for examining global financial networks (Khanna 2021), and the offshore tax havens system (Garcia-Bernardo et al. 2017; Dominguez et al. 2020). In all of these papers the authors emphasize the importance of network structure as a powerful determinant of financial flows in the international system, and emphasize the ways in which those who occupy key roles within this structure have out-stated influence over developments within it (e.g., for financial stability, for macroeconomic growth, for development, and for tax avoidance). In many cases these authors emphasize the ongoing central role of the US in international finance, which did not decrease—and by some measures actually *increased*—following the subprime financial crisis.

The rapid proliferation of this research in this area indicates that network analysis is very well-suited for studying the politics of global finance. The results from these analyses of the financial system are important. Fichtner (2016) generated the first detailed description of the relationships underlying the Cayman Islands in the international financial system, finding that both the US and Japan play a massive role in this OFC. After examining these flows in a network context, Fichtner concludes that the Cayman Islands are so important to the US that they should be considered part of the Anglosphere of international finance. For example, Fichtner found that the Cayman Islands were the largest holder of US securities in the world, and this is primarily driven by hedge fund activity, with about 60 percent of global hedge fund activity being centered there.

Garcia-Bernardo et al. (2017) build large networks of 98 million firms connected through 71 million ownership ties, which allows them to investigate the structure of financial offshoring practices around the world. While many depictions of offshore financial centers point to “sinks” that attract and retain foreign capital, Garcia-Bernardo et al. (2017) find a significant role for “conduit” OFCs which act as intermediate destinations in the routing of international investments and enable the transfer of capital without taxation. What is significant about these findings is the location of both kinds of OFCs—rather than being located in small islands that are difficult to regulate, many sink and conduit OFCs are highly developed countries with advanced regulatory infrastructures, such as the UK, Ireland, the Netherlands, Switzerland, and Singapore.

Oatley et al. (2013) find evidence for the continued hierarchical nature of the international financial system. This is reiterated by Winecoff (2015, 2020), who argues that the concept of structural power—as proposed by Susan Strange—can be empirically analyzed using network methodologies. This stream of research demonstrates that a “fitness plus preferential attachment” (FPA) model provides a plausible explanation for how international hierarchy has evolved over time. The FPA model argues that the internal characteristics of national

political economies—their “fitness”—provides an initial advantage in attracting links in the network, which is reinforced over time via an endogenous “preferential attachment” process that is analogous to a “rich get richer” effect. Network analysis shows the redistribution of structural prominence underway in world politics, highlighting how some emerging market economies are increasing their relative network importance, but this is largely at the expense of Europe and Japan, rather than the US, so that speculation over the decline of American financial hegemony appears to be very premature.

Another area in which network analysis has been applied has been in the area of inter-governmental treaties that secure bilateral investment treaties (BITs). These are legal mechanisms to grant extensive property rights to foreign investors, and have grown considerably in recent decades. Elkins et al. (2006) use network analysis measures to explain the widespread adoption of BITs by international competition among developing host countries for foreign direct investment (FDI). Elkins et al. (2006) examined the causal drivers of signing BITs and found that it was related to competitors doing so, rather than cultural explanations, with coercion and learning playing only a small role. BITs have proliferated because of competitive economic pressures among developing countries to capture a share of FDI flows.

Bandelj and Mahutga (2012) use BITs as a proxy for evaluating the evolution of the world system. They analyze BIT network data over a 50-year period and find that the BIT network is both increasingly dense and increasingly centralized over time. Their BIT network data is used to evaluate evidence for different operational tendencies within the global political economy. They find, surprisingly, that the supposedly contradictory forces of universalism, regionalism, and the North-South divide have been coexisting over this period. Among their interesting findings is that the structure of the BIT network reveals something about the relationship between BITs and FDI. While existing scholarship at the time had suggested that BITs bore a surprisingly weak association with FDI, Bandelj and Mahutga (2012) found an explanation for that weak link, based on the differences in the availability of capital between countries. Countries that lack capital sign on to BITs with other countries that also lack capital. In addition, while most global FDI flows are between rich capitalist countries, BITs are rare among such countries.

NETWORKS OF OWNERSHIP AND CORPORATE CONTROL

Network analysis has been a useful descriptive tool for ascertaining patterns that are difficult to understand or visualize because of their complexity. One of these areas that has been of interest to IPE scholarship is that of networks of ownership and control.

This goes back a long time. In the early twentieth century, Otto Jeidels (1905) was the first to study the network of interlocking directorates of the six leading German banks, and their interlocks with large firms in the iron and steel industry. Jeidels pointed to a practice whereby banks engage in a “systematic policy of placing their representatives on boards of directors” of non-financial firms (Jeidels 1905, 180). Empirically, Jeidels showed evidence of enormous bank representation on the boards of industrial companies, with the largest six

banks in Germany holding 751 board seats in industrial companies. The fact that a few banks created 1350 board interlocks with industrial firms did not go unnoticed by other scholars trying to make sense of capitalism's evolution at the time.¹²

Soon after, Rudolph Hilferding (2010) showed that German banks centralized power across corporate boards. Cartelization of industry, Hilferding observed, was being matched by cartelization of finance. Industrial activity was still the centerpiece of productive activity, but production and industrial profits were not the primary driver in the organization of capitalism: "finance capital" was. This was an argument that Lenin and others picked up to argue for the degenerative tendencies within capitalist development, arguing that capitalist monopolies inevitably led to the "domination of a financial oligarchy" (Lenin 1999). Financial instruments allowed banks to control other kinds of firms; it allowed them to fleece the public; facilitated imperial ventures; and most importantly they transformed the state. These early twentieth-century scholars appreciated differences across national jurisdictions, yet they also saw parallels between countries. Hilferding observed similar patterns of financial ownership and control in the US as in Germany; the patterns that Lenin explored in Germany he also saw occurring in the UK and in France.

More recently, IPE scholarship has examined patterns of ownership and control using more advanced methods and data. Haberly and Wójcik (2017) challenge the conclusion that the globalization of ownership has diffused ownership through market-based mechanisms. They show that about three quarters of the world's largest firms are linked to a single global company network of concentrated ownership ties. Rather than diffuse ownership, this system is extremely hierarchical and with surprising elements: US fund managers are at the core—as in Fichtner et al. (2017)—but there is also a state capitalist periphery in this network that is geographically diverse. Drawing from Polanyi's observation that *laissez faire* was an organized state-driven set of arrangements, Haberly and Wójcik's (2017) analysis directly challenges the concept that the architecture of global finance operates as a marketplace, not just because of its pervasive hierarchy but also because of the role of states as investors. This work on state capitalism, and the role states play within global investment and ownership networks, has been extended since, particularly by Babic et al. (2020) and Babic (2023).

Brancaccio et al. (2018) find evidence of a highly centralized global network of corporate control, consistent with earlier scholarship suggesting massive concentration in corporate ownership (Vitali et al. 2011). Within their large global network comprised of firms owning at least 5 percent of shares in other firms, Brancaccio et al. (2018) find that approximately 80 percent of total network control is held by between 1 percent and 2 percent of all firms.

These general trends regarding centralization of ownership is consistent with other scholarship. Recently, Fichtner and Heemskerk (2020) argued that the shift from equity investment via active portfolio management into passive index funds (such as ETFs) has the potential to transform the nature of shareholder capitalism that has been dominant for the last three decades in a way that empowers management and reduces industry competition (thus potentially increasing inequality and oligopoly). Petry et al. (2021) find that this situation affords special powers to heretofore underappreciated actors, such as index providers, with consequences for corporate governance. Babic et al. (2020) deploy a large-scale analysis of the weighted ownership ties around the world to investigate transnational foreign state-led investment, which they find to be an increasingly powerful structuring force in the global economy. Ajdacic et al. (2021) investigate the complexity of corporate structures based on

financial subsidiarity networks, in order to uncover how accountancy firms act as a “wealth defense industry” in shifting profits around the world.

GLOBAL ELITE NETWORKS

Another, related, area where IPE scholarship has deployed network analysis extensively has been in the study of individual elites and elite networks, in particular corporate elites and the study of so-called “global elite networks” (see Henriksen and Seabrooke 2021). Elite social networks are important because they help to facilitate the social relationships that sustain broader governance practices, from the formal organizational moves we recounted above to informal governance practices. Elite social networks help to explain why, in the face of uncertainty and potential conflict, governance practices within finance (in particular) are not as fractious and divisive as might be expected in a competitive capitalist economy. By linking together highly prominent individuals with mutual and reciprocal ties, networks facilitate a sense of shared norms as well as recurring encounters that help to forge common identities and strategies for political action (Tsingou and Seabrooke 2009; Carroll and Sapinski 2010; Murray 2014; Kentor and Jang 2004; Young 2015). This does not mean that all identities and ideology are unified among elites, but rather they are arguably more unified than they would otherwise be, in the absence of these connections. For example, both Chalmers and Young (2020) and Young and Pagliari (2021) find that firm embeddedness in business networks provides a better explanation for political action on financial regulation than firm-level explanations alone.

A regularity within a lot of scholarship is that there exists differential prominence or stratification within the global corporate elite. Individuals who sit on multiple boards tend to be more politically active and class conscious (Useem 1984), a concept called the “inner circle” effect. This property of elite stratification has been explored in a range of literature, most recently for example in Heerwig and Murray (2018), who investigate whether changes to the US economy have altered the political orientation of corporate elites in terms of advocacy for class-wide interests. They find both that corporate elites are more politically active and unified relative to the past, and being a member of the inner circle—which they measure by weighted centrality, an aggregation of network ties—has a significant moderating effect. Global elites may also be stratified on the basis of traits such as gender and race, which has been recently explored in Young et al. (2020). They find that there are distinct differences between the core and the periphery of global elite networks with respect to group-level representation. White males are much more strongly represented in the core of the network than the periphery, while this is not true of White women. Other groups—for example, Black women and South Asian women—are barely present in the core of these global elite networks.

Individuals sitting on multiple corporate boards simultaneously are very often working for financial firms—a regularity that has traditionally facilitated finance’s centrality within the corporate community. Financial firms are thus understood to influence political behavior through their mediating role with other firms, a point made within scholarship explaining patterns of interest group coordination within the business community (Pagliari and Young 2016; Young and Pagliari 2017). Mizruchi (1992) found that such indirect ties

between firms contributed significantly to defining “the structure of corporate political action.” While Mizruchi examined the campaign contribution activity of firms through political action committees and corporate testimony before the US congress, more recent scholarship has extended this kind of analysis of the political coherence of corporate actors to international activity. In particular, Murray (2014) has examined patterns between corporate interlocks and political action of foreign firms in US politics. Murray assembled the largest 500 firms in the world between 2000 and 2006 and compared these firms’ location in the interlock network with the political spending activity of these foreign firms—through their subsidiaries, divisions, or affiliates—and found a close relationship. Murray found that firms highly central to this international elite system contribute more money to US elections than do less central firms; and furthermore he found evidence that political action is coordinated via these international networks.

Seabrooke and Tsingou (2021) examine revolving doors in international financial governance, looking at the Basel Committee on Banking Supervision and the staff of the IMF’s Financial Sector Assessment Programs (FSAPs). Network analysis is used to visualize the prominence of staff working on FSAP programs within rich countries. Like some other recent work on elites (Ellersgaard et al. 2019), Seabrooke and Tsingou use sequence analysis to trace career histories as their main analytical technique, and find that within elite policymaking circles—but less in IGO policy teams—there is substantial evidence for the revolving-door intuition.

For some, there has been declining support for the idea that financial firms sit at the most prominent positions within inter-firm networks. This position usually accompanies the notion that corporate elite networks at the national level have become more fragmented (see Mizruchi 2013; Chu and Davis 2016). This is somewhat unexpected from the perspective of the “financialization” trend that has been documented in a variety of countries, and indeed around the world (see van der Zwan 2014; Witko 2016; Maxfield et al. 2017; Young 2015; Stockhammer 2004). Recently, Banerjee and Murray (2021) find that financial firms no longer have more of a broad political interest than do nonfinancial companies, but that mechanisms nevertheless still exist for broader class-wide interests. Specifically, they point to two factors. First, a firm’s embeddedness in corporate networks and in policy-planning networks are associated with broader lobbying activities. Second, firms with more global operations have greater breadth of their lobbying activities. Consequently, while banks may have stepped out of an important role building cohesion within the corporate elite, alternative mechanisms continue to fulfil this role (see also Banerjee 2020).

Network analysis has been deployed extensively to adjudicate the longstanding theoretical issue of whether or not a “transnational” capitalist class exists. Corporate board interlocks are commonly used in such analysis, often combined with membership in transnational policy planning groups such as the World Economic Forum, the Bilderberg conferences, and the World Business Council on Sustainable Development. These groups are important to include, it is claimed, because they are the organizations in which hegemonic strategies and norms around how the global economy should be governed are fomented (see Carroll and Carson 2003). The importance of this approach is also reflected in Mirowski and Plehwe (2015) and Plehwe et al. (2018). These emphasize the important of transnational epistemic communities (such as the Mont Pelerin Society) and think tanks in generating and propagating neoliberal ideology and austerity advocacy. Paterson et al. (2014) examine how policy ideas about carbon emission trading circulate among elites transnationally.

Since scholarship has begun to focus on these ties, there has been disagreement about forms of measurement and how to interpret them. Carroll and Fennema (2002) found that between 1976 and 1996 an Atlantic business system developed but Japanese firms were not integrated into this network. Kentor and Jang (2004) found a greater number of transnational directorate interlocks between 1983 and 1998 among the Fortune 500 largest firms in the world (by revenue), and conclude that there has been a dramatic growth in an emerging transnational business community over this period. Carroll and Fennema (2004) disagreed, arguing that tie formation was not properly conceptualized and that the earlier ties in Kentor and Kang (2004) were under-counted, leading to the appearance of a larger difference.

Network analyses have taken on much larger scales—culminating in the Heemskerk and Takes (2016) study comprised of the largest million firms in the world. What is interesting is that there has been some continuity with earlier analyses, despite the larger scale and different method.¹³ Specifically, while there were extremely dense transnational ties found within a North-Atlantic supercluster (see also Carroll and Sapinsky 2010), they found a more autonomously organized East Asian cluster.

Indeed, the integration of East Asian elites with the rest of the global elite is an issue that is likely to generate a lot of interest in the years to come. De Graaf (2020) analyzes the networks resulting from the connections of a small number of Chinese corporate elites: the board members of China's largest firms. She examines if and how China is engaging with other transnational business communities at the heart of the liberal international order. In contrast to the finding of a relatively independent cluster of East Asian corporate elites by Heemkerk and Takes (2016), De Graaf finds evidence for substantial transnational linkages of the Chinese business elite. Ties to the party-state are very strong as well, but de Graaf argues that Chinese elites have two faces: on the one hand, they partially and pragmatically integrate into the existing network, while on the other hand they hold to a distinctive state-directed capitalism orchestrated by the CCP.¹⁴

CONCLUSION

There has been a recent proliferation of IPE research that utilizes network theory and analysis. Developments in both network science and computational capability have coincided with a renewed interest in complex interdependence and other forms of structural analysis in IPE. This convergence has engendered a richer array of empirical techniques than were available in the past, and creative research that employs them. Network analysis provides IPE scholars with a means of capturing phenomena that we are often able to intuitively describe but not systematically measure. In many ways it brings the ontological disposition of “systems-level thinking” into line with empirical methods to study the social world. This is true whatever position scholarship takes with respect to the dynamics of interdependence or of complexity.

As the use of network analysis within IPE expands in the future, two tendencies seem likely. First, that network analysis will be increasingly combined with other methods. The selective use of case studies alongside network analysis has taken place for some time (see Babic et al. 2020), as has regression analysis, yet the use of methods such as focus groups (Carpenter et al. 2014), Qualitative Comparative Analysis (QCA, see Cardenas 2012), and

sequence analysis have recently been employed in IPE scholarship (Buch-Hansen and Henriksen 2019; Henriksen and Seabrooke 2017).

Much IPE scholarship that employs network analysis does so using relational data coded from qualitative information, another way that network analysis can be part of a mixed-method research program. For example, Christopoulos and Quaglia (2009) combine a qualitative case study with formal network analysis measures, in their analysis of EU banking regulatory reform. Paterson et al. (2014) examine the transnational diffusion of carbon trading practices using network analysis supplemented with interview and archival data to identify the ways that policy ideas traveled along the observed network. The nature of ties gathered through data is likely to diversify, bringing in other methods to achieve this. Scott and Christopoulos (2018) examine networks among organizations involved in banking reform initiatives within the UK, whereby informants were interviewed and asked to list collaborators in order to generate the network. Both James et al. (2021) and Pagliari and Young (2020) use new text analysis techniques related to plagiarism detection to generate ties among interest groups, to examine patterns of information exchange in the EU.

Second, the size, and availability, of network-relational data is likely to increase. While only a decade ago a lot of research worked with just a few dozen nodes, the size of network data has expanded rapidly (Heemskerk and Takes 2016; Garcia-Bernardo et al. 2017; Larsen and Ellersgaard 2017, 2018; Henriksen et al. 2018). The “Big Data” trend raises important questions about network data quality, at a time when IPE is concerned with this question more generally (Linsi and Mügge 2019). This is especially true given both the complex nature of modern firm structures and the sources that many scholars use for data (Heemskerk et al. 2017), as well as the fact that IPE scholars are also interested in flows between countries that are often “rewired” through network structures (Arel-Bundock 2017; Fichtner et al. 2017).

Concepts that IPE scholars make use of seem to be increasingly aligned with a network perspective, and this is even the case when network analysis is not formally used (e.g., see Oatley and Petrova 2022; Ba 2018; Arel-Bundock 2017; Tsingou 2014; Carpenter 2011) or when network analysis is only one descriptive component within a broader collection of observations (Carpenter et al. 2014). Indeed, one of the main conceptual and theoretical advantages of network analysis is that it aligns with common intuitions that IPE scholars often express regarding interdependence, diffusion/contagion, and path dependence. It also allows for a deeper exploration of organizational complexity in the politics of the global economy, which is needed to keep the field at the scientific frontier. There is a renewed focus on ecology, entropy, and evolution among political economists (Lake 2021; Oatley 2019; Winecoff 2017a; Pagliari and Young 2016; Tsingou and Seabrooke 2009), and network science provides a valuable opportunity for engaging with these wider sciences.

Lastly, the application of network theory and method in IPE will allow scholars to re-center power in their analyses. It is not uncommon to spend an entire conference listening to IPE presentations without hearing a single mention of power. This strikes us as unfortunate on aesthetic grounds, but it is also worrisome from the perspective of inference: can we *really* be confident in our explanations of political economy phenomena if power plays little part in our studies? Network science provides a useful pathway forward that retains empirical rigor. A great deal of scholarship using network analysis in the social sciences is dedicated to understanding how structural prominence is related to power, via control of information or resources, the occupation of gatekeeping and brokerage roles, the ability to stabilize (or destabilize) wider systems, and other opportunities for influence. The international political

economy is undergoing profound structural changes; IPE should employ theory and methodology capable of understanding these structures and their evolution, and network science is one promising avenue to explore to that end.

NOTES

1. Graph theory was incorporated into international relations, and the nascent discipline that would later become known as IPE, during this era by Harary and Miller (1970) and Hart (1974).
2. IPE does not exist within a vacuum, of course. Similar trends were evident in international relations, as well as the broader fields of international economics and political science. Winecoff 2017a, b traces this evolution across the history of IPE. Notable early incorporations of network analysis into IR include Hafner-Burton et al. (2009), Jung and Lake (2011), and Cranmer and Desmarais (2016). See also Dorff and Ward (2013), which has an application to international trade networks.
3. We mostly ignore, for now, purely theoretical applications of graph theory (the mathematical precursor to network analysis), even though these were some of the earliest contributions to the IPE networks literature. However, we discuss opportunities for more complex formal modeling.
4. Some of these have a long history in the economic sociology literature, e.g. Nemeth and Smith (1985) and Smith and White (1992). Glückler and Panitz (2016) provide a more recent example, which explores social divisions in labor markets.
5. For example, a multiplex network in IPE might include the following as layers: trade flows, FDI flows, FPI flows, and IGO membership. Analyzing these networks collectively may reveal interesting community patterns, or more nuanced core-periphery structures than exists in traditional hegemony or polarity studies. One paper examining the nuclear cooperation network using economic relations as one of the layers in a multiplex network is Goldblum et al. (2019). Another examines bilateral (state-to-state) lending as part of a co-evolving network with the system of defense cooperation agreements (Kinne and Bunte 2020). Chen (2021) introduces a multiplex extension of the ERGM model discussed. As these studies show, multiplex analysis is especially promising for research designs that straddle the typical political economy-security boundary.
6. The Simulation Investigation for Empirical Network Analysis (SIENA) model (also known as the Stochastic Actor-Oriented Model (SAOM)) is another method for analysis of longitudinal networks. These are compared to ERGMs by Block et al. (2016) and Leifeld and Cranmer (2019).
7. Cranmer et al. (2021) provide a primer on these and other network modeling approaches.
8. Due to space constraints, we largely ignore research on global governance outside of trade and finance (e.g., climate change, immigration, ideational communities and spread, or other topics in IPE that have used—or could use—network analysis. For example, the diffusion of (or convergence to) domestic economic policies (such as those related to capital taxation) is explored by Cao (2012), while others diffusion of political institutions and democracy (Goodliffe and Hawkins 2017; Cranmer et al. 2020).
9. The relationship between the trade network and conflict was more extensively studied by Kinne (2012), who argued that centrality in the trade network substantially reduced a country's propensity for conflict. Cranmer et al. (2015) showed that fractionalization in

- the trade network contributed most of the “Kantian tripod” variables to peace in the international system (no dyadic “democratic peace” effect remained once the network structure was accounted for). This topic was also explored by Lupu and Traag (2013).
10. These models are also known as Stochastic Actor-Oriented Models, or SAOM.
 11. Special issues on the implications of the crisis for IPE were published by many journals, including *International Interactions*, *New Political Economy*, and *Review of International Political Economy*. But many of the leading “American” IPE journals published little to nothing about the crisis within five years of it occurring, and neglected money and finance issues more generally both before and after the crisis (Pepinsky and Steinberg 2014).
 12. Although not formally a network analysis, Lee (2020) qualitatively shows how merchant bankers in the early-modern period in Europe gained representation across a wide range of political and economic entities, often through marriage or other family linkages.
 13. Heemskerk and Takes (2016) use community detection algorithms to look at clusters within the network.
 14. Others have examined elite circulation through government posts as a means for regime survival, e.g. Woldense (2018).

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CHAPTER 12

CONTENT ANALYSIS IN INTERNATIONAL POLITICAL ECONOMY

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INTRODUCTION

CONTENT analysis is a “a research technique for the objective, systematic and quantitative description of the manifest content of communication, for the purpose of interpreting it” (Berelson 1952, 18). In the light of Krippendorff’s classical definition (Krippendorff 2009) the communication material to be coded includes oral, visual, or written messages as well as of the indicators (quantitative or not) which enable the inference of knowledge regarding the production and reception conditions to generate reproducible and valid inferences that can be applied to their context.

Content analysis is relevant to International Political Economy (IPE) because it can be deployed to get at systematic patterns relevant to key questions typical of the field: How do ideas get mobilized through the internal mechanisms of international economic institutions to power policy change (Wonka 2008; Ban 2015a)? How do we know that/if an international organization changed its views on a given policy area such as social policy (Vetterlein 2007), development policy (Joshi and O’Dell 2013), food and agriculture policy (Daoud et al. 2019), demography (Seabrooke, Tsingou, and Willers 2020) or fiscal policy (Ban 2015b)? How and why do the policy areas covered in international finance institution (IFI) loan conditions change over time and across borrowers (Cormier and Manger 2021)? How have policy templates such as the Washington Consensus evolved within important disseminators such as the International Monetary Fund (IMF) (Kaya and Reay 2019)? How do policy networks (Capie 2010; Klüver, et al. 2015; Ban and Patenaude 2019), principals (Fratzscher and Reynaud 2011; Heldt and Mahrenbach 2020) or changing staff demographics (Blackmon 2021) shape policy change in international financial institutions? How do global economic ideas get translated into domestic ones (Ban 2016; Pleines 2021) and how do domestic activists understand global economic policy imperatives (Rice 2010)? How do we measure the gap between expert discourse and policy advice/conditionality in international

organizations (IOs) (Broome 2015; Hernandez 2020)? Does international policy advice have any bite on domestic policy debate in all countries, or only in some (Momani 2006)? How do market sentiment (Breen et al. 2021) and IO sentiment (Breen et al. 2020) change over time vis-à-vis domestic policy choices and how do we make sense of the ensuing variation?

From its beginnings, the method's cleavage was between its deductive and its inductive varieties. The former used pre-set variables extracted from the relevant literature to code a large corpus of text. The results lend themselves for replication and the approach is predominantly quantitative. Conversely, inductive content analysis extracts recurring themes from a smaller body of texts, is predominantly qualitative, and is not designed to be replicable (Neuendorf 2017). Irrespective of the variety of content analysis that one embraces, it is generally acknowledged that scholars working in this tradition should adopt a minimum standard consisting of the provision of sufficiently explicit detail about all the methodological decisions taken with the regard to the following two aspects (a) sampling procedure, by explaining the key term choice acknowledging sample limitations and (b) coding protocol transparency in reference to variable definition and justification, resolving coding disagreements between coders, eliminating variables due to intercoder and intracoder reliability (Riffe et al. 2019). The application of these tools and procedures is necessary to lend more empirical depth to the claims being made by IPE scholars calling on content analysis in their work.

HISTORICIZING CONTENT ANALYSIS

For a long time, content analysis remained the province of communication scholars interested in the media but since the 1980s it spread throughout social science and the (quantitative) humanities, with political science joining the phenomenon (for useful overviews see Bass and Semetko 2021). A lesser known fact is that one of the first adopters of (quantitative and manual) content analysis was early Cold War-era International Relations (IR), a process that continued into the 1960s (Janowitz 1968), with Holsti and Laswell as its key proponents (Pashakhanlou 2017). The method's frequency counts were savaged by none other than Morgenthau and Jervis (1967) who pointed out that inferring importance from frequency counts is absurd and that overall quantitative content analysis had unsurpassable interpretation, context, and validity problems. It was in this atmosphere of lacerating criticism that Hedley Bull wrote that the method was 'as remote from the substance of international politics as the inmates of a Victorian nunnery were from the study of sex' (cited in Pashakhanlou 2017, 451). Throughout this period, qualitative content analysis was relegated to utter marginality in US and British universities at least.

In contrast to IR, IPE reached out for this tool much later, in the 2000s, when a second wave of content analysis reached IR (Schonhardt-Bailey 2005), this time bolstered by Andrew Bennett's (2015) plea that computer-assisted quantitative content analysis can overcome the Morgenthau-Jervis critique through a structured integration of qualitative methods such as argumentation analysis, narrative analysis, and especially discourse analysis. It was also around this time that the work of German scholars who had taken qualitative content analysis to new heights in the 1970s reached US and UK universities, with Neuendorf and Krippendorff's *magnus opuses* introducing to mainstream social science the

‘strict’, ‘flexible’, and ‘summative’ varieties of qualitative content analysis on the legitimate methodological menu alongside the more familiar quantitative varieties.

FIRST-WAVE CONTENT ANALYSIS IN IPE

Qualitative content analysis proved attractive to IPE scholars’ prevailing interest in systematic rather than holistic analysis. The reasons for its attractiveness in this regard consisted of its middle-range interpretation, contextualization, and validity capabilities. Around the time when political science seemed split between those going “all in” for automated nonparametric content analysis (Hopkins and King 2010; Owens and Wedeking 2012) and those trying to highlight the value added of both automated and traditional versions of this method (Rooduijn and Pauwels 2011), Broome and Seabrooke (2007) shot the opening salvo¹ in IPE, challenging scholars to study economic ideas in international financial institutions such as the IMF in a more systematic yet reflexively qualitative way. Riding on the rehabilitation of qualitative content analysis in IR, they sought to build qualitative and context-sensitive frequencies of causal statements made across time by the IMF in its policy surveillance reports. Thus, Broome and Seabrooke worked with a pre-set coding spectrum and simple counting of the points on that spectrum to visualize shifts in the policy discourse across several decades (1975–2004). Moments of high frequency were seen as potential “policy revolutions,” whereas persistent but lower frequency advice was seen as “policy recombinations,” prompting the authors to focus on the former in their empirical analysis based on Fund archives and interviews with staff. This foundational article disclosed the procedure for selecting the elements of the coded corpus and were transparent about the variables in the codebook and especially about the way they were scaled. In brief, Broome and Seabrooke set the minimum standard that content analysis scholars today think one should have (Lacy et al. 2015) while charting the path toward doing frequency counts in reflexive ways.

Inspired by this call, in the mid-2010s a slew of studies began to use content analysis in IPE while complementing this method not just with interviews, but other, less conventional methods such as sequence analysis or hierarchical analysis. The preferred choice here ranged between inductive theme mapping and deductive content analysis.

Inductive theme mapping is most apt when the corpus is too small to lend itself to quantitative analysis yet too large for the more intensive discourse analysis. A representative study for theme mapping using a relatively small number of documents generated along a limited time frame is Andrew Baker’s (2018) study of the institutionalization of macroprudential regulation uses inductive qualitative content analysis to trace the themes that constitute the social purpose of this regulatory approach. In this important article on the literature dealing with the aftermath of the Great Financial Crisis, Baker used a combination of qualitative content analysis of streams of conceptual work on macroprudential regulation in BIS and Geneva Group, and interviews with some of these early macroprudential advocates. His strategy was to identify the most recurrent themes used consistently across the work of these two expert groups close to the corridors of international regulatory power. The result was the unearthing of the “deep structures” of the macroprudential accounts of economic and financial systems: procyclicality, complexity, endogeneity, and fallacies of composition. All of

these elements are important because they contain “distinctive macro-moralities regarding systemic stabilisation, systemic design and questions of the good system” (311).

Another example in this vein is the use of content analysis by Seabrooke and Tsingou (2014) on the content of post-crisis financial reform reports, linking their content to Andrew Abbott’s fractal distinctions. Going beyond Baker, they then matched variation in content to the network of authors involved in the debate. As such, this calibration of the method enabled the authors to make the first differentiation of “issue entrepreneurs” (who are consistent in their opinions on post-crisis reform in wanting to change the structure of the system) with “issue professionals” who were present on different reports arguing, contrarily, for reform to the structure of the system (G30 reports) and simultaneously only reform to the units (IFF report). Rather than merely finding patterns in discourse, the Seabrooke-Tsingou approach enabled the mapping of power asymmetries and the actors behind it.

This task of studying the discourse-power nexus through the prism of content analysis was carried further by other scholars (Ban 2015b; Singh 2021). For example, Ban (2015b) used content analysis in conjunction with organizational analysis to establish the extent and politics of doctrinal change on fiscal policy at the IMF since 2008. All relevant passages from IMF studies on fiscal policy (working papers, staff papers) as well as its policy doctrine documents (Global Fiscal Monitors) and research doctrine documents (World Economic Outlook reports) were coded using two simple categories: revisionist and orthodox. The studies that reproduced the pre-2008 fiscal policy line were coded as “orthodox.” Those that deviated from the pre-2008 fiscal policy line were coded as “revisionist.” The paper published the coding sheet and indicated how borderline cases would be adjudicated. This approach enabled the production of visually suggestive descriptive statistics about the positions of IMF staff who authored IMF studies on fiscal policy. Interpreting these positions relies exclusively on coders’ training in macroeconomics. Indeed, subtle nuances and signaling through citations being of essence, makes the option to delegate the coding process to an algorithm impossible.

To link discourse to power and find how important these voices were in the Fund, the article used LinkedIn (a biographical database) as well as the IMF website to code the professional trajectories of all these IMF authors, and matched their position in the organizational hierarchy of the fund onto their doctrinal positions. As some authors (co)authored revisionist papers on some topics of fiscal policy and orthodox ones on other topics, they were coded as “mixed.” Interviews with staff at the IMF headquarters helped refine the findings by providing insight into the perceptions of the staff on select issues. The remaining gaps were covered through research on media sources.

Similarly, J. P. Singh (2021) examined the international trade regime from the perspective of its American anchor, as represented by the office of the US Trade Representative. Singh hand-coded 13 years’ worth of hundreds of the United States Trade Representative (USTR) press releases and inductively coded by theme. The upshot was that the numerical output showed that 93 percent of what he coded as “paternalistic” references (as opposed to favorable, unfavorable, mixed, or neutral) from the USTR concerned the developing world and contained advice to open their markets, an approach not taken vis-à-vis the protectionism of the developed North. The finding was then used to map out the structural barriers to fairer trade relations posed by paternalistic and even racist frames used in North powers to situate the Global South in a symbolically inferior position. Overall, the insights developed from the systematic coding of such policy-critical text enabled the author to highlight cultural

blind spots in IPE of the kind that made the field miss the rise of right populism, racism, and xenophobia.

Similar hand-coded theme mapping exercises rested at the basis of studies of the IMF's change in its fiscal policy doctrine after 2008 (Ban 2015a), IMF policy biases in its country programs (Hernandez 2020), the contestation amongst experts in prominent financial regulation venues such as the Bank of International Settlements or the Financial Stability Board (Ban et al. 2016), new powers' politics in the international trade governance (Stephen and Parizek 2019), the resilience of New Keynesian macroeconomics (Helgadottir and Ban 2021) and the rational expectations assumption that underpins it (Helgadottir 2021), and the intellectual travails of the European Commission on the topic of demographic change (Seabrooke et al. 2020).

In contrast to the community of hand-coders, other IPE scholars relied more on automated content analysis or unsupervised machine learning. In this approach, words that tend to occur together with high probability are mapped out by various software packages such as N-Vivo or in the R system. The researchers using this approach can claim advantages such as higher transparency, higher coding speed, data error removal, easy calibration of coding protocols, and reliable coding of very large bodies of text. Thus, Crespy and Vanheuverzwijn (2019) screened the surveillance reports of the European Union (EU) (the so-called European Semester) to map how causal relationship between "structural reforms," fiscal consolidation, or growth reforms were narrated in order "to understand how and whether the hierarchy of broad objectives changed over time and how structural reforms were located within the economic reasoning" (97). The coding strategy was mixed (inductive and deductive) and it entailed first the software-assisted identification of the coding (paragraphs containing certain key words) and then the hand-coding of specific policy objectives in those paragraphs. Deductively, the authors then coded the corpus of ambiguous paragraphs using pre-set categories developed in the literature for such settings. To explore the (inter)subjective understanding of the meaning of structural reforms in the corpus, the authors then carried out semi-structured interviews with the policymakers involved.

Some of these studies' methodological sections (Ban et al. 2016; Hasselbalch 2019) tried to be innovative in their mixing of content analysis and methods that had not been explored in IPE at the time (such as network analysis), in what can be seen as an answer to Lamont and Swidler's (2014) call on qualitative scholars to mobilize several forms of evidence. For example, Hasselbalch (2019) mapped discourse networks to make distinctive storylines ooze out of the analysis as constellations of frames that change over time, with the relative importance of frames within each storyline deduced from its centrality in the network, their connections to other frame objects, and their frequency of usage by different policy actors.

Most of this scholarship could also be said to be adequate in disclosing and defending the procedure for selecting the corpus or in defining the variables (for deductive content analysis). At the same time, most had little transparency on the coding protocol or how the coders solved their coding disagreements. Furthermore, their carefully contextualized output of content analysis was too often limited to simple descriptions, with the explanatory heavy-lifting done by other methods. Thus, to better understand why the some patterns of ideas identified by content analysis were more important for policy decisions than others, these authors used a set of methodological tools (network analysis, regression analysis, and sequence analysis) to match the patterns on policy doctrine to patterns in the professional and institutional positions of the actors who proposed those ideas in the first place. While

network analysis visualizes the structure of the policy networks involved in the governance of the crises analyzed in the project, sequence analysis visualizes the specific professional trajectory of the individual policy stakeholders involved in various episodes of crisis management. As such, content analysis played a subordinate (descriptive) role relative to the analytical firepower of these methods. These advances had to wait for the second wave of content analysis scholarship in IPE.

SECOND-WAVE CONTENT ANALYSIS IN IPE

At the cusp of the 2020s, a new generation of IPE studies emerged that was built on greater awareness that content analysis deserves detailed methodological sections, linkages with other methods (formal and otherwise), and skepticism toward the robotization of content analysis via unsupervised machine learning. The emerging wave passed harsh judgment on this trend by pointing out that algorithms “often split meaningful topics inadvertently, or worse, combine distinct themes because identical words appear in multiple topics even though the terms have different meanings depending on the context” (Cormier and Manger 2021). Obviously, this level of meaning-making is not legible to robotic algorithms, at least not yet and claiming that it is otherwise risks post-hoc topic adjustment and interpretive arbitrariness. Moreover, other scholars raised the impossibility of the automatic detection of irrelevant data (Bass and Semetko 2021, 58). It turned out that, left to its own devices, the algorithm was doing violence to the socio-linguistic reality fed to it by the researcher. Three main reactions emerged in response to this crisis.

The first reaction was to give up on automated coding altogether while bolstering traditional content analysis with other automated methods in order to lend it the capacity to co-produce analytical, not just descriptive knowledge. Thus, Ban and Patenaude (2019) proposed a sociologically-inspired comparative professional field analysis to uncover the precise role of different qualifications, experiences, and hierarchies in shaping the economic expertise invoked by the European Central Bank (ECB)’s and the IMF’s main policy documents, with a specific focus on debates over fiscal consolidation in the wake of the global financial crisis of 2008. To do this, the article deployed a novel combination of content, network, and regression analysis. Thus augmented, traditional content analysis enabled not just a visualization of where sympathetic interlocutors come from for specific economic ideas deployed by the ECB and the IMF but also what ideas those interlocutors and the institutions’ own staff would most likely have, given certain sequences in their careers.

The second and more prolific reaction was to deepen automation while relegating human coding to specific tasks of the bigger techno scheme (Kentikelenis and Voeten 2021; Cormier and Manger 2021; Breen et al. 2020, 2020; Moschella et al. 2020). Thus, one study of World Bank conditionalities (Cormier and Manger 2021) brought the human coder back into the quantitative revival of content analysis via keyword-assisted topic models of the latest generation (keyATM). These models entail that the researchers choose pre-set keywords to distinguish topics results in much more interpretable topics that unsupervised models do. At the same time, the model does not simply generate fast frequency distributions. It also yields a mixture of probability distributions, one with positive probabilities for keywords and another with positive probabilities for all words. For example, the probabilities with which

individual words are associated with a specific topic and from this distribution the authors “can infer substantive meaning and verify that our chosen topics appropriately operationalize the concepts of interest [and] the dependent variable is not merely based on the relative frequency of keywords, but on the probability of co-occurrence with other words that represent meaningful and coherent topics.” Based on this analysis it was found that shifts in the Bank’s economic research and policy priorities influence Bank loan conditions, even after controlling for country characteristics and international political aspects.

In turn, Breen et al. (2020) use a combination of regression analysis and sentiment analysis on 400 surveillance documents of the IMF and the EU over the period 1997–2014 to get at the mechanisms that produce incoherence in international regime complexes. Unlike in the case of manual coding, where the corpus is represented only by policy prescription paragraphs that are deemed substantively relevant, sentiment analysis tends to look at these documents as a whole. By treating differences in the tone of the language used as a proxy for measuring policy coherence, this approach reveals the extent of incoherence within the surveillance regime complex. On average, rather than evolve in sync, the IMF appears more pessimistic on average than the EU but the EU seemed to be less pessimistic in its surveillance efforts before the global financial crisis hit and more so thereafter. The quantitative model then establishes that this variation is explained not by organizational politics but by technocratic rules, with each institution being more pessimistic when the rules framing their fiscal surveillance are in force and reduce these institutions’ room for discretion. This means that when the rules are different, the sentiment is different.

A similar “twin engine” of human and automatized content analysis powers the sentiment analysis approaches (Kentikelenis and Voeten 2021; Breen et al. 2020; Baranowski et al. 2021). First applied by economists looking at the effects of central bank communication (Picault and Renault 2017; Algaba et al. 2020; Bennani et al. 2020), this is a mighty tool, indeed. One can use specific software packages (i.e., R package *Rsentiment*), as Kentikelenis and Voeten do, to work with “valence shifters,” such as negators (e.g., “*not* approve”), amplifiers (e.g., “*strongly* approve”), de-amplifiers (e.g., “*hardly* approve”), and adversative conjunctions (e.g., “approve *but*”) deployed to code individual sentences. What sentiment analysis does is superbly modular: precisely locating affective language of consequence for institutional legitimacy such as spectra of criticism and spectra of agreement across time and issues. Once the terrain is mapped, the qualitative analysis can dive down over the pressure points of criticism or hover above the patterns of consent in effective and systematic ways, as opposed to the rather haphazard or extremely time-intensive ways in which one would be trapped in the absence of these tools.

Other advocates of this methodological turn took a different tack. They deployed sentiment analysis to focus not on written policy material but on speeches, with an emphasis on central banks’ legitimation strategy. For example, Moschella et al. (2020) use automated content analysis to study blame-shifting strategies used by central banks to deflect responsibility and channel negative public sentiments at other actors, thus challenging the view according to which the ECB conceives of its sources of legitimation based almost exclusively on the achievement of its mandate. Instead, the analysis unveils increased levels of politicization chosen by the ECB to redefine the sources of its legitimation strategy from output legitimacy to input legitimacy. After taking a first cut with topic mapping and ranking over nearly 20 years, this approach then measures issue diversity and core salience of each key term using a bespoke model.

Finally, Ferrara (2020) used a novel combination of automated text classification and unsupervised (Wordfish) scaling methods to detect ideational variation in ECB Executive Board members' public speeches. In conjunction with a qualitative analysis of the events, this methodological mix enabled the author to trace not just how the ECB has progressively moved away from a fiscal profligacy narrative to a systemic narrative of the euro crisis, but to also trace the policy learning mechanisms that powered the gradual shift that accompanied the ECB's commitment to unlimited bond purchases in summer 2012.

This techno-optimistic approach moderated by a revaluation of human coding is well supported in the broader methodological literature outside of IPE. Thus, some political scientists (Grimmer and Stewart 2013) take a less dim view of the problems of computational content analysis and push for better automation, with the traditional content analysis approach in a supporting role. Going further, the fact that some of these calibrated models approximate the performance of human coders (Eshima et al. 2020) suggests that this hybrid is promising. The separation of labor between human and algorithm seems to work smoothly: the manual coding based on close reading is used on partitioned documents to generate the coding categories that can be used to train the coding algorithm which can then be left to map out unknown/unanticipated coding categories. To be convincing, the computational approach should be based on a kind of formalized theory (the phrase "formal model" is used), like in quantitative economics. And like economists, they do not dismiss the complexity of meanings in a text. Instead, they merely argue that a formalized approach helps to simplify and quantify the content of language in analytically useful ways. The idea behind it all is essentially a form of cautious techno-optimism: over time, computer-based coding will replicate human coders but only if the role of human coders is taken seriously when creating coding schemes and calibrating coding models.

The third option combines traditional and computational content analysis into "fully integrated content analysis" that gives human coding the primary role (Pashakhanlou 2017). In line with this approach, other political scientists (Lewis et al. 2013; Zamith and Lewis 2015) suggested that one should resort to automation for some questions (the "what" questions) and tasks (preparing data, identifying "hot points" in the corpus, identifying major themes to be incorporated into manual coding sheets, speeding up coding based on manual coding sheets) while leaving the "how" and "why" questions, the coding of complex concepts, and relationships or nuances of various kinds (geographic, temporal, sociological) to traditional content analysis methods. Comparative political economists show the way in this regard. By the late 2010s, the second wave of content analysis in political science began to take a radical turn with the integration of computational linguistic toolkits by qualitative political scientists working with data science centers in their universities. Of particular note in this regard is Cathie Jo Martin's (2018) use of computational linguistics to not only descriptively show that certain cultural scripts were more plentiful in the Danish corpus of national literature than in the UK one, but also to trace the causal sequencing of shifts in cultural narratives and policy reforms.

There is evidence of this already happening in in IPE. Thus, Thiemann et al. (2020) sought to uncover how unsuspected alliances were forged between central bank and academic economists to legitimize the transnational macroprudential agenda in the aftermath of the 2008 crisis. To do this, the authors use author-topic modeling (ATM), a method that facilitates a clear association of different authors to different topics that emerge from the texts. By using ATM, one can more accurately trace the professional affiliations of the authors and, thus, the policy orientations interest. But to make the use of ATM possible in the first

place, the authors first carried out document analysis and insider interviews at the Fed, ECB, and academia in order to be able to select the most relevant forms of systemic risk. Indeed, in the overall architecture of the paper, the ATM plays a subordinate (albeit highly technical and transparently unpacked) role to careful qualitative analysis of the traditional kind.

Kaya and Reay (2019) took this challenge further when they used computer-assisted content analysis to code over 200 million words of IMF documents to track occurrences of a series of words and phrases representing the Washington Consensus on a year-by-year basis. Yet the critical step of choosing the phrases only occurred after traditional close readings sensitive to sense-making and context. To see if changes in the IMF document types consistently preceded changes in the others, the paper uses vector autoregression (VAR) techniques for the first-time, which automatically seek any correlations between present and past (i.e., lagged) values. This technique enabled the authors to uncover not just the process whereby the Consensus moved through the IMF's internal structure over time and the different aspects of the institution's operations, but also the kind of (fragmented) change experienced by the Consensus as a policy paradigm.

Called "text mining," content analysis (including its sentiment analysis spin-off) migrated from academia to the internal research bodies of the very international organizations studied by IPE scholars, such as the IMF or central banks. As some IPE studies have showed (Ban and Patenaude 2019; Thiemann et al. 2020), economists based in these institutions often engage in bolder research enterprises than academic economists and, as such, their work is important. It also overlaps to some extent with that of IPE scholarship, with the foundational work developed in this tradition (Bholat et al. 2015) authored with IPE scholars such as Cheryl Schonhardt-Bailey. Overall, it can be said that content analysis undertaken in these policy environments is perhaps the most quantitatively exploratory version and IPE scholars should take note of it.

For example, the IMF carries its own text mining approaches, including a sentiment analysis off-shoot. One such study deploys this approach to reveal significant variation in priorities across time and constituencies by the weight which they place on three key priorities: (i) growth; (ii) debt and development; and (iii) crisis management and quota reform. Its sentiment analysis techniques also show that addressing climate change is a topic which is viewed positively by an increasing number of constituencies (Medina et al. 2021). Using a similar approach, other IMF studies uncovered how member states receive IMF policy advice (Fayad et al. 2020). As part of their mandate, central bank teams at institutions as different as the Bank of Korea (Lee et al. 2019) or Finland (Paloviita et al. 2020) used sentiment analysis to study the effects of central bank communication on financial markets.

CONCLUSIONS

Content analysis has proven to be a reliable methodological tool for IPE scholars. It made possible a more systematic set of answers to questions ranging from the ways in which international financial institutions claim legitimacy for themselves to the actual translation of international economic ideas or policy templates into domestic ones. Technically speaking, the method has come a long way, from simple word frequencies and three-point coding schemes to sophisticated and transparent cocktails of automated machine learning, network

analysis, scaling methods, and regressions. These are important advances that bolster the scientific basis of the field and, as such, they should be applauded.

But important as these advances are, it's hard to resist the urge to rage against the technological fetishism that the discovery of automated machine learning by IPE scholars can bring. Indeed, it is key to keep the method flexible, pluralistic, and in touch with the qualitative evidence that has anchored the fundamentals of IPE. In other words, and at the risk of sounding trite, content analysis should remain a methodological tool to address important questions. If the pitfalls of economics are a guide, it (and other methods that can be modeled with great finesse) should not morph into the main object of scholarly sophistication. As this review shows, some questions do lend themselves quite well to a reflexive mix of human coding and automatic coding. Other questions, however, do not. Whenever the object of coding is represented by economic controversies where battles are won and lost based on subtle hints, strategic citation patterns, and ideas hidden beyond econometric models whose extraction entails significant economic expertise to uncover, automated text analysis would be a crude if not destructive tool indeed.

NOTE

1. Before 2007 there was some research in IPE claiming to use content analysis. For example, Bessma Momani (2006) asked whether the Fund staff "teaches" and G7 countries like Canada "learn" from the annual surveillance exercises. Based on a combination of text analysis and interviews she found that teaching occurred only as an academic, not as a policy exercise. However, Momani's analysis does not reveal a coding scheme or any other procedure used for content analysis. Neither is there a descriptive statistic of the findings.

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CHAPTER 13

PROCESS TRACING AND INTERNATIONAL POLITICAL ECONOMY

JEFFREY T. CHECKEL

INTRODUCTION

OVER the past 15 years, process tracing has advanced significantly as a method (George and Bennett 2005; Bennett and Checkel 2015; Beach and Pedersen 2019). We have gone from a situation where many liked process tracing—“process tracing is good!”—to one where we have clear standards and best practices for the method. Researchers can now examine a particular application of the method and conclude “this is an instance of good process tracing” (Waldner 2011, 7).

Qualitative International Political Economy (IPE) scholars have not kept up with this methodological evolution. Work in this tradition typically presents its findings via narrative case studies, often to very good effect. Kirshner’s (2007) study of financial sectors as a buffer against war and Ban’s (2016) examination of how global neoliberal economic ideas diffuse and “go local” are exemplars of this IPE genre. Yet, as process tracing qua method has advanced and our more general expectations and standards for qualitative methods have risen, one could argue a methods gap/lacunae/neglect has arisen in qualitative IPE.

Reflecting on this gap, I advance two arguments. First, the majority of qualitative IPE scholars utilize a narrative case study style that hides their methods, process tracing as well. The result is an empirics–method disconnect, where readers are unsure how data for the narrative was gathered and causal inferences or interpretive understandings gleaned from it. Second, these scholars can and should do their process tracing better. However, in making this methodological move they should resist the temptation simply to pull process tracing “off the shelf.” Rather, they should use their interest in the method to address three cutting-edge issues for process tracers.

The chapter proceeds as follows. I begin with a survey of the contemporary (2010–2020) IPE literature, assessing its use of qualitative methods and, especially of process tracing. My main finding is this work has been good at taking on board the case study method, but

less attentive to the natural follow-on: the within case methods needed to carry out the case study. This is surprising as the methods literature on such within-case methods (process tracing, interviews, document analysis, discourse, ethnography) is by now quite substantial.

If my first section is backward looking, the next looks to the future. As qualitative IPE scholars begin to utilize methods such as process tracing, I argue they should not be passive consumers of the technique. Instead, they should use the turn to process tracing to advance it in three ways. For one, they should push process tracing to be more transparent, but not by applying Bayesian logic. In addition, these scholars are well-placed to help process tracers address a neglected issue: the additional methods process tracing requires to gather data on its causal mechanisms. I will call this the challenge of developing within-process-tracing methods. Finally, those qualitative IPE scholars who work interpretatively should help develop the currently missing interpretative variant of process tracing.

I offer one caveat before beginning. Qualitative IPE is not alone in its neglect of process tracing or the operational use of qualitative methods more generally. This methods–empirics disconnect is also evident in constructivist international relations (IR) theory (Checkel 2018), while historical institutionalists and students of comparative historical analysis confront serious methodological challenges in measuring one of their central concepts: critical junctures (Symposium 2017). Thus, while my arguments and critiques here address qualitative IPE, they likely generalize to other political science subfields where qualitative methods play an important role.

PROCESS TRACING AND IPE—AN OVERVIEW

How do qualitative IPE researchers use their methods? To answer this question, I focus on the period 2010–2020, and survey the following literature and sources: the journals *Review of International Political Economy* (RIPE), *New Political Economy* (NPE), *International Studies Quarterly* (ISQ), and *International Organization* (IO);¹ qualitative IPE books published by Cambridge University Press and Routledge;² and publications by leading qualitative IPE scholars (Mark Blyth, Kathleen McNamara, Craig Parsons, Leonard Seabrooke). In addition, we asked the last group of scholars about other IPE researchers using qualitative methods/process tracing; their recommendations produced six additional names: Rawi Abdelal, Cornel Ban, Jacqueline Best, Nicolas Jabko, Jonathan Kirshner, and James Ashley Morrison.³

I present the findings in three steps, beginning with some over-arching results. Next, I work through several examples, designed to capture the modal way in which qualitative IPE uses process tracing. Finally, I turn to a small body of IPE literature where the process tracing is well executed. This work represents the proverbial “exception that proves the rule.”

General Findings—Where Is the Process Tracing?

My review of the literature indicates the methodological debate on process tracing has only partially reached qualitative IPE and there is room for improvement in how these researchers use the method; this is seen in six ways.⁴ First, it is worrying that many IPE scholars claim to

use process tracing, but do not cite any methodological article or book. At a minimum, such a connection is needed so readers can better understand the type of process tracing being used: deductive, inductive, Bayesian, or interpretive.

In a similar fashion, scholars frequently refer to process tracing as a within-case method in their qualitative study, but it seems to be more a label rather than a method that comes with certain requirements, considerations, and best practices. This is process tracing more as metaphor than as methodological tool. It is puzzling that one stills sees this usage, as by 2015, the methods literature had elaborated the techniques and best practices for using the method in a rigorous manner (Beach and Pedersen 2013; Bennett and Checkel 2015).

Data from the two leading qualitative IPE/political economy journals support these points. For *RIPE*, 12 out of the 26 articles explicitly employing process tracing between 2010 and 2020 do not cite any methodological text. That is, in nearly half the cases (46 percent), the reader is told a key method will be process tracing, but given zero information on how the method will be used to make causal inferences or reconstruct interpretive narratives. Four articles do have one citation to the process tracing literature, but it is to George and Bennett (2005). While this is certainly a foundational text—it put process tracing on the map for political scientists—it lacks the operational focus of later work (how do I use it? what can go wrong?).⁵

For *New Political Economy*, the numbers are similar. Of the 12 articles between 2010 and 2020 that explicitly use process tracing, fully half (6 out of 12) do not cite any methodological text.

Second and consistent with a lack of engagement with the methodological literature, process tracing, even when it is the explicit method of choice for IPE scholars, is poorly defined (Thiemann, Birk, and Friedrich 2018) or weakly operationalized (Helgadóttir 2016; Röper 2020). Several inductive studies do not specify in their method section that they will employ inductive process tracing (Quaglia 2012; Steinlin and Trampusch 2012; Piroška and Podvršič 2020). Others seem to mistake process tracing for content/discourse analysis (Wood and Ausserladscheider 2020). Indeed, researchers often use “process tracing” to mean nothing more than her/his case study has a temporal element.

Third, the meta-theoretical and theoretical understanding of causal mechanisms is often underdeveloped in qualitative IPE. Without hypothesized mechanisms, the use of process tracing makes little sense, as its whole purpose is to measure their observable implications. One sees too many instances where the language of causal mechanisms is used, but the case study is in fact tracking the effect of independent variables in a historical narrative (Ciplet 2017; Kluge 2017).

Fourth, qualitative IPE case studies tend to be heavily empirical. That is, they relate a richly documented historical narrative that is neither theory-guided nor refers explicitly to the hypothesized factors, variables, or mechanisms. It is not uncommon for these scholars to theorize a causal connection, then narrate a case study where they carefully explain how certain negotiations or policy bargains unfolded and then—only in the last paragraph (article) or concluding chapter (book)—transition back from the empirics to the theoretical claims. If there is no theory in the case study, then there is no need for methods like process tracing (Eagleton-Pierce 2012; Trumball 2014; Avigor-Eshel and Mandelkern 2020).

Fifth, and a more general point on how qualitative IPE is using methods, discussions on transparency or ethics are largely missing. This is odd given the attention these issues have

gotten in the general political science literature since 2014–2015 (Moravcsik 2014; Cronin-Furman and Lake 2018). Specifically on transparency, Van Evera's four tests for sharpening the inferences drawn from process-tracing evidence (Van Evera 1997, 30–4) are virtually never utilized; the same holds true for using Bayesian analysis (Fairfield and Charman 2017) to improve the method's transparency.⁶

Sixth, in my sample, there is a special issue in *New Political Economy* devoted to process tracing (Palier and Trampusch 2016). Yet, its content confirms that the method is not a priority for qualitative IPE. For one, most of the contributors are not IPE scholars, but researchers who have participated in the more general debates over mechanisms and process tracing (Derek Beach, Tulia Falleti, James Mahoney, Renate Mayntz). More important, the collection concludes with an essay by a prominent political economist expressing significant hesitation and worry over any use of process tracing (Hay 2016). Overall, the special issue makes for an interesting methodological read, but it does not connect process tracing to IPE.

Qualitative IPE—Process Tracing (Not) in Action

In this section, I turn from trends to specific examples, working through two cases that capture the modal ways in which qualitative IPE uses process tracing. The first is an instance where process tracing is invoked as a main method, but then vanishes or is implicit in the empirical sections. The second captures the use of a narrative case study where the theoretical set-up—mechanisms, process—is primed for the use of process tracing, but it is not used. The overwhelming majority of the literature I surveyed falls into one or the other of these modal patterns.

In selecting two specific scholars, my intent is not to “name and shame.” Each has produced a very good piece of research, and their use/non-use of process tracing is very much in keeping with qualitative IPE community standards. This chapter argues those standards—in regards to the operationalization of methods—need to be raised, but the chosen scholars are doing no worse than the vast majority of their colleagues.

Published in 2012, Francisco González's *Creative Destruction? Economic Crises and Democracy in Latin America* is an exemplar of my first modal type of qualitative IPE research. The book explores the relation between economic crises in the 1930s and 1980s and democratic development in Latin America. González situates his study within comparative historical analysis (Mahoney and Thelen 2015) and—like many in this tradition—structures his narrative around a number of case study chapters.

Within the case studies, he uses process tracing for measuring the theorized causal mechanisms. To be more precise, this is the claim in the book's opening pages (González 2012, 5–7), where he defines process tracing as identifying the causal chain and causal mechanism between the independent variable(s) and the outcome of the dependent variable (González 2012, 5—drawing upon George and Bennett 2005, 206). This is an excellent start, but the book has little more to say about the method. Ideally, the author would have used these opening pages also to operationalize the process tracing. That is, given the three mechanisms González theorizes, their observable implications in the data would be X, Y, and Z. Unfortunately, this does not happen.

As a result, in the case studies that constitute Parts I and II of the book, the process tracing is implicit. The mechanisms are there in the cases, but it is not clear to the reader how process tracing is being used to measure their effects. Nor is there any effort to deal with the problem that bedevils mechanism-based theorizing: equifinality. This means that alternative causal mechanisms can very well lead to the same outcome. Testing for equifinality would have required even more explicit process tracing in the chapters. In sum, González's nicely argued book highlights a key feature of many qualitative IPE studies: Process tracing is invoked as a main method, but then vanishes/ is implicit in the empirical sections.

Cornel Ban's 2016 book, *Ruling Ideas: How Global Neoliberalism Goes Local*, fits my second modal pattern. This is a richly documented account of the how economic ideas with global reach are translated into different national settings, where domestic translators make these ideas "go local," in Ban's apt phrase. Two of the case study chapters (6, 7) explicitly theorize and document the "mechanisms" (socialization, diffusion) through which this spread and translation occurred. Overall, the book offers a carefully argued and detailed study of the various ways in which economic ideas diffused and were then translated locally.

Methodologically, the set-up is there for Ban to employ within-case methods like process tracing. At the outset, he situates his study within the comparative historical analysis tradition (Ban 2016, 30–1), one of whose defining features is case studies theorized in terms of causal mechanisms that are measured with process tracing (Mahoney and Thelen 2015, 12–20). Indeed, the latter method would seem a perfect fit for Ban's Chapters 6 and 7, where he is mapping the mechanisms through which ideas diffused to and were translated in Spain and Romania. Yet, there are no within-case methods in these chapters, and certainly no process tracing. This makes it difficult for the reader to evaluate how Ban is gathering evidence for the argument (Ban 2016, Chapters 6, 7).

Qualitative IPE—The Exceptions

In this last section, I consider a small body of IPE literature—a total of six publications in my sample—where the process tracing is well executed; this demonstrates that methodological operationalization and transparency are possible for qualitative IPE. These scholars clearly define the method, carefully operationalize their mechanisms, make their tests explicit, and are transparent about evidence. In all six exemplars, the process tracing is of the deductive, theory testing type. I work through one study here, and refer the reader to the others.

Lisa Kastner's (2018) study of the relation between civil society activism and financial regulation in the wake of the 2008 crisis is a smartly designed and rigorously executed example of theory-testing process tracing. Specifically, she explains "how diffuse interests were translated into post-crisis financial regulatory policy by systematically applying process-tracing to test the presence or absence of a hypothesized causal mechanism" (Kastner 2018, 34).

Kastner does this by referencing the process tracing literature (Beach and Pedersen 2013; Bennett and Checkel 2015) and—more importantly—by operationalizing the method, asking what are the observable implications of her theorized mechanisms. After each case study, she summarizes the evidence, showing the causal chain and whether each step is supported by the empirics. This is an exemplary and transparent use of process tracing.⁷

PROCESS TRACING AND IPE—THE FUTURE

My argument in the previous section is that the qualitative IPE/ process tracing relation is best characterized as one of benign neglect. These scholars are aware of the method, but too often fail to use it or apply it in an implicit, metaphorical, and non-operationalized way. This a good news/ bad news situation. The bad is this neglect is undermining the rigour, quality, and transparency of their case studies. However, the good news and silver lining is that qualitative IPE—as a newcomer/ latecomer to the method—can utilize its outsider perspective to rethink and address, in new ways, three cutting-edge issues for process tracing: transparency and formalization; within-process-tracing methods; and developing a robust interpretive variant. This rethink will benefit qualitative IPE and all the many others—international relations theorists (Evangelista 2015), comparativists (Wood 2003; Waldner 2015), students of comparative historical analysis (Gibson 2019)—who also regularly use the method.

To begin, how did I establish the “cutting edge” for process tracing? In short, I consider how we teach the method and what we publish about it. Regarding pedagogy, courses at key fora such as the European Consortium for Political Research (ECPR) methods schools, the Syracuse Institute for Qualitative and Multi-Method Research (IQMR), and APSA short courses consist of process tracing basics plus sessions on formalization; the latter includes applications of Bayesian logic, set theory (Barrenechea and Mahoney 2019), or causal graphs (Waldner 2015).⁸ On research and publications, 55 percent of the journal articles, newsletter contributions, and book articles on process tracing in the period January 2017–August 2020 have been devoted in whole or in part to formalization.⁹

The cutting edge is thus defined by what the literature has prioritized—formalization—and what it has neglected. Formalization is one of the last steps in the use of process tracing, helping us with data analysis. By focusing so intently on it, scholars have neglected what comes before: how one does the data collection (within-process-tracing methods) and, more fundamentally, meta-theory (the missing interpretive variant).

Transparency and Formalization

The debate over transparency among process tracers is linked to the broader debate in the discipline over data access and research transparency, or DA-RT for short.¹⁰ Despite a professed epistemological pluralism, these discussions have mostly been among positivists. Indeed, of the 13 reports released by the “Qualitative Transparency Deliberations” in January 2019, only two addressed transparency from an interpretive perspective.¹¹

Consistent with a core positivist tenet that seeing is believing (Johnson 2006), much of the DA-RT/transparency debate and recommendations have come down to some version of “show me the goods.” This might mean archiving one’s data in qualitative data repositories (Mannheimer et al. 2019); or using active citation (Moravcsik 2010); or attaching numbers to our causal hunches in process tracing to make the logic behind them more visible (Bennett 2015). These are all sensible proposals, but note that they are premised on a separation between the researcher and what she studies—something again consistent with the positivist worldview.

So, how might we operationalize transparency principles for a qualitative IPE researcher using process tracing, with—say—interviews as a key method for data collection? The answer for some students of process tracing is to employ a combination of active citation and Bayesian logic. However, such a choice would be incomplete as there is a prior, interpretive operationalization of transparency that one needs to consider.

Active citation works in the following manner. A footnote citing an interview is linked to a transparency index that contains interview transcripts or excerpts. A reader—by perusing the index—can better understand the evidence behind a causal claim the author advances in her text (Moravcsik 2014).

If we stop at this point, however, we are missing important information that allows us to understand the interview excerpt and what it is telling us. As ethnographers would argue—and interviews are a key method for them as well (Gusterson 1996; Gusterson 2008; Holmes 2009)—one needs to ask how that interview data has been shaped and changed by the researcher. How has her gender, skin color, identity, and the power relation inherent in the interview affected how the interviewee answered? In ethnographic/interpretive jargon, this is to reflect upon one's positionality—and how it has influenced both participants in the interview (Borneman and Hammoudi 2009; see also Borneman 2014).

Returning to active citation's transparency index, then, more than the raw data would be required. We also need to record—in a positionality index?—how a researcher thinks the interview answers/dialogue were a function of her gender, the way she asked a question, and the like. Transparency is now defined not by what we see, but by clarification of context and researcher–interviewee interaction. Reflecting on positionality also pushes a researcher to consider the ethical dimension of her work. For a transparency debate that has too often failed to take ethics seriously (Parkinson and Wood 2015; Monroe 2018), this can only be welcomed.

My argument is not to do away with active citation and transparency indices; rather, we need an additional—more foundational—layer of transparency. For the latter, Lee Ann Fujii's work on relational interviewing provides excellent advice—from a fellow political scientist no less—on how to establish one's positionality in an interview context (Fujii 2017; see also Fujii 2010). If Moravcsik's transparency index allows us to see the data, a positionality index helps one better understand the social process through which that data was constructed.

Her interview data now in hand, transparently collected, the IPE process tracer now needs to figure out what that data means—the data analysis. What causal inferences can she glean from it? This is where Bayesianism enters the picture, with the argument being that it improves and make more visible the logic behind the causal inferences we make in process tracing (Bennett 2015; Fairfield and Charman 2017; Fairfield and Charman 2019). A number of leading scholars support it and Bayesian process tracing is being taught widely, as shown above.

Does the use of Bayesianism result in better, more rigorous and transparent applications of process tracing? Proponents cautiously answer in the affirmative (Fairfield and Charman 2017), while critics argue that the application of Bayesian logic is undercut by a number of logical and practical challenges (Zaks 2021). My own view accords more with the critics. Bayesian process tracing offers little improvement over the way the method has been carried out previously—for three reasons.

First, Bayesian logic cannot work with inductive forms of process tracing, as one has no (deductively derived) theoretical priors to which values can be assigned. This was

a limitation recognized early in the debate (Bennett 2015, 276), but which has since been forgotten. Second, applying Bayesian logic and its accompanying mathematical formulas requires the assignment of estimated probabilities on the prior likelihood a theory is true as well as the likelihood of finding evidence (in two different ways). Bayesian analysis is impossible without these three estimated probabilities, which are derived in a subjective manner lacking any transparency.

Bayesian process tracers are aware of this problem (Bennett 2015, 280–1), but it is not clear how one fixes it. Maybe we need a transparency index (another one!), where a researcher explains what data she drew upon to fix a certain probability, assuring us that cognitive bias played no role, and that she did not cherry pick the data to get a probability that will make her favored theory work. I am being facetious here, but the lack of attention to how estimated probabilities are assigned simply pushes to a deeper level the transparency challenges that process tracing faces.

Third, much of the application of Bayesianism to date has been to process tracing greatest hits, especially Wood (2003) and Tannenwald (2007). Yet, none of the Bayesians who replicate Wood or Tannenwald demonstrates where the Bayesian approach improves the process tracing. As Zaks argues, “Wood and Tannenwald are excellent data collectors, analyzers, and writers—skills that consistently prove to be the most central assets to good (and transparent) process tracing. Until Bayesian proponents can demonstrate where their method reveals new conclusions or more nuanced inferences, the costs of adoption will continue to outweigh the benefits” (Zaks 2021, 71).

In the end, Peter Hall—in an early intervention in the DA-RT, transparency debates—got it right. The best theoretical-empirical qualitative research already does what proponents of enhanced transparency—including Bayesians—seek. For Hall, it is a commitment to “research integrity”—policed by peer-review processes and scientific research programs—that ensures a particular author takes methodological transparency seriously (Hall 2016). Hall’s argument is only strengthened when one considers the opportunity costs as well of learning the logic and mathematics of Bayesianism. For a qualitative IPE seeking to use process tracing in a transparent manner, Bayesian logic is an analytic bridge too far.

Within-Process-Tracing Methods

Going forward, process tracers must devote more pedagogy and research to the techniques needed to do the method’s “front end”—the data collection—well. When teaching process tracing, I am struck that most students think it starts when we measure those observable implications of a causal mechanism. But the data for measuring those mechanisms comes from somewhere—typically, interviews, fieldwork and ethnography/ political ethnography, archives, surveys, and discourse analysis.

Thanks to the revolution in qualitative methods since the early years of the new millennium, we have a wealth of practical, “how to” literature devoted to these various within-process-tracing techniques. These include Mosley (2013) and Fujii (2017) on interviewing; Kapiszewski, MacLean, and Read (2015) and Schatz (2009) on fieldwork and political ethnography; Trachtenberg (2006) on archival research; Fowler (2013) and Bryman and Bell (2019, Chs 5–7) on surveys; and Hansen (2006) and Hopf and Allan (2016) on discourse analysis.

Teaching these methods must become a part of our process tracing pedagogy. Instead of devoting half the short course on process tracing at the APSA convention to Bayesian analysis,¹² we should instead be giving more attention to these within-process-tracing, data collection methods, which easily constitute the majority of one's time and effort in a process tracing study. Bayesian analysis requires high-quality data, gathered with diverse methods; without such data, the application of Bayesianism is simply not possible.

Many scholars cite Elisabeth Wood's (2003) book on the Salvadoran civil war as a process tracing exemplar (Lyal 2015, 189–91). It is a model because of the richness and quality of her data, gleaned from interviews, political ethnography, and her ethnographic map-making workshops. Her process tracing works because she devotes an entire article and a part of her conclusions to operationalizing her within-process-tracing methods, discussing how she will use them to draw inferences on insurgent preferences, threats to the validity of those inferences, and the like (Wood 2003, Ch. 2, 243–6). The data she has gathered is of a very high quality; it sets the stage and provides the raw material for her process tracing. Wood's use of the method is exemplary and transparent because of all this “front-end” work.

For process tracing as method, we thus need to make time in our teaching and space in our research for these data-gathering methods. Process tracers need to get right the balance between front-end methods training and back-end data analysis. Zaks (2021, 72) nicely captures these trade-offs and balancing act.

In the context of qualitative research, scholars have a lot more access to training in the analysis of data than they do in the research processes that get them the data in the first place. But the process of research and the processes we are researching are inextricable. Researchers would likely yield greater benefits from intensive training in ethnographic, interview, and sampling techniques; understanding the politics and biases associated with archival work; or even just additional and specialized language training needed to conduct research on a specific topic.

For a qualitative IPE turning to process tracing, this means less training on analysis (set theory, Bayesianism) and more on within-process-tracing methods.

A greater focus on the methods utilized within process tracing would have the additional—and much needed—benefit of bringing research ethics to the fore. This is a topic on which process tracers have been largely silent.¹³ In process tracing's less scientific days, I would tell students that it gets you down in the trenches and really close to what you are studying. This is true, and the “what” is often policymakers, activists, civil war insurgents, and the like—human subjects in ethics talk. Teaching those additional methods as a part of process tracing—and especially the interviews, field work, and ethnography—would drive home the need to address and operationalize the research ethics of the method.

We do not need a separate research program on the ethics of process tracing, but we should teach more about how one operationalizes the challenging ethics of immersive within-process-tracing methods such as interpretive interviewing and ethnography. Addressing ethics also forces a scholar to confront her positionality in the research process. At a minimum, and since transparency is currently much discussed among process tracers, we need to build modules into our process tracing curricula on the ethics/transparency relation and how we operationalize core ethical precepts (do no harm) in an era of open science. In making this pedagogical move, there is a rich and growing applied ethics

literature upon which we could draw (Wood 2006; Parkinson and Wood 2015; Fujii 2017; Monroe 2018; Cronin-Furman and Lake 2018; Delamont and Atkinson 2018; Kaplan, Kuhnt, and Steinert 2020).

An Interpretive Process Tracing

I start with three facts. Fact #1 is that process tracing has adopted a meta-theoretical stance where there is a place for both positivist and interpretative variants (Bennett and Checkel 2015, 10–16). Interpretive scholars in political science—fact #2—have become increasingly interested in process—a move most clearly seen in the “practice turn” (Neumann 2002; Adler and Pouliot 2011). Practices are “inarticulate, practical knowledge that makes what is to be done appear ‘self-evident’ or commonsensical” (Pouliot 2008, 258). Practices are built on a relational ontology that mediates between structure and agency (Adler and Pouliot 2015); meta-theoretically, they thus capture process and social mechanisms (Guzzini 2011). In terms of method, this means interpretive scholars need techniques that gather data on and measure process—something like process tracing.

Given these first two observations, fact #3 is a surprise: There is little interpretive process tracing. It is taught virtually nowhere, perhaps because our leading process tracing texts have almost nothing to say about it (Bennett and Checkel 2015; Beach and Pedersen 2019).¹⁴ Regarding empirical applications, there are but a handful of published works that utilize interpretive process tracing (Guzzini 2012; Norman 2015; Pouliot 2015; Norman 2016; Robinson 2017; Cecchini and Beach 2020).

This small literature is quite diverse and there is no agreement on what constitutes an interpretive form of the method. For some (Norman 2016), interpretive process tracing is similar to mainstream, positivist/scientific-realist efforts, but operates inductively. Others (Robinson 2017) favor a stronger grounding in an interpretive meta-theory, but it is not clear what the actual process tracing does.

Scholars working on practices have come the furthest in developing an interpretive form of process tracing—perhaps not surprising as social practices are all about process. Pouliot (2015) is explicit on this point: What he calls “practice tracing” is interpretive process tracing. True to an interpretive ethos, he crafts a process tracing that operates inductively, but also takes considerable effort to show how it would work. Pouliot does this by engaging with Bennett and Checkel’s (2015, Ch. 1) 10 best practices for process tracing, and how they must be modified to work interpretively. His resulting practice tracing occupies a meta-theoretical middle ground, showing how practices create meaning (interpretism), but also thinking hard about how to measure the process through which those practices operate (scientific-realist).¹⁵

Other scholars working on social practices build upon Pouliot, but argue for additional within practice tracing methods. Whereas Pouliot (2015) captures practices through ethnography and interviews, Cornut and Zamaroczy (2020) add an interpretive form of document analysis to this mix. All this work is promising and exciting, as it marks the beginning of a conceptually clear and empirically operationalized interpretive process tracing.

At the same time, practice tracers will need to address two challenges. First, it is not clear how either interviews or document analysis can measure social practices.¹⁶ Recall that such

practices are “inarticulate, practical knowledge”—in layperson’s terms, stuff that is implicit and in the deep background. Ethnography, with its commitment to immersion, is best placed to access such background knowledge; however, it is not clear how asking questions or reading documents can do the same. With interviews, the researcher is interfering with and indeed likely changing the interviewee—through the questions she asks, her gender, etc. (Fujii 2017). Accessing implicit background knowledge through all this distortion seems next to impossible.

Second, whatever additional methods they decide upon, practice tracers need to operationalize them in greater detail. Consider ethnography, which is the “gold standard” method for practice tracers (Pouliot 2010). When done well, ethnography addresses—before going to the field—two issues that bedevil it: access and ethics. Thinking about the former requires operational plans for dealing with gatekeepers (Gusterson 2008), while getting the ethics right involves much more than ticking the boxes on documents submitted to your institution’s ethics review board (Delamont and Atkinson 2018). Practice tracers—to date—have been silent on both issues.

Summary

For process tracing as method, there is a rich pedagogical and research agenda to be pursued. It would rethink and broaden the manner in which process tracing operationalizes research transparency; deepen it (within-process-tracing methods; ethics); and expand it to interpretive forms. This agenda is meant to complement—and not replace—the focus on formalization and transparency. There is nothing wrong with the latter. Perhaps process tracing needs further formalization, but we should do this with an appreciation of the likely opportunity costs. We may get a more rigorous, transparent version of one type of process tracing: deductive, scientific-realist/positivist. But we will miss an opportunity to develop a richer, more ethically grounded, meta-theoretically plural method.

CONCLUSIONS

The “qualitative” in qualitative IPE is in need of renewal. Scholars working in this tradition have made very good use of the case study method, producing a number of excellent articles and volumes structured around case study narratives (Blyth 2002; Kirshner 2007; Blyth 2013; Helleiner 2014; Ban 2016; see also Odell 2001). However, while IPE scholars stayed with the case method, the qualitative methods literature moved on, arguing that one needed additional methods—within the case—to structure all that data collection (George and Bennett 2005; Mahoney and Thelen 2015; Beach and Pedersen 2016).

This chapter considered one particular additional method—process tracing—that should be of special interest to qualitative IPE. Yet, as my review demonstrates, IPE has at best a tenuous relation to the method. Too often, process tracing is employed, but in an implicit way that makes its application invisible to the reader; in other cases, it is explicitly used, but in a metaphorical and non-operational manner. One could consider these criticisms as

nothing more than methodological nitpicks, but they matter: weakening the validity of the causal and interpretive claims advanced by these scholars.

Certainly, qualitative IPE is not alone in its non- or misuse of process tracing; one sees similar methodological misfires in other subfields (international relations) and literatures (comparative historical analysis). In addition, the expectations for how we use an array of qualitative methods have been rising fast in recent years. This latter fact alone makes it almost impossible for qualitative IPE to meet the methodological concerns of a sympathetic but critical analyst applying contemporary standards.

While this context is important, it does not change the chapter's bottom line: Qualitative IPE needs to finish the job. The case study narratives it has mastered need to be followed and complemented by methods like process tracing. Above, I outlined a three-fold agenda for IPE to pursue as it applies process tracing. The reaction of some may be: "Is Checkel serious?" "Does he want to make us methodologists?" My reply: "No and no!"

I answer in this way for three reasons. First and conceptually, process tracing ain't rocket science. It is true that to use it well requires a clear understanding of causal mechanisms and the different, processual understanding of cause they carry. However, this is nothing like getting one's head around, say, the Boolean logic of qualitative comparative analysis. More important, for the aspiring process tracer, the literature now offers clear, operational discussions of such mechanisms (Hedstroem and Ylikoski 2010; Bennett 2013; Beach and Pedersen 2016, Chs 3–4).

Second, while my research agenda for process tracing may look daunting to a qualitative IPE scholar new to the technique, it is not. Its starting point is the shared understanding we now have of process tracing as method.¹⁷ Yes, it needs corrections and adjustments, but these are fine tuning what we already have. In practical terms, for the IPE scholar wanting to use process tracing, the start-up costs are therefore not high. She should begin with one of the foundational texts (Bennett and Checkel 2015; Beach and Pedersen 2019) and then turn to the work of specific scholars for getting the operational details correct—be it on applied ethics, practice tracing, or Bayesian process tracing. In this chapter, I purposely provided extensive citations—for the IPE scholar seeking this operational detail.

Third, qualitative IPE is already producing excellent work where the parts are there for the application of process tracing. Consider Sending and Neumann's (2011) study of the World Bank's social practices, where they trace the emergence of multiple practices within the Bank and how these define the boundary between it and member states; this is an application of practice tracing in all but name.

Process tracing is not manna from heaven. Done well, it can take considerable time, and it has an inferiority complex dating back to the days when leading methodology texts considered it little more than journalistic "soaking and poking" (Gerring 2006, Ch. 7). There are also parts of IPE where it likely has no role to play—Marxist approaches, world-system theory, or research based on experimental designs.¹⁸ Yet for that substantial body of qualitative IPE surveyed in this chapter—with its interest in historical processes and dynamics, in causal mechanisms, in capturing the complex interplay between the global political economy and domestic politics—process tracing is a practical tool that will help it make better arguments, which, in turn, will be read by a broader set of scholars. The investment is worth it.

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NOTES

1. Keyword search terms: “process tracing” and “qualitative studies.”
2. Keyword search term: “process tracing.”
3. A few comments on the data are necessary. First, despite sampling limitations, I am confident of the conclusions drawn—both because the findings are so strong and because they are consistent with my own “outsider” sense of qualitative IPE. Second, we searched for both “process tracing” and “qualitative studies” as it quickly became apparent that “process tracing” alone would miss the many instances when the method was being used implicitly. Third, the time span chosen (2010–2020) captures the most recent and methodologically advanced qualitative IPE. In addition, by 2015—the mid-point of this period—process tracing as method had come into its own, as evidenced by the publication of two well-received “how to” volumes: Beach and Pedersen 2013; and Bennett and Checkel 2015.
4. My thanks to Wolfgang Minatti for the data collection and part of the analysis in this section.
5. In the remaining 10 articles, the reference for process tracing is simply to cite another scholar using the method in her study.
6. In the surveyed literature, we found only one instance where the author emphasized transparency, in this case, including an appendix to outline the logic behind his analytic reasoning: Meissner 2019.
7. The other good examples of deductive process tracing in qualitative IPE are: Trampusch (2014); Monheim (2014); Weinhardt (2017); Meissner (2019); and Weiss (2020).
8. The data on course content comes from my own involvement in the APSA short courses, plus a review of online syllabi/course-descriptions for the other schools and institutes, for the years 2018–2020. See also Zaks (2021, 59).
9. For the period from January 2017 to August 2020, I searched: (1) the journals *Political Analysis*, *Perspectives on Politics*, and *Sociological Methods & Research*; (2) publications listed in Google Scholar for Tasha Fairfield, Andrew Bennett, Derek Beach, and James Mahoney; and (3) the International Bibliography of Social Sciences database. Keywords used were “process tracing,” “Bayesian,” “set theory,” “formalization,” and “qualitative methods.” The search resulted in 20 articles, book articles, and newsletter contributions on process tracing. Of these, 11—or 55 percent—dealt in whole or in part with formalization, understood as Bayesianism, set theory, or causal graphs.
10. For background on DA-RT and the debates it has spurred, see Symposium (2014); Symposium (2015); Symposium (2016); Hall (2016); and Monroe (2018).
11. See <https://www.qualtd.net/>. These deliberations and subsequent reports were organized by the American Political Science Association’s Organized Section for Qualitative and Multi-Method Research; see Jacobs and Buthe (2021) for an overview of the process and findings. On the dominant positivist impulses shaping DA-RT, see also Isaac (2015).

12. I have been one of the lecturers at the APSA short course most years since 2014.
13. Neither of the two main process tracing textbooks—Bennett and Checkel (2015); Beach and Pedersen (2019)—devote a chapter or even a section of a chapter to research ethics.
14. On the teaching data, see n. 8.
15. More generally, the most exciting and innovative theoretical-methodological work occupies precisely this epistemological middle ground. See Hopf (2002); Hopf (2007); Pouliot (2007); Hopf and Allan (2016).
16. While recognizing they are a clear second best, Pouliot (2010, 66–72) offers a more optimistic take on the ability of interviews to access practices.
17. To appreciate the extent of this understanding, see the broadly similar way the method is now being taught at the ECPR summer and winter methods schools, IQMR in Syracuse, and the APSA short courses.
18. However, see Dunning (2015) for a brilliant argument that experimental designs actually require a prior stage of process tracing.

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CHAPTER 14

HISTORICAL INTERNATIONAL POLITICAL ECONOMY

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INTRODUCTION: WE ARE ALL HISTORIANS NOW; AND WE ALWAYS HAVE BEEN SO

THE field of International Political Economy (IPE) was born of history. This is true in a formal sense as several of IPE's "intellectual entrepreneurs" emerged from history departments (Cohen 2008, Introduction). But it is also true in a deeper sense. The great puzzles in those early days, concerning the evolving structures of the global political economy, were overtly historical (Cox 1983; Kindleberger 1986; Krasner 1976; Strange 1987). This is hardly surprising given that each of IPE's major paradigms—liberalism, mercantilism, and Marxism—is historical in its own way.¹

Historical analysis transcends the classic "rationalism-constructivism debate." Several of the central concepts in neoliberal institutionalism—repeated play, the shadow of the future, and reputation—are temporal in nature. In this approach, actors' decisions at any given point must be analyzed within the longer-run "historical" context of the dynamics that preceded this moment and the consequences that were expected to follow (Axelrod and Keohane 1985; Keohane 1984; Oye 1986; Snidal 1985). So, too, with actors' identities, ideas, and interests—the construction of which is a historical process (Goldstein 1993; McNamara 1998).

Conventionally, "historical method" connotes analyzing evidence from the archives in narrative form. But what is an "archive"? What counts as "historical evidence"? And what is a "narrative"? If all the vital facts were preserved within ministerial "structure[s] of glittering white concrete," we should have our answers easily (Orwell 1961, 4). But that is less true now than ever before.

Indeed, historical IPE spans all methodological cleavages as scholars pursue knowledge and understanding in ever more places using a growing set of tools. Consider two recent publications in the *Review of International Political Economy*: Sahasrabuddhe's analysis of federal currency swaps during the Global Financial Crisis; and Jamal and Milner's survey experiment on Tunisians' support for globalization. Initially, it seems that the articles have little in common. But, from a broader view, both relay narratives/findings of how actors/

respondents reacted to a shock/treatment. Both do this toward the end of teasing out broader lessons/implications about the role of ideational and material variables, even as both acknowledge the limitations of generalizing from their limited cases/samples. The principal difference lies in the nature of the experiment analyzed—natural versus contrived (Jamal and Milner 2019; Sahasrabuddhe 2019). Yet, once the experiments have run their course, both are history.

Thus, in a most banal sense, historical IPE is simply the study of IPE “in the past.” But empirics are always things that have happened “in the past,” whether that “past” was a few millennia or a few moments ago. These days, there is rather little in IPE that is devoid of empirics. But to say that “we are all [historians] now,” is not to suggest that we all “do history” in the same way (Friedman 1966; “We Are All Keynesians Now” 1965). Here, there is “constructive non-engagement” among different groups of scholars who grapple with history in different ways, who see history as operating on different scales, and who engage history for different purposes.

HOW WE ENGAGE HISTORY

Broadly, scholars take three approaches to history in IPE. There are those who consider the history of the whole global political economy as such. A second group compares the paths of particular units within that system. Finally, there are those who trade breadth for depth.

Holistic IPE

Following Marx, Andre Gunder Frank famously argued that global political economy can only be understood “holistical[ly]”—“in terms of the whole world economy/system” (2007, 39). Frank combatted the traditional “stadial” theories of development—dating back to Adam Smith—in which countries progress through a “succession of capitalist stages” with “today’s underdeveloped countries . . . still in the stage, sometimes depicted as an original stage, of history to which the now developed countries passed long ago” (Frank 2014, 106). Instead, Frank argued, “contemporary underdevelopment is in large part the historical product of past and continuing . . . relations between the satellite underdeveloped and the now developed metropolitan countries” (2014, 106). Wallerstein distilled Frank’s perspective into World-systems Theory. “Capitalism,” Wallerstein explained, “was from the beginning an affair of the world-economy and not of nation-states” (1974, 401). For him, the “basic unit of observation” was “a concept of world-systems that have structural parts and evolving stages.” Only “within such a frame-work,” can one “fruitfully make comparative analyses—of the wholes and of parts of the whole” (1974, 415).

The Hegemonic Stability Theorists were quintessentially holistic in their approach. They explained the overall structure of the international system with reference to the “distribution of potential economic power” among the systems’ states. For them, a given state’s foreign economic policies could not be understood apart from that state’s position within the structure at the time (Krasner 1976).

While Hegemonic Stability Theory is now seen as passé, holism continues to thrive among many of today's critical histories of the global political economy. In particular, Trotsky's infamous rendering of history as the story of "uneven and combined development" has found new life among those scholars seeking to take the history of the global political economy beyond "Eurocentric" tropes (Anievas and Matin 2016). Even those who have complicated World-systems Theorists' "homogenous" reductions similarly describe "a system that was simultaneously both intensely connected and deeply divided." These views are not less—but more—holistic. "Global modernity," Buzan and Lawson argue, "pulled the world into a *single* system." The "global transformation" of the nineteenth century "foster[ed] the emergence of a *full international system*."²

In each of these holistic views, the international system has not only a history but also a trajectory. But where, precisely, are things going? All the major permutations have been predicted. The Hegemonic Stability Theorists—particularly Gilpin—saw a cyclical tendency (Gilpin 1987, xiii). Frank, by contrast, hailed "the return of the world's economic centre to Asia, where it had always been before the past period of temporary Western ascendancy" (2007, 52). Wallerstein predicted something like chaos. "We have now entered into the post-American era, but also the post-liberal era," he proclaimed. It "promises to be a time of great world disorder, greater probably than the world disorder between 1914 and 1945 . . .," he warned (1993, 4). Buzan and Lawson, by contrast, see history unfolding along a certain path but not necessarily toward any knowable endpoint. They see the twentieth and twenty-first centuries as "close kin" to the nineteenth century; but we have yet to see the full realization of the dynamics unleashed in that previous "global transformation" (2015, 5).

Comparative History

Those writing comparative history reject what they see as an over-emphasis on the global setting of particular elements and episodes of foreign economic policy.³ They do *not* suggest that "the international system"—however understood—is irrelevant. But, for them, the international is just one of the several "levels" from which influences emanate. They compare distinct experiences and moments both macro- and microscopically. This increases the number of observations and, at its best, enables the formulation of generalizable lessons.

These days, comparative history is the most prevalent approach to history in IPE, and there is no shortage of exemplary work. It might be most useful to highlight the underpinnings of this approach by examining two of the pathbreaking pieces that have exerted an enduring influence on the discipline.

First, Rogowski's classic work on trade and domestic political cleavages was theory-driven. From the start, he acknowledged that his "preliminary survey of the evidence" was "sketchy" (1987, 1127). But even his more extensive case analyses were brief. For instance, he wrote, "The details of the US experience are too familiar to require extensive discussion . . . workers joined owners in support of a triumphant, highly protectionist, and increasingly imperialist Republican party" (1989, 43–4). And, he conceded, "the present theory can offer no explanation whatever for the acute class conflict that characterized Britain in the interwar years" (1989, 82).

Second, Simmons's classic analysis of interwar foreign economic policy squared off directly against the central accounts offered by the Hegemonic Stability Theorists. Like

the holists, she recognized that states' different positions within the global economic structure shape the constraints and opportunities that their policymakers face. Yet, she highlighted, this was insufficient to explain why similarly situated countries reacted to their circumstances differently. It was thus necessary to "supplement" "systemic international relations theory" with historical analysis of the "*variations* in policies over the two decades and from country to country."⁴ She concluded, "the ability to comply with internationally accepted adjustment norms may be conditioned by the domestic political and institutional variables that influence a country's macroeconomic tastes more generally" (1994, 46).

Deep History

A third category of IPE scholarship emphasizes the cross-temporal and cross-sectional variation that marks the global political economy. It celebrates the "messiness," as Phillips and Sharman put it, of "the heterogenous international systems that have predominated for most of world history"—"international systems," plural (2015, 3–4). It revels in the distinctiveness of particular cases and contexts rather than pressing their comparability.⁵ This can lead to mere "story telling" in which we learn more and more about less and less. But deep history trades generalizability for analytic precision and theoretical parsimony for empirical robustness.

Whereas comparative history is often theory-driven, deep history typically begins with the history—the empirics. Consider how Schonhardt-Bailey casts the puzzle in her landmark analysis of the repeal of the Corn Laws in 1846:

This is a book about an anomaly of spectacular proportions. For more than 150 years, researchers have sought to unravel the puzzle of repeal. Indeed, this anomalous case continues to spark the curiosity of leading political scientists on both sides of the Atlantic into the twenty-first century.

(Schonhardt-Bailey 2006, 2)

Schonhardt-Bailey was intrigued by the analytical challenge presented by the elusive puzzle. She renounced "the attractive . . . simplicity" of an account based exclusively on "economic interests" and instead grappled with the "thicket of overlapping and intertwined interests, ideas, and institutions" (Schonhardt-Bailey 2006, Preface, 2).

But it is not just that Schonhardt-Bailey developed a multi-causal explanation. Her deep history *emphasized* the complexity of the course of events:

Britain's bold move to free trade in 1846 . . . violated the core protectionist ideology of the Conservative party while simultaneously undercutting the economic interests of the ruling landed aristocracy. The Conservatives entered government in 1841 with a strong and . . . unified commitment to protecting agriculture, and yet . . . Prime Minister . . . Peel, completely reversed this stance within five years.

(Schonhardt-Bailey 2006, 1)

This is nothing like the "clearly defined" "stable class cleavages" described by Hiscox (2002, 71). Nor was it reducible to Rogowski's just-so story in which "capitalists and labor . . . unite[d] effectively in the Liberal party and forced an expanded suffrage and curtailment of (still principally landowning) aristocratic power" (1987, 1125). Schonhardt-Bailey shows

that these simplified explanations are manifestly inaccurate. And they gloss over the far more profound puzzle. “Rarely,” she notes with delight, “do we find a case in which political representatives acted seemingly against not only their own economic and political interests but also against the mandate on which many of them were elected” (2006, 1). And, yet, this is precisely what happened in 1846. Schonhardt-Bailey resolves the puzzle only by working through thousands of pages of primary source material, analyzing them with a mix of traditional and cutting-edge methods across hundreds of pages.⁶

Staying with the UK for illustrative purposes, deep histories reveal the same types of limitations with comparative histories of the interwar period as well. Simmons’s “explanatory domestic political variables” include regime type, the political orientation of party in power, labor unrest, government instability, and central bank independence (1994, 42–6). Such a general typology is necessary for useful comparison across contexts; but the fit with specific cases is always subject to contention.

For instance, Simmons scores the Bank of England—the “most independent of central banks”—as fully independent across this period (1994, 230, Appendix II). But deep histories show a more complicated story. During and after the First World War, Bank Governors Cunliffe and Cokayne broadly deferred to the Treasury in setting monetary policy. Montagu Norman came to power in 1920 determined to reverse course. He spent the next two and a half decades establishing the Bank as the ultimate arbiter of macroeconomic policy. This reached a crescendo in 1931, when the Bank dictated fiscal (as well as monetary) policy to the government in its misguided campaign to save the gold standard. Thus, the Bank’s “independence” was hardly invariant. More importantly, the Bank’s securing of its “independence” is itself a key variable to explain.⁷

On regime type, Simmons counts the UK among those powers “that remained democratic for the entire period” (1994, 293). Yet, the UK’s “regime-type” was itself transformed within this period. Prior to 1918, all women and poor men (42 percent of the total male population) were disenfranchised. The Representation of the People Act 1918, granted wealthy women the right to vote and removed the property requirements for men, expanding the total suffrage from eight to twenty-one million. It was only with the 1928 Equal Franchise Act that women were given voting rights on the same basis as men. One hesitates to count a country a “democracy” when one-third of the adult female population is denied the right to vote.⁸ Substantively, this matters because those most negatively affected by the foreign economic policies of the interwar period were those poor men and women whose voting rights were most restricted. Moreover, as Teele shows, these changes in the suffrage themselves interacted with the broader struggle between the working class and the Conservative and Liberal elites (2014).

This oversight is part of an oversimplification of the domestic politics that defined the UK’s response to its changing position in the international system. The typical story has been that the UK left the gold standard in 1931 in the context of “labour unrest” and an “unstable,” “Left-wing Government” (Simmons 1994, 281). Indeed, the Labour government—supported by the Liberals—was so “unstable” that it fell entirely just prior to the suspension of gold convertibility. But it was replaced with an effectively *Conservative* government. The “unrest” at that time was nothing compared to the strikes in 1919 and 1921, let alone the 1926 General Strike—the largest strike in Western European history (Shefftz 1967, 387). No doubt, this was due to the Parliamentary Act severely *restricting* labor rights in 1927. Even prior to this, however, the “unrest” did nothing to thwart the return to gold or to “reform” *laissez faire*.

Indeed, the 1931 General Election saw the Conservatives win the largest electoral mandate in modern British history by promising to save the gold standard. They did not deliver but, instead, introduced protection, after it was “no longer necessary” economically (Keynes 1931). More broadly, the leading parties’ orientation on the major issues—trade and the gold standard—shifted considerably across the interwar period.⁹ This revolution followed from both the “unintended experiments” of elites and the “everyday discourses constructed by mass public agents” (Morrison 2016; Seabrooke 2007, 795). In all of these ways, the history of political-economic reality defies the elegance of generalized IPE theory.

Specialize and Trade for Richer Histories

It is worth considering the UK’s lead toward, and away from, the liberal international order for several reasons. First, the UK has had an outsized effect on the trajectory of IPE across the last several centuries. For better and for worse, the decisions made in London substantially altered the constraints and opportunities for many other actors, from Dublin to Delhi. Perhaps for this reason, the UK has assumed an altogether outsized importance in the field. From the Hegemonic Stability Theorists to the pathbreaking comparativists, the UK has been IPE’s indispensable case. And despite that it is also the most heavily conceptualized case, new methods and new perspectives continue to grant new purchase on even this well-trod ground.

So, it is worth understanding the dynamics that gave rise to those world-changing shifts emanating from the world’s “core” political-economic centers. Such deep histories might evade the sin of Eurocentrism by disposition, as they interrogate how some powers assumed such influence at these pivotal moments. But they do not counter it. The field needs more such deep—and deeply informed—historical analyses of other places, like those offered by Pepinsky (2007), Naqvi (2018), and Dafe (2019) in recent years. Such new histories provide fertile ground for building and testing theory in less familiar contexts. So, too, does the field want serious engagement with non-Western theories of IPE as such, like Chey’s (2019) exemplary analysis of the history of Korean IPE thought. Taken together, such work offers starting points for crafting “connected histories” within IPE (Subrahmanyam 2004a, 2004b).

But whether even a multitude of rich case studies offers generalizable lessons can only be determined by studying history comparatively and globally. Ultimately, IPE benefits from historical work done in all three modes: holistic, comparative, and deep. As Pevehouse and Seabrooke (this volume) remind us, it is more important than ever that scholars bridge such divides. The oldest lesson of IPE remains true here as elsewhere: we all gain when we specialize according to our comparative advantages and then trade the fruits of our labor.¹⁰

HOW MUCH DOES HISTORY MATTER?

Originating in the subfields of American and Comparative Politics, *historical institutionalism* has become increasingly central to the analyses of International Relations (IR) and IPE. Broadly, there is agreement about the pre-eminent *ways in which* the past shapes the present. “That tradition’s core,” Fioretos (2011, 370) explains, “includ[es] the role of founding

moments in shaping later developments, how institutional legacies affect the degree to which power resources can be harnessed, and the ways in which varied patterns of incremental adaptation shape institutions over time.”¹¹ Yet, there is little consensus about *the extent to which* history exerts its influence. From Brexit to the rise of Trumpism, even the most surprising shifts in global political economy of the recent past are subject to the timeless debate over the relative weight of structure and agency, inevitability and contingency (Peterson 2018; Thompson 2017). This section will review exemplary work from both perspectives.

History’s Heavy Hands

When asked to judge the consequences of the French Revolution, Chinese Premier Zhou Enlai is reputed to have replied, “It is too soon to tell.” While apocryphal, the quip epitomizes his long-run perspective. Such is the heavy hand of history that its effects must be gauged across at least centuries—perhaps millennia.

Some of the most prominent historical work in IPE emphasizes the ways in which history compounds the effects of small differences over the (very) long run. Most famously, Acemoglu, Johnson, and Robinson identified colonial-era institutions as among “the fundamental causes of the large differences in income per capita across countries” today (2001, 1369). In the decades since, subsequent analyses have enriched our understanding: by showing that such initial differences may produce complex dynamics that evolve over time (Stasavage 2014); by exploring the origins of such key institutional changes (North et al. 2009); and by revealing that such divergences may go back even further—and affect more today—than previously recognized (Gaikwad 2020).

Similarly, many of our most important ideas and identities prove remarkably stubborn. This is true for leading individuals, most of whom refuse to “learn” even as new facts come to light (Blyth 2013; Chwieroth 2009; Nelson 2017). But this is also true for states and international organizations. Consider Zarakol’s recasting of “reputation” as *stigma*, “a label of difference imposed” on, and internalized by, those actors judged as “backward” by the “modern” state system. Even as countries’ circumstances shift radically—and regimes become “zombies”—perceptions, and behavior, can remain much the same (Gray 2018; Zarakol 2011, 4).

Today, leading international regimes theorists see history as continuing to loom large. While he has acknowledged the importance of critical junctures and founding moments, Ikenberry contends that “the old American-led hegemonic system” has proven “durab[le]” even in the face of “remarkable upheavals in the global system—the emergence of new powers, financial crises, a global recession, and bitter disputes among allies over American unipolar ambitions” (Ikenberry 2011, 6). Such accounts are partly explicable in terms of path dependency: any such entrenched order is unlikely to be overturned easily. But the American-led order maintains its inertia today partly because it has demonstrated (repeatedly) that it “works” (Drezner 2014, Ch. 2).

For the global order’s critics, it still “works” for the wrong sorts. Initially, there were hopes that the Global Financial Crisis might signal the “last gasp of [the] old-fashioned concert of great powers” (Woods 2010, 51). In the years since, it seems that Strange’s decades-old warning was prescient. The “persistent myth of lost [American] hegemony” has only fanned the flames of America First and American ego-centrism, even while the US has remained vital to the structures of the global economic order (1987). For Frank, even the resurgence of

China, one of the largest potential destabilizers, is intelligible only in terms of the “long historical continuity” of the processes of the “Rise of the West” and “[t]he (cyclical) decline of Asian economies” (2007, 39–40).

But is the global political-economic order really so obdurate?

Making History

“The power to become habituated to his surroundings is a marked characteristic of mankind,” Keynes wrote in the wake of the First World War. Yet, he warned, “[v]ery few of us realise . . . the intensely unusual, unstable, complicated, unreliable, temporary nature of the economic organisation by which Western Europe has lived for the last half century” (Keynes 1920, 3). Frieden repeats the warning: “Today’s international economic order also seems secure, but in historical perspective it may be only a brief interlude. The historical forces that shaped the world economy in the twentieth century continue to determine today’s version of globalization and will decide its fate” (2007, xvii). In such accounts, the structure of international politics is supple and the stream of time, mercurial.

Several histories of the Anglo-American postwar order paint images of perennial change, complete with regular moves by the leading powers to exit and re-make the principal institutions that comprise this regime complex. Gowa (1983) showed this in the case of the International Monetary Fund (IMF) in her classic study. Barton et al. (2006) have told an analogous story in their history of the trade regime. The story is not as plain in the case of the World Bank. But, as Allan (2019) shows, the US steadily advanced a “neoclassical growth imperative” across the postwar period, even as the content of that vision evolved over time. Regardless of the direction of change, such accounts remind us that change is the norm, that our multilateral economic institutions are always contested and always evolving, as both Seddon (2017) and Kaya (2015) describe.

So, too, with actors’ all-important reputations. Analyzing sovereign debt across three centuries, Tomz (2012) shows that one’s “reputation” is far more nuanced—and mutable—than we have traditionally assumed. One way to alter a reputation, Gray (2013) finds, is by changing the “company states keep.” It seems that Benjamin Franklin’s aphorism that good reputations are hard-won and easily lost applies to IPE as well.¹²

Moreover, not all of the challenges we face are familiar. Climate change is without precedent and, arguably, without even analogy. But “the *idea* of climate emerged from a process of co-production in which both state imperatives and scientific knowledge shaped and reshaped the form and content of the climate.” It was *constructed* as an “epistemic” “governance object”—which is to say that there is a history to our understanding of, and approach to, the climate crisis.¹³ More broadly, all “[c]rises”—and the “appropriate” responses to them—“... are constituted in and through narrative” (Hay 1996, 254).

EMANCIPATION(,) FROM HISTORY

At its best, historical IPE is emancipatory. Studies of trade sometimes read like so-called “Whig histories,” with their notion that “cooperation” is “Pareto optimal”—or, at least, that

trade war is typically suboptimal (Krasner 1976, 318). Such narratives tend to elucidate those factors that have enabled liberalization—read: liberation—from primitive protectionism.¹⁴

In other areas of IPE, the multiple equilibria are more ambiguously ranked. Scholars of money are acutely aware that the story of international monetary order across the last several centuries includes at least as many steps backward as forward. This history proves invaluable for putting the present into proper perspective (Helleiner 2003). So, whether we are considering the “future of money” (in general) or just the future of particular challengers to dollar hegemony, historical analysis is a necessary precursor, even if only to reveal the limits of precedent (Cohen 2006; Matthijs and Blyth 2015; McNally and Gruin 2017, 603).

The field of migration is darker still, as the work of Peters and Watson shows. At first blush, their treatments appear to be a world apart—emblematic of the “trans-Atlantic divide” (Cohen 2007). Peters uses a positivist, mixed-methods approach to analyze the empirical evolution of the international migration regime across two centuries; while Watson offers a reflexive account of the use of *Robinson Crusoe* in economics textbooks to “construct” a particular “theory of market exchange” (Peters 2017; Watson 2018, 547). Yet, Peters’s “policymakers” and Watson’s *Crusoe* (as lawgiver) both overtly treated the subjects of rule as “nothing more than a combination of latent labouring possibilities” (Watson 2018, 550). In both analyses, we see that international migration regimes depend upon the broader social, political, and economic structures in which they are generated. The disappointing conclusion is that anyone seeking to understand the regime governing, say, North African migrants in Europe today, must recognize both that these individuals are evaluated in a “raced market frame” and that their “rights” are determined by a complex of international arrangements that govern how capital is mixed with labor and how goods are brought to consumers.

The same is true with other frames—such as gender—and across other issue areas within IPE, as Elias and Roberts (2018), among others, show. In all of these areas, exploring how things became the way they are is a necessary preliminary to advancing alternatives. But whether the mode is excavation or deconstruction, laying bare the foundations of the present is a preliminary to renovation.

For others, history is invaluable as a means to revisit roads not taken—and to reassess our shared narratives about the roads we have taken. Chang’s brief but provocative book employs “the historical approach in the study of economic development” both as a source for “new” ideas and to furnish empirical evidence in support of the proposals that follow therefrom (Chang 2002, 8). This is, itself, reminiscent of the German Historical School’s infamous rehabilitation of mercantilism in Imperial Germany (Schmoller 1896). And so, too, is the response. Just as Viner challenged the historical veracity of Schmoller’s account, so too has Irwin challenged Chang (Irwin 2004; Viner 1948).

As ever, our understandings of the past shape our perceptions of the present—and vice versa. This is why we always have done—and always shall do—history in IPE.

CONCLUSION: THE FUTURE OF HISTORICAL IPE

From “Make America Great Again” and Brexit in the West to the textbook controversies in the East, the “culture wars” over the rendering and meaning of history have never loomed larger (Bell and Vucetic 2019; Shin and Sneider 2011; Watson 2019). IPE is uniquely

positioned to shape those debates. Its deep, expansive roots in historical analysis offer a wealth of wisdom. Its appreciation of the political and economic consequences of narrative ensures an earnestness too seldom supplied in an age of politics-by-Tweet. Its commitment to inclusion across sectional divides promises a diverse array of perspectives, a powerful toolkit, and a global sensibility. We have much to offer, indeed.

But realizing that potential requires more “constructive engagement” across our traditional cleavages. Our history is too rich and our understanding of it, too important, to leave it to any particular method of analysis, to any single mode of engagement, or to any specific locale of scholarship. We have always been historical, and we shall always be so. But the histories of the future need not look strictly like the histories of the past. Let us couple the tried and true modes, traditional methods, and familiar foci with new approaches, modern methods, and an ever more diverse cast of characters and contexts. This will make our accounts more inclusive, more interesting, and—crucially—more accurate. By deliberately bridging these divides, we will see more of our world—and see all of it more richly.

NOTES

1. The classic liberals believed in the promise of progress–improvement over time. Locke (1967) *Second Treatise*, Ch 5 §§ 39–51 (Hume 1987); Smith (1976) Bk III. Marxists contend that the march of history is resisted by entrenched elites defending their interests (Marx 1977, 176–98). The mercantilists used history to expose liberals’ hypocrisy (Schmoller 1896).
2. Emphasis mine. Buzan and Lawson (2015, 2–3, 9).
3. Rogowski, for instance, quite deliberately followed Barrington Moore’s “comparative sociology” Rogowski (1989, 33).
4. Simmons (1994, 3, 13), emphasis in original.
5. Cf. Rogowski’s suggestion that “Britain” “at the middle of the nineteenth century” “can stand as the surrogate for the advanced and labor-rich economies of northwest Europe generally” Rogowski (1987, 1128).
6. Dean (2016) has similarly highlighted key problems in these comparative historical accounts of US trade policy in the late nineteenth century.
7. Moggridge’s several books and Sayers’s 3-volume history work through these questions. Moggridge (1969, 1972); Sayers (1976).
8. UK parliament (2021). The UK’s Polity IV score was an eight from 1901 through 1922, when it then jumped to a ten (Marshall et al. 2018). In 2016, it reverted to an eight (Marshall et al. 2018). Of course, Polity scores are not calculated based strictly on the size of the suffrage in national elections. But given the massive changes in the rights to vote in 1918 and 1928, it is hard to understand the reasons for these codings. This author has raised these concerns with the authors of the Polity IV dataset, but they have not responded at the time of publication.
9. To his credit, Hiscox noted the “substantial decline in party cohesion” in this period. But, surprisingly, he explains these changes without any reference to the gold standard (Hiscox 2002, 79). Williamson explores at length the centrality of the gold standard to these other shifts (Williamson 2003).
10. Blyth made this point to great effect in his roundtable presentation at the IPE Society meetings at the University of Pennsylvania, November 15, 2008.

11. See also Fioretos et al. (2016).
12. By leaving the gold standard in 1931, the UK liquidated the credibility it had built up by maintaining a stable gold value of the pound (almost without exception) for more than two centuries (Morrison 2016).
13. Allan (2017, 154–7), emphasis added.
14. Morrison (2012) is an extreme example. But other work within that literature has a similar flavor (Bailey et al. 1997; Hiscox 1999; Irwin et al. 2008).

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PART III

PROCESSES

CHAPTER 15

DIFFUSION PROCESSES IN INTERNATIONAL POLITICAL ECONOMY

BRIAN GREENHILL

WHAT IS MEANT BY “DIFFUSION”?

DIFFUSION refers to the process through which policies, practices, ideas, or events spread from one jurisdiction to another. Examples of this abound in international politics. For instance, the financial crisis of 2007–2009 began with a crisis in the subprime mortgage market in the United States that quickly triggered financial instabilities in many other countries. More recently, right-wing populist movements have experienced a dramatic rise in support across many Western societies. Other notable examples of diffusion that have attracted significant scholarly attention include the widespread adoption of liberal economic policies in recent decades (Simmons, Dobbin, and Garrett 2008), the ongoing (but recently stalled) spread of democracy across the world (Gleditsch and Ward 2006), and the adoption of policies promoting gender equality (True and Mintrom 2001) and LGBT rights (Kollman 2007; Ayoub 2015).

The diffusion phenomenon goes by many different names such as spillover, contagion, or interdependence. Although there can be subtle but important differences in the connotations of the various terms, the key idea is that the outcome of interest (e.g., the adoption of a particular economic policy in a state) is caused at least in part by events occurring in other states. In other words, if state A and state B both end up adopting a similar policy, then we can say that diffusion is occurring if we find evidence to suggest that state A's decision to adopt the policy was influenced by that of state B (or vice versa), rather than by either a simple coincidence of events occurring at the domestic level in both countries, or by a common external shock.¹

Diffusion can occur through many different channels. For instance, a state may be more willing to take the risk of adopting a new policy if one or more of its neighbors has already done so. Alternatively, it may look beyond its geographical neighbors and instead consider the examples set by states that constitute a peer group in some other respect—for example,

states that are culturally similar, or states that share membership in an international organization.² In other cases, diffusion can occur as a result of market forces. The much-studied “race-to-the-bottom” phenomenon describes the way in which states come under pressure to lower their regulatory standards in order to remain competitive in an international marketplace.

As we shall explore in more detail at the end of this article, the research frontier in diffusion studies has moved away from establishing evidence of the existence of diffusion through different channels and is now more concerned with questions about the conditions under which diffusion may be more or less successful.

DIFFUSION PROCESSES

Implicit in each of the channels of diffusion is one (or more) distinct diffusion processes. Although many different typologies have been suggested, perhaps the one that has had the greatest influence in IPE is that developed by Simmons, Dobbin, and Garrett (2006). They identify four distinct processes of diffusion—competition, learning, emulation, and coercion—that characterize most cases of international diffusion. For instance, the race-to-the-bottom scenario involves a process of competition. This describes a situation where states compete with one another for access to export markets or to sources of foreign investment. When faced with these pressures, one state might, for example, try to gain a competitive advantage by relaxing costly regulations governing the amount of pollution that its factories can produce. Other states will then come under pressure to adopt similarly lax regulatory standards in order to maintain their market access. Soon, more and more states will follow suit, eventually leading to the spread of weakened regulatory standards across the entire market.

A second process involves learning. In this case, policymakers deal with uncertainty concerning suitable policy choices by looking to the examples set by others. If one state has already experimented with a particular policy, then other states might learn from their successes (or failures) and adjust their behavior accordingly.³ For example, Ovodenko and Keohane (2012) argue that the US success with a cap-and-trade scheme for regulating sulfur dioxide pollution inspired the adoption of analogous schemes for regulating the emissions of greenhouse gases in Europe (although see Paterson et al. (2014, 425) for an alternative account).

A third, and closely related, process involves states emulating the policies adopted by others. The key difference between learning and emulation involves the extent to which policymakers engage in a conscious process of weighing the costs and benefits of the policy. If they carefully evaluate other states’ experiences and consider the implications for their own policy choices, we tend to say that learning is at work. If, on the other hand, they choose to adopt another state’s policy simply because they see other states doing so—in other words, it just seems like “the right thing to do”—then we tend to say that emulation is going on. Learning is therefore driven by rational, or cognitive, calculations, whereas emulation is driven by social pressures, or a desire to maintain a particular identity. The process of emulation is sometimes referred to using alternative labels such as mimicry, socialization, or acculturation.

A fourth process, coercion, involves states forcing other states to adopt a policy. In a situation of coerced diffusion, a donor state or intergovernmental organization might condition economic aid on the adoption of certain policies by the recipient state—for instance, the adoption of structural adjustment policies or austerity measures. Another example would be the US effort to impose democracy in Germany and Japan in the aftermath of World War II (Ikenberry and Kupchan 1990). In a case like this, the policy has diffused from state A to state B neither because state B was convinced of the merits of the policy (learning), nor because state B wanted to look more like state A (emulation), but rather because state B was faced with no alternative but to adopt the policy.

CHANNELS OF DIFFUSION

In addition to thinking about diffusion phenomena in terms of the causal processes involved, we can also categorize these processes in terms of the channels through which policies or practices travel from one jurisdiction to another. In this section we shall consider four such channels: geographical proximity, economic interdependence, membership in international organizations, and cultural ties. Although the channel of diffusion can in some contexts imply a particular causal process, it is usually more helpful to think of these as two independent dimensions of the diffusion phenomenon.

Geographical Proximity

A useful way to introduce the concept of policy diffusion to an audience that is unfamiliar with the term is to showcase examples where states adopt policies or practices that are similar to those of their neighbors—for instance, when Norway, Sweden, and Finland followed Denmark's pioneering efforts to grant legal recognition to same-sex unions (Kollman 2007, 336–7). While geographical proximity is certainly not the only channel through which policies and ideas can diffuse, it is perhaps the most intuitive one.

In one of the earlier applications of the concept of diffusion to international politics, Kopstein and Reilly (2000) used a model of geographical diffusion to explain variation in the extent of political and economic liberalization among post-communist states. They found that states were more likely to have had success in consolidating political and economic reforms if their neighbors had already done so, and if the country had a greater degree of openness to outside influences in the first place. In other words, their study showed that a state's geographical location can be a significant predictor of its policies.

In a study using a global sample of states, Gleditsch and Ward (2006) used data on regime types from the Polity project to study transitions to democracy over the period 1951–1998. They found that states are more likely to undergo a transition to democracy if there is a greater proportion of democracies in their immediate vicinity. (The distance threshold they used to define geographical proximity in this study was 500 km, although they note that the results are robust to alternative measures of proximity.) This finding holds when controlling for a number of other factors that are likely to influence the probability of a democratic transition at both the domestic and international levels—including, most importantly, global

pressures toward democratization as reflected in their measure of the global proportion of democracies. As with Kopstein and Reilly (2000), their results suggest that a state's more immediate regional context matters. Moreover, Gleditsch and Ward (2006, 927) found that this general finding is robust to the exclusion of the Eastern European countries that account for a significant number of democratic transitions in the post-Cold War period.

If studies like these indicate that states' policies are influenced to some extent by events that take place among their geographical neighbors, then what might this imply about the underlying diffusion processes? A couple of possible explanations exist. One is that policymakers are more likely to consider their neighboring states as relevant role models. If so, geographical proximity determines the constitution of the reference group that is relevant to the learning or emulation mechanisms discussed earlier.

A second possibility is that information, as well as people themselves, can flow more easily among states that are geographically close to each another than among states that lie far apart. This means that both policymakers and the wider public are more likely to be more exposed to—and therefore more likely to be influenced by—the ideas and practices of their neighboring states than to those of other, more distant states.

However, the geographical channel of diffusion is not just about learning and emulation; it can also operate through processes of competition and coercion. Given the obvious relationship between geographical proximity and the costs of transporting goods between states, states will often find themselves competing intensely with their neighbors for foreign investment or for access to similar export markets. For instance, Cao (2010) finds that levels of corporate taxation tend to be strongly influenced by those of their geographical neighbors.

Alternatively, geographical proximity can lead to diffusion through a more coercive process. This could result from situations where one state threatens the use of force or economic sanctions to compel its neighbors to adopt particular policies—for example, when the Soviet Union used forceful measures to suppress opposition among its satellite states. It could also result from the unintended spillover of a security or economic crisis from one country to its neighbors, which in turn forces a state to respond in certain ways. For instance, Salehyan and Gleditsch (2006) show that civil wars can spread from one state to another due, in part, to the political destabilization that is sometimes caused by refugees seeking shelter in neighboring countries. In a similar vein, Ades and Chua (1997) show that domestic political instability tends to negatively affect the rate of economic growth among neighboring countries, most likely due to a combination of trade disruptions and pressure on the neighboring countries to increase their defense spending.

Economic Interdependence

A second important channel of diffusion involves economic ties—for example, trade or foreign direct investment among states. As one might expect, the underlying diffusion process is one of competition. The race-to-the-bottom argument provides perhaps the best-known (but empirically uncertain) illustration of this, although some interesting variations on this theme also exist. One of these is the flip side of the race-to-the-bottom effect, known as the “race-to-the-top” phenomenon. This occurs when states compete with one another to offer *more* stringent regulatory standards in order to attract international investment, or to improve their chances of gaining access to more discerning export markets.

One of the most interesting examples of a race-to-the-top comes from David Vogel's discussion of the "California Effect" in the context of US domestic environmental standards (Vogel 1995). Vogel noted that California was able to use its significant share of the US vehicle market to transmit its more stringent emissions standards to other US states, as well as to countries outside the US. The logic of the California Effect works as follows: (1) voters in California lobby for stricter air quality standards; (2) in order to retain access to the California market, car manufacturers located elsewhere in the US shift to producing cars that can comply with California's stricter standards; (3) in order to protect their competitive advantage after having made these costly investments, the car manufacturers—in conjunction with environmental activists—lobby other states to adopt similarly stringent standards. The result is the trade-based diffusion of California's more stringent regulatory standards to other jurisdictions.

Diffusion studies have been used to provide evidence for a California Effect operating at the international level. Prakash and Potoski (2006) show that country-level adoptions of the ISO 14001 environmental management standard increase when firms in a given state are exporting goods to countries that have embraced the standard. Moving beyond environmental issues, Greenhill, Mosley, and Prakash (2009) find that the collective labor standards of developing countries are influenced by the labor standards found with their main export destinations. In a later study, Cao, Greenhill, and Prakash (2013) undertake a harder test of the California Effect by applying it to the cross-national diffusion of physical integrity rights—an area in which firms have less direct influence. They find that in this case trade-based diffusion of human rights can still occur, but only when the standards in the importing country are sufficiently high.

Similar arguments about race-to-the-bottom versus race-to-the-top dynamics have been made with respect to foreign direct investment (FDI). Prakash and Potoski (2007) find that adoption of the ISO 14001 environmental standard diffuses among states as a result of both FDI ties and trade ties. They argue that FDI can serve as a vehicle for transmitting superior environmental standards due to the fact that many multinational corporations (MNCs) have an interest in maintaining uniform production standards across their supply chains. This idea finds support in other studies which suggest that MNCs bring their home-country standards with them as a result of consumer pressures, efficiency considerations, habit, or through a more indirect process of knowledge transfer to other firms operating in the host economy (see Spar 1998; Garcia-Johnson 2000; Mosley and Uno 2007; Lin and Kwan 2016).

International Organizations

A third channel of diffusion involves the role of membership in international organizations. While some scholars working in this area adopt a rationalist approach to the ability of intergovernmental organizations (IGOs) to diffuse ideas amongst their member states—for instance, when the European Union uses its accession criteria to encourage democratic reforms among prospective member states—others draw from sociological insights about organizations to suggest that IGOs can "teach" their member states appropriate forms of behavior. For instance, Finnemore (1993) argues that membership in the United Nations Educational, Scientific, and Cultural Organization (UNESCO) led many states to adopt scientific research bureaucracies that otherwise would have been very unlikely to

do so. Similarly, in a study of the impact of NATO's engagement with the Czech Republic and Romania after the collapse of the Soviet Union, Gheciu (2005a, b) demonstrates how NATO worked to socialize elites in these states into adopting liberal norms concerning civil-military relations.

Other studies of IGO-based diffusion focus less on specific organizations and instead argue that IGOs can be thought to constitute a larger network through which states can influence one another. For instance, Pevehouse (2002, 2005) shows that states are more likely to undergo a transition to democracy if they belong to regional IGOs whose members are predominantly democratic. He is able to show that this IGO-based diffusion holds even after controlling for the geographical channel of diffusion discussed earlier.⁴ Other studies that examine the issue of diffusion within large networks of IGOs have addressed outcome variables such as interest convergence among states (Bearce and Bondanella 2007; Taninchev 2015), the diffusion of economic policies (Cao 2009, 2010, 2012), human rights practices (Greenhill 2010, 2015), democracy (Torfason and Ingram 2010), and even corruption (Hafner-Burton and Schneider 2019).

In general, studies that adopt the IGOs-as-network approach tend to view IGOs as social environments. This approach focuses less on coercion and competition and more on the ways in which IGOs can provide forums for representatives of different states to come together and begin to influence each other through a process of learning and/or emulation. As a result, ties to IGOs can be shown to have an impact on outcomes that are not necessarily connected to the mandate of the organization. For instance, Greenhill (2015) finds evidence of diffusion of human rights practices even among organizations that have no substantive concern with human rights issues. This approach has some similarities to the "world society" school in sociology which views IGOs—as well as international non-governmental organizations (INGOs)—as transmitting a set of liberal values to the states that engage most closely with them (Meyer et al. 1997; Boli and Thomas 1999; Frank, Hironaka, and Schofer 2000; Schofer and Meyer 2005). Unlike the world society approach, however, the diffusion studies mentioned earlier tend to view the IGO network as constituting more of a value-neutral conduit through which ideas can flow among states, potentially leading to the diffusion of both liberal and illiberal practices.⁵

Cultural Channels of Diffusion

In addition to geographical proximity, economic ties, and connections through international organizations, a fourth channel through which policies can diffuse involves cultural ties among states. In their seminal study of the diffusion of economic liberalization, Simmons and Elkins (2004) found that states are more likely to adopt liberal economic policies if a large number of states that share the same major religion (or, in some cases, language) have already done so. To be clear, their argument is not that certain religious or linguistic identities somehow make states more amenable to economic liberalization, but rather that states often look to examples set by culturally-similar others when debating whether or not to adopt a particular policy. In other words, the diffusion process that is implicit in this channel is one of learning or emulation. As a result of these findings, measures of cultural similarity such as shared religion, language, or colonial history are often included when investigating patterns of diffusion at the international level (see, e.g., Elkins, Guzman,

and Simmons 2006; Prakash and Potoski 2006; Greenhill, Mosley, and Prakash 2009; Cao 2010; Jandhyala, Henisz, and Mansfield 2011; Greenhill 2015).

HOW TO STUDY DIFFUSION

Using statistics to establish evidence of convergence is actually much easier than most people realize. One of the most widely-used approaches involves estimating a regression model where the dependent variable is the policy or practice of interest and the key explanatory variable—the diffusion variable—reflects the average level of the dependent variable found amongst each country's peers. For example, if we were interested in testing for a correlation among a state's level of democracy and that of its geographic neighbors, we could run a regression where the dependent variable is, say, the states' levels of democracy as measured by the Polity score, and the independent variable is the mean values of the Polity scores found among each state's neighbors. (This is referred to as a "spatially lagged" variable.) If the variable yields a positive and statistically significant coefficient after controlling for other factors affecting democratization, then one can infer the existence of convergence.⁶

Changing the channel of diffusion is relatively easy. If, instead of studying the impact of geographical neighbors, we were interested in, say, the impact of a country's trade partners on its probability of transitioning to democracy, we could simply substitute our indicator of geographical proximity for one that captures proximity in a trade network (see Beck, Gleditsch, and Beardsley 2006). In other words, rather than calculating the average Polity score found among each state's geographical neighbors, we would calculate the average Polity score found among the states that engage in the most trade with each of the states in our dataset.

Adopting this approach involves recording how "close" each state is to every other state in the system using a connectivity (or adjacency) matrix, which is usually referred to as the weights matrix, or W matrix. The value of each $[i,j]$ element in the matrix reflects the extent to which state i is connected to state j . For instance, if we were studying geographical diffusion, we might assign each element a value of either 0 or 1 to indicate whether or not state i shares a border with state j . If we were testing a hypothesis about trade-based diffusion (e.g., in the context of the California Effect), we might choose to assign each $[i,j]$ element a value that represents the proportion of state i 's total exports that are sent to state j . Once the construction of the W matrix is complete, all we need to do is to multiply it by a vector containing the values of the dependent variable for each of the states in the system. The result will be a new vector where each element i contains a value that reflects the average value of the dependent variable found among state i 's interaction partners, weighted by each other state's closeness to i as defined by the W matrix. This is the diffusion variable that can then be included on the right-hand side of the regression equation.

While it is relatively easy to use this approach to generate spatially-lagged variables, what is more difficult is to make the move from establishing a pattern of convergence to inferring a diffusion process.⁷ A positive and statistically significant coefficient on the diffusion variable only indicates the existence of convergence and is not necessarily indicative of a social influence (diffusion) process. Instead, the observed pattern of convergence could be the result of one (or more) of three separate causal processes:

1. A diffusion process where the behavior of one state influences the behavior of others (this claim is usually what motivated the study in the first place);
2. Some other factor (whether internal or external) that causes a group of connected states to adopt similar behaviors at around the same time; or
3. A selection (or “homophily”) process whereby states that are behaving similarly are more likely to form connections to each other.

All three of these processes would give rise to a statistically significant coefficient for the spatially-lagged (diffusion) variable, but only the first one actually involves diffusion. The challenge for the researcher is then to assess the extent to which the observed convergence might be better explained by the alternative processes described in items 2 or 3 in the above list. In most cases, researchers try to account for the second process by including appropriate control variables that account for common external shocks, or domestic variables that tend to show some degree of clustering. For instance, Gleditsch and Ward (2006)’s model of the geographical diffusion of democracy includes a variable that reflects the global extent of democratization. This allows them to infer a regional diffusion process that operates independently of a global one. Similarly, Brooks and Kurtz (2012)’s study of capital account liberalization in Latin America includes the US Federal Funds rate as a variable intended to capture more global pressures toward liberalization. Other studies include temporally lagged dependent variables and time fixed effects (e.g., Böhmelt et al. 2016).⁸

Dealing with the third process—what might sometimes be addressed under the label of homophily, a selection effect, or reverse causality—can often be more tricky, especially when the states involved have some agency in choosing the extent to which they interact with others. In the case of diffusion within a trade network, the challenge is to show that the existence of a trading relationship leads states to adopt similar policies, rather than the other way round (meaning that states with similar policies are more likely to have established close trading relationships in the first place). Diffusion researchers have, for the most part, tried to exclude this alternative explanation using some combination of temporal lags, instrumental variables, matching techniques, attempts to model the formation of the network, or, more straightforwardly, the use of theoretical arguments and/or case studies to argue that the outcome of interest to the study is unlikely to be a driver of states’ proximity to each other within the network. (The latter is especially pertinent in the case of diffusion through relatively stable networks representing geographical or cultural ties.) Other approaches include techniques to endogenize the *W* matrix—for instance, through the use of the “mSTAR” model developed by Hays, Kachi, and Franzese Jr (2010).⁹

RESEARCH FRONTIERS

We will now consider some of the new directions in which research on diffusion is developing. These include studies of the conditions under which diffusion may (or may not) occur; new and creative ways of conceptualizing connections between political units; meta-analyses of diffusion processes; and employing mixed-methods approaches to the study of diffusion.

Conditional Diffusion

Most of the recent work on diffusion has moved beyond establishing evidence of diffusion and is instead asking questions about the *conditions* under which diffusion is more likely to occur. Answering this question is not only useful for understanding the scope of diffusion, but also for gaining more insight into the specific diffusion processes involved.

For instance, in a study of the diffusion of policies concerning unemployment benefits among a sample of OECD countries, Gilardi (2010) finds that the lessons governments learn from the experiences of others vary as a function of the political ideology of the ruling party or coalition. The results of the study suggest that when right-wing governments are in power, they are likely to copy other countries' decisions to cut unemployment benefits regardless of the impact that their decisions had on the rate of unemployment. However, when left-wing governments are in power, they are likely to copy other countries' decisions to cut unemployment benefits only when these decisions led to a fall in the unemployment rate. The results also suggest that left- and right-wing governments differ in terms of their sensitivity to the electoral consequences that result from other countries' decisions to cut unemployment benefits; it appears that right-wing, but not left-wing, governments are more likely to imitate benefit-cutting policies if these efforts had resulted in an electoral success. In other words, the diffusion effect is a conditional one: for diffusion to operate, it depends upon a successful interaction between the external signal (in this case, the policies adopted by other states) and the characteristics of the state that "receives" that signal.

The study of the conditions under which diffusion occurs can be extended into other areas of the diffusion process. In addition to asking questions about the characteristics of states that make them more or less receptive to outside signals, we can also ask questions about why some states are more likely to serve as "influencers" than others, or whether different aspects of the proposed diffusion channel (e.g., certain subsets of IGOs, or trade in certain types of goods) make the network more or less conducive to diffusion.¹⁰

Another interesting way to think about the conditional nature of diffusion is to consider how the impact of diffusion processes varies over time. In their study of the diffusion of bilateral investment treaties (BITs), Jandhyala, Henisz, and Mansfield (2011) use a piecewise event history model to argue that the adoption of BITs proceeded in three distinct phases. In the first phase, the adoption of BITs was driven mainly by the need to make credible commitments in situations where investing countries had reasons to fear for the security of their investments in the host countries. In the second phase, adoption was influenced to a greater extent by the desire to emulate the practices of relevant peers, such as states that share a common language. In the third phase, by which time concerns about the possible costs of BITs to the host countries were becoming more apparent, countries' adoption decisions were influenced to a lesser extent by those of their peers.

New W Matrices and New Units of Analysis

Most statistical analyses of diffusion involve the use of data that indicate the extent to which each unit is connected to every other unit in the system (see the discussion of W matrices in the section "How to Study Diffusion"). Usually the W matrices are constructed using

widely-available data on relations among states such as their degree of geographical proximity, their volumes of trade with one another, or their status as fellow members of international institutions. However, some recent studies have adopted more creative approaches to the construction of the W matrix. For instance, in their study of the diffusion of capital account liberalization in Latin America, Brooks and Kurtz (2012) hypothesize that states that have pursued the more advanced forms of import-substituting industrialization (ISI) are more likely to be influenced by the policy choices of others that have embarked on a similar path. In order to evaluate this claim, they construct a novel W matrix where each element is coded as either 0 or 1 to indicate whether or not the pair of countries share an experience of advanced ISI (Brooks and Kurtz 2012, 114). The resulting diffusion variable therefore captures the average extent of capital account liberalization found among other states with similar developmental histories. By using a more nuanced model of the specific ways in which states can learn from the examples set by others, their approach allows for a deeper understanding of the underlying diffusion process.

Using a similar strategy, Böhmelt et al. (2016) test claims about the extent to which political parties' positions on a left-right political spectrum are influenced by the successes or failures of their foreign counterparts. To do so, they use cross-national data at the party level to construct a series of W matrices that indicate which foreign parties have recently been in power, and which ones share an ideological orientation with others. Their results suggest that, after controlling for various domestic influences on party positions, political parties are indeed influenced by the positions adopted by the examples set by parties in other countries.

By addressing diffusion among political parties, Böhmelt et al. (2016) illustrate another interesting area in which diffusion research has advanced: the study of diffusion need not necessarily be confined to the country-year unit of analysis. Not only can diffusion studies address patterns of diffusion at sub-state levels of analysis (see, e.g., [Shipan and Volden 2008] on the diffusion of anti-smoking policies among cities and states within the US, or Füglistner (2012) on the diffusion of health policies among Swiss cantons), but they can also consider diffusion among very different types of actors. For instance, Ovodenko and Keohane (2012) and Paterson et al. (2014) consider the diffusion of environmental governance arrangements across actors operating at very different scales, ranging from sub-state actors to intergovernmental organizations. Indeed, Paterson et al. (2014) draw from Elinor Ostrom's concept of "polycentric governance"¹¹ in coining the term "polycentric diffusion" to describe this type of process. In cases like these, the vectors for transmission among the different types of jurisdiction would appear to be the individual experts that engage with policymakers at multiple levels of governance.

Meta-Analyses of Diffusion

Rather than focusing on a single instance of diffusion—for example, the diffusion of one specific policy—a number of recent studies of diffusion in the US context have attempted to simultaneously analyze the diffusion of multiple policies. In doing so, the researchers are able to test hypotheses about how attributes of various policies affect the ease with which they diffuse. For instance, Nicholson-Crotty (2009) conducted a meta-analysis of 57 policies that have diffused among US states. In this case, the unit of analysis is the policy itself. This

study was able to show that policies that are more salient (in terms of the media attention they generate) diffuse more rapidly than those that play a smaller role in public discussions.

Using a similar approach, Makse and Volden (2011) pooled together data on the diffusion of 27 criminal justice policies among the 50 US states in order to create a dataset where the unit of analysis is the state-policy-year. This allowed them to test hypotheses about diffusion processes, attributes of the states involved, as well as attributes of the policies themselves within a single model. This type of large-*n* comparative approach to policy diffusion would seem to lend itself well to the study of policy diffusion in the international arena, although as far as I am aware this has not yet been applied to the fields of international relations or comparative politics.¹²

Another intriguing possibility is to use diffusion data from multiple cases of diffusion in an attempt to infer more general pathways of diffusion. In other words, rather than assessing the extent to which a *W* matrix that has been defined on the basis of some pre-defined form of connectivity among states (e.g., geographical proximity) can account for observed patterns of diffusion, one can instead use the pattern of diffusion to infer the *W* matrix. This is the approach pioneered by Desmarais, Harden, and Boehmke (2015) in the context of US policies, but which, as far as I am aware, has yet to be applied in the international realm.

More research comparing the ease with which different policies can diffuse within the international system would also help to address a very real possibility of selection bias in diffusion research—meaning that studies of successful diffusion tend to be published, while those that do not end up in the “file drawer.” This causes us to overlook the equally important question of what policies or events do *not* diffuse through the international system (Solingen 2012). As Karch et al. (2016) demonstrate in their study of the diffusion of interstate compacts in the US, this selection bias can not only lead us to systematically overestimate the ability of policies and practices to diffuse, but it can also lead to faulty inferences about the underlying processes involved.

Incorporating Other Forms of Evidence

Most of the recent literature on diffusion has involved statistical analyses of the patterns of adoption of specific policies or practices. But qualitative approaches provide an essential complement to the statistical study of diffusion.¹³ Of these, process-tracing is perhaps the most important. It allows the researcher to gain valuable insights into the diffusion processes itself, or to assess the plausibility of an inference drawn from a large-*n* analysis. For instance, Kollman (2007) used interviews with representatives of governments and INGOs to show how transnational advocacy organizations helped to educate policymakers about other countries’ success with laws recognizing same-sex unions. Similarly, in a study of the adoption of quotas for women’s representation in national legislatures, Towns (2012) showed how activists in developing countries managed to successfully push for quota laws by appealing to policymakers’ concerns about their own country’s standing vis-à-vis the more developed countries in the international system.

Another complementary approach is to use survey data to gain insight into the diffusion process. Massey et al. (2014) conducted a survey of European elites working on climate policy in order to assess the extent to which various factors motivate policymakers to adopt

climate adaptation measures. Interestingly, their study revealed some important limitations to the extent to which policymakers tend to be influenced by the examples set by their foreign counterparts. Rather than giving policy diffusion a central role, it appears that a country's past experience with extreme weather events, as well as exposure to programs established by IGOs, turn out to have stronger overall influence in this particular context.

Finally, experimental approaches represent a promising method for testing hypothesis about the transmissibility of ideas. Although the discipline of political science has embraced experimental approaches in recent years, the field of diffusion research has relied almost entirely on observational studies. A notable exception is Butler et al. (2017)'s experimental study of sources of influence among policymakers in the US. They used a survey-based experiment to test hypotheses about the conditions under which policymakers are willing to learn from the examples set by other jurisdictions. Their study involved asking a sample of municipal officials to read vignettes describing another jurisdiction's experience with a particular policy choice. The experimental manipulations involved making adjustments to the way in which the success of the policy and the ideological orientation of the local government were described in the vignette. Their findings echo those of Gilardi (2010) insofar as policymakers' willingness to learn from others is highly sensitive to the ideological orientation, although Butler et al. (2017) found that compelling evidence of success with the policy can overcome these barriers to diffusion. While their study dealt with US domestic politics, it would be very interesting to see IPE scholars extend this approach to the study of diffusion at the international level.

CONCLUSION

In the Introduction to this volume, Jon Pevehouse and Leonard Seabrooke argued that the field of IPE has, for the most part, adopted a "problem-centered" approach to its research agenda. In other words, IPE scholarship has tended to focus on the more pragmatic task of explaining puzzling outcomes found in the real world, rather than on selecting research questions with a view to advancing particular sets of theoretical claims.

The study of diffusion processes would appear to fit this model quite well. Most studies of diffusion are motivated by an attempt to explain a curious pattern of outcomes—usually by posing a question along the lines of "Why did some countries adopt this policy more quickly than others?" or "Why did this particular policy diffuse relatively quickly through the international system, but this other one did not?" By taking this more puzzle-driven approach, scholars of diffusion have not been tied to one particular theoretical perspective. Statistical models of diffusion have been used successfully by scholars operating within both a rationalist framework—for example, when examining the extent to which states compete with one another for access to investment—and a more sociological one—for example, when studying patterns of norm diffusion within the international system. Indeed, one of the advantages of the spatial modeling framework described in this article is that it can be applied in a way that pits the predictions of these different theoretical approaches against one another—for example, by using different types of W matrices to ask whether the adoption of a policy can better be explained by material or normative pressures (see, e.g., Simmons and Elkins 2004; Cao 2010; Jandhyala, Henisz, and Mansfield 2011; Greenhill 2015).

It is also worth pointing out that while most diffusion research has involved the use of statistical models, it need not be tied to a restrictive set of assumptions about *who* the key actors in the international political economy are. As discussed earlier, while much of the research has focused on diffusion among states, there has been some interesting work applying diffusion models at the level of sub-state units, international organizations, or individual people (see, e.g., Füglister 2012; Paterson et al. 2014; Butler et al. 2017). And while most of the recent work on diffusion has tended to adopt quantitative approaches, it seems that this research program would benefit from more work that attempts to integrate qualitative evidence—especially in the form of process-tracing exercises that can help to shed light on the underlying processes of diffusion.¹⁴

NOTES

1. Maggetti and Gilardi (2016) point out that in this respect the concept of diffusion differs in an important way from the concept of convergence. Although the terms are sometimes used interchangeably, convergence refers to an outcome that may or may not have arisen from a true diffusion process. They cite the example of a group of people opening up umbrellas when it starts to rain; in this case, we certainly observe convergence among the actors' behavior, but this outcome can usually be better explained by a common external shock (the rainstorm) than by a process of diffusion in which people open their umbrellas in response to other people's decision to do so (Maggetti and Gilardi 2016, 90).
2. See the "space is more than geography" argument advanced by Beck, Gleditsch, and Beardsley (2006).
3. A slight variant of this process is what some scholars refer to as *persuasion*. Rather than waiting for states to learn of the benefits of a policy by themselves, persuasion involves a state (or international organization) actively trying to teach the benefits to others.
4. More recently, Cranmer, Desmarais, and Campbell (2020) used a network approach to examine diffusion of democracy through both IGO and alliance networks. They find that participation in alliances, rather than IGOs more generally, has an important effect on the diffusion of democracy.
5. For more on the diffusion of norms or practices that are inconsistent with liberal values, see Greenhill (2015) and Hafner-Burton and Schneider (2019).
6. For an introduction to conducting spatial regression—and, by extension, other forms of diffusion analysis—see Ward and Gleditsch (2008) and Darmofal (2015).
7. The difficulty in distinguishing between a pattern of events that occur independently of each other from those that are truly interdependent is often referred to as "Galton's problem" (see Braun and Gilardi 2006).
8. For a discussion of the dangers of incorrectly inferring a causal impact of membership in international institutions, see Chaudoin, Hays, and Hicks (2018).
9. Bove and Böhmelt (2016)'s study of the relationship between migration and terrorism provides a nice illustration of how this can be used to mitigate concerns about endogeneity in a diffusion analysis.
10. See, e.g., Greenhill (2015 Ch. 4). For a discussion of empirical strategies for testing hypotheses about the conditions under which diffusion is likely to occur, see Neumayer and Plümper (2012).
11. See Ostrom (2010).

12. See Graham, Shipan, and Volden (2013) for a discussion of the gap between scholars of American politics and IR/CP in their approaches to diffusion research.
13. For a more detailed discussion of qualitative approaches to diffusion research, see Starke (2013).
14. See Starke (2013) for a detailed discussion of qualitative and mixed-methods approaches to the study of diffusion.

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CHAPTER 16

ECONOMIC COERCION

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INTRODUCTION

IN an international dispute, each state seeks to secure the best possible outcome for itself, while paying the lowest cost in the process. Often, this desire results in a disputant's use of economic coercion against the opponent. Economic coercion is a threatened or actual imposition of economic costs on one state by another with the objective of extracting a policy concession. Such a concession can require the coerced party to implement a policy that the coercer favors, or abandon a policy that the coercer opposes. In either case, the coercer's action lowers the target's expected utility from pursuing the policy path the coercer wishes to avoid, which makes it more likely that the target will act in ways consistent with the coercer's preferences.

Economic coercion then hinges on a state's ability to generate costs for its opponent. Scholars identify conditions when this is feasible, and analyze how coercers can maximize such costs. The state of complex interdependence that exists in modern economic relations extends to a large number of areas, from trade and investment to economic agreements and development aid. These diverse forms of economic interdependence and especially asymmetries in economic relations create leverage that can be used against the more dependent state in coercive bargaining.

Economic costs that underpin coercion do not necessarily lead to political costs for the leadership of the targeted country. In fact, coercion can generate political benefits through rally-around-the-flag effects. Therefore, coercive efforts can fail not only because economic costs were not sufficiently high to make concessions a more preferred course of action, but also because economic coercion resulted in political benefits and, hence, strengthened the target's bargaining position.

Throughout the chapter, the terms "economic coercion" and "economic sanctions" are used interchangeably, following the established convention in the field.¹ Baldwin (1985) points out some definitional differences between the terms, and distinguishes them from "economic statecraft." However, the differences between "economic coercion" and "economic sanctions" are sufficiently minor to be safely ignored. For instance, unlike "economic coercion," the term "economic sanctions" can be used in some contexts as a narrow legalistic

concept linked to enforcement of international law (Baldwin 1985, 35). At the same time, I will avoid referring to “economic statecraft” because it includes a broader range of economic instruments than economic coercion: whereas the latter covers only negative economic measures, such as restricted trade or foreign aid, the former also encompasses positive economic measures, such as lower tariffs or investment guarantees.

This chapter provides an overview of key contributions to the study of economic coercion. I begin by reviewing the research on states’ motivations for using economic sanctions and highlighting instrumental and symbolic explanations offered by sanctions scholars. Once a state chooses to coerce its opponent, the next critical choice concerns the type of coercive instrument and the scale of coercive effort. After a discussion of sanction design, I examine the literature on effectiveness of economic coercion. The main conclusions that emerge from this literature relate to the design and enforcement of sanctions: both play a vital role in determining whether the sender achieves its goal. In addition to assessing the coercer’s ability to secure concessions, scholars have evaluated other sanctions outcomes. These outcomes are unintended (i.e., different from the sender’s main objective), although they can increase the sender’s odds of achieving desired concessions, and frequently indirect because economic costs associated with coercion generate side effects, even when senders design targeted sanctions. Finally, I conclude by addressing the issue of “constructive non-engagement.” Scholarship on economic coercion does not experience this issue: the shared focus on the problem of economic coercion results in a significant degree of engagement and collaboration among sanction scholars.

WHY DO COUNTRIES USE ECONOMIC COERCION?

Economic coercion presents a puzzle that is similar to the rationalist puzzle of war (Fearon 1995): *ex post*, both economic and militarized forms of coercion are inefficient. They generate costs for at least one of the involved parties, and typically for both. The size of such costs is usually greater for militarized conflict, although existing studies suggest that in some cases sanctions can impose economic damage and human costs comparable to losses attributed to wars (Allen and Lektzian 2013; Mueller and Mueller 1999). Given that both sides in a dispute are aware of such costs, they should try to avoid costly conflict by settling their dispute before coercion can be used. Yet, there may be something preventing the two sides from reaching such a settlement *ex ante*.

While Fearon’s main conclusion is that war is a result of private information or commitment problems, sanctions studies have only considered the first mechanism, and entirely overlooked the second. If states in a dispute cannot reach an agreement due to private information about intentions, resolve, or capabilities, the states may have no choice but resort to inefficient economic conflict to send a credible signal revealing this private information (Ang and Peksen 2007; Drezner 1999; Lektzian and Sprecher 2007; Morgan and Schwebach 1997). However, for a signal to be credible, it has to be costly, and subsequent research suggests that, on average, sanctions are not sufficiently costly for the sender to be informative (Whang and Kim 2015). Similarly, sanction threats reveal no new information to the sender’s opponents, and hence have no signaling value (Whang et al. 2013). Therefore,

due to the lack of evidence in support of the signaling mechanism, sanctions scholarship has concluded that states do not use sanctions to reveal private information (Morgan 2015).²

The commitment mechanism requires the presence of preventive motivations. Specifically, a target of sanctions should be unable to make a credible commitment to comply with the terms of the present agreement in the future due to shifting dyadic capabilities. In this case, the sender imposes sanctions to prevent the shift that would create incentives for the target to renege in the future. For instance, a major trading partner of a rapidly growing state may recognize that a settlement of a bilateral dispute (e.g., a dispute over human rights violations) may not be sustainable in the long run if the gap in economic capabilities between the two states is rapidly closing. Hence, the major trading partner may prefer to impose sanctions to slow down the rapid economic growth of its opponent, thereby decreasing the probability that the target would be tempted to abandon the agreement later, when it reaches and surpasses the sender's economic capabilities. Fearon (1995) points out that states often bargain over objects that can themselves contribute to states' capabilities. Negotiations over a country's nuclear program illustrate this scenario. Even though both the country pursuing the nuclear program and its opponents may prefer to reach a settlement rather than engage in economic conflict, the nuclear program itself is a source of bargaining leverage. Thus, the country may prefer to be subjected to sanctions rather than close the program because the country does not trust its opponents not to take advantage of its weakened bargaining leverage in the future. Research in this area is required to assess whether there is any evidence that commitment problems can account for the use of economic coercion.

Given the absence of evidence for signaling or commitment mechanisms, many scholars have implicitly or explicitly concluded that economic coercion may not be inefficient *ex post*, after all. In other words, we may observe economic coercion because the benefits that the target receives from pursuing its chosen course of action exceed economic and other costs imposed by sanctions (Smith 1995). Otherwise, the target would concede immediately after sanction initiation or even at the threat stage. The sender also needs to care about the target's behavior more than about costs that the sender experiences as a result of sanctions, and the sender may receive significant political benefits for demonstrating its opposition to the target's behavior. If sanction costs imposed by the sender limit the target's resources and force policy adjustments, then economic coercion has instrumental value.

Existing research offers some estimates of sanction costs for target and sender countries. Specifically, overall economic losses experienced by target countries tend to be modest. According to Hufbauer et al. (2007), on average, target economies experience a 2.9 percent decline in their gross national product (GNP) under comprehensive sanctions, combining trade and financial restrictions, when Iraq sanctions are excluded from the sample. In some cases, however, economic damage can be substantial: for Iraq, the United Nations (UN) embargo in the 1990s resulted in the loss of approximately half of the country's annual GNP (Hufbauer et al. 2007, 105). When Iraq is included in sanction cost calculations, the estimate of overall economic losses goes up to 4.2 percent of GNP (Hufbauer et al. 2007, 198). Similarly, Neuenkirch and Neumeier (2015) indicate that annual real per capita gross domestic product (GDP) growth rate decreases by 0.9 percent under US sanctions, and by 2.02 percent under UN sanctions. In contrast, aggregate economic costs for senders, which tend to have larger economies, are typically inconsequential (Hufbauer et al. 2007, 109). When senders anticipate that sanctions could generate noticeable economic costs, they can choose to abstain from economic coercion (von Soest and Wahman 2015b).

Political Costs and Rally Effect

Politically, economic coercion has a mixed impact on target governments. Allen (2008) finds that sanctions are politically costly for democratic governments where anti-government protest is less risky for dissatisfied citizens, whereas autocratic governments appear to benefit from sanctions because opposition can be curbed more effectively, and leadership can use its control over economic resources to appease supporters. Moreover, authoritarian leaders use sanctions as an opportunity to consolidate their hold on power (Peksen and Drury 2010). Personalist autocracies may be an exception to this finding: these regimes tend to depend on external sources of revenue more than other autocratic regimes, and at the same time have lower institutional capacity to withstand economic costs imposed by sanctions (Escribà-Folch and Wright 2010, 2015; Peksen 2019). Similarly, Marinov (2005) shows that economic coercion increases government instability, measured as leadership turnover, more dramatically in democracies than in autocracies.

At the same time, the result linking democratic institutions in a target country with greater political costs has come under scrutiny, as scholars seek to assess whether political leaders may be able to generate a rally in support of their policies that led to economic coercion. The rally effect was initially described in the context of military conflict (Mueller 1973), but sanctions scholars quickly recognized that economic coercion could produce a similar rally effect (Kaempfer and Lowenberg 1988). Such rallies can occur during international crises when citizens experience greater patriotism and feel less critical toward their country's leadership (Schultz 2001). Under these circumstances, citizens should be unlikely to protest against their government and seek to overthrow it. On the contrary, they should give more support to the government and unite in opposition to the sender country. Grossman et al. (2018) provide experimental evidence of this effect. Their survey experiment shows that sanctions boost popular support for the policy that resulted in sanctions, as well as hostility toward the country imposing sanctions. The study identifies a moderate decrease in public support for the incumbent, but only because survey participants preferred a more hawkish position in the dispute with the coercer.

The result reported in Grossman et al. (2018) may be limited due to its focus on one target country (Israel), which is a mature democracy; at the same time, it is consistent with findings from a study conducted in a less stable democratic target country (Ukraine). Seitz and Zazzaro (2020) analyze survey data of Ukrainian households and provide evidence of a rally effect during a dispute with Russia. When the sender country applied economic coercion against Ukraine by restricting gas exports, individuals with a greater exposure to rising gas prices expressed more support for the Ukrainian government's policy of cooperation with the European Union (EU) and a more negative opinion regarding economic integration with Russia.

While the rally effect hypothesis receives support in settings with democratic accountability mechanisms, other studies find a similar pattern in the context of Russia, a less democratic target (Alexseev and Hale 2020; Frye 2019). Public support for the country's leadership increases when survey participants recall the policy that caused the imposition of sanctions (i.e., the Crimea annexation). On the flip side, individuals do not feel more supportive of the government due to sanctions, and those individuals who were more skeptical of Russia's president and experienced greater economic adversity tended to offer lower approval of the

government, contrary to the rally hypothesis. This research suggests that public approval of the target government and public support for the government's policies may diverge, generating a complex mix of political costs and benefits for target leadership. In addition, authoritarian regimes supported by strong claims to legitimacy and without significant links to the sender are in a better position to turn sanctions into a boost for their legitimacy and, hence, experience a rally-around-the-flag effect (Grauvogel and von Soest 2014). Further theoretical and empirical work should unpack the impact of economic coercion on target leaders' political calculus and, consequently, their willingness to resist sanctions.

Scholars have also revisited the conclusion that autocratic regimes remain robust in the face of economic coercion and provided evidence of increased political instability in autocratic countries targeted by sanctions. Grauvogel et al. (2017) argue that sanctions can communicate to target countries' domestic audiences that sender countries disapprove of target governments and their policies, and hence have a positive view of domestic protest in target countries. Empirical analyses offer support for this argument: domestic protests intensify when sender countries issue sanction threats. Citizens of countries with electoral authoritarian regimes take advantage of their right to vote, and deliver significant losses to incumbent governments by voting against their candidates. While democracies also experience some voter backlash in elections, sanctions are more costly for autocratic governments (Park 2019). Escribà-Folch and Wright (2015) evaluate the relationship between sanctions and political stability of autocracies and find that economic coercion does destabilize these regimes; however, failed autocracies are replaced by new autocratic regimes, rather than democracies.

Political Costs and Benefits for the Coercer

When it comes to sender countries, regardless of their ability to extract concessions, an exercise of economic power appears to provide political benefits to sender leadership. Sanctions scholars argue that governments have incentives to impose sanctions in international disputes because that generates domestic political benefits (Kaempfer and Lowenberg 1992; Lindsay 1986). For instance, US presidential approval ratings increase following sanction imposition (Whang 2011). Moreover, domestic support does not appear to depend much on success of economic coercion. Heinrich et al. (2016) show that sender country voters' support for sanctions increases modestly when the voters expect the target to offer immediate concessions. McLean and Roblyer (2016) also report that voters are more likely to support sanction imposition when they anticipate a more successful outcome. However, voters' expectations of sanctions effectiveness are consistently lower than expert-provided estimates of success, indicating a skeptical attitude among general public regarding sanctions' instrumental value. Hence, concerns regarding sanctions effectiveness may not serve as a significant constraint on sender governments' ability to receive symbolic benefits from sanctions imposition. Moreover, governments may be incentivized to resort to economic coercion whenever they need to increase their approval among domestic audiences, or show their active approach to addressing an international dispute.

Any political costs that the sender may experience as a result of sanctions appear to stem from foreign relations. Specifically, potential senders are reluctant to undermine strategically important partners by damaging their economies and weakening them politically. Nielsen (2014) and von Soest and Wahman (2015a) indicate that senders prefer not to sanction

friendly authoritarian governments that support the senders' international agenda by voting with the sender countries in the United Nations General Assembly. McLean and Radtke (2018) identify a more nuanced relationship. They argue that the removal of a friendly regime is costly, while the replacement of a hostile government is a benefit. Sanctions are more likely when they are most beneficial for reaping the political benefit or contribute the least to the political cost, which means that senders prefer to coerce friendly regimes when they are stable, and adversarial regimes when their stability is already low.

HOW DO COUNTRIES DESIGN COERCIVE POLICIES?

Hufbauer et al. (1985) describe main types of economic coercion as “withdrawal, or threat of withdrawal, of customary trade or financial relations.” Similarly, the manual for the Threat and Imposition of Economic Sanctions (TIES) dataset adopts a broad view on coercive instruments, which states apply to “limit or end their economic relations” to impose costs on their opponents. Given a wide range of economic exchanges that states engage in, specific coercive instruments are also diverse, and include restrictions on flows of goods, services, people, and capital between two or more states. Individual instruments can be used individually or in combination, but they can also be scaled up or down, depending on the preferred level of coercive effect. For instance, imports from the target to the sender can experience minor restrictions, or a complete ban—or the sender can choose some middle ground, potentially leaving room for escalation in the future.

An important development in sanction design occurred during the 1990s, as policymakers and political scientists converged on the pessimistic conclusion that sanctions do not work very well in extracting concessions, but they can lower the quality of life for people in targeted countries. Sanctions against Iraq illustrated these conclusions vividly: the country was a target of harsh sanctions that cut its GNP in half but did not yield any concessions from Saddam Hussein's regime. At the same time, the sanctions resulted in high levels of humanitarian suffering among Iraq's population, especially among its most vulnerable groups, such as women and children. According to Garfield (1999), between 100,000 and 227,000 deaths among young children during the period from August 1991 to March 1998 can be attributed to economic sanctions. Gordon (2010) provides an even higher estimate of deaths among children of the age five years old or younger—over 500,000. Just as sanctions against Iraq were failing to coerce the government and harming the population, political scientists began providing evidence that sanctions did not work more generally. The dataset compiled by Hufbauer et al. (1990) put forth the estimate of sanction success of 34 percent.³ Pape (1997) criticized this estimate as overly optimistic and provided his own estimate—5 percent.

Subsequently, senders began tailoring, or targeting, their sanctions. Such targeted sanctions seek to limit economic costs to some individuals, groups, or economic sectors within the target state, while leaving the rest of the country and its economy unaffected. Even though such targeting may ameliorate the problem of collateral damage to some extent, effectiveness of targeted sanctions tends to be lower than that of comprehensive sanctions, especially when the target government views the disputed issue as highly salient (Ang and Peksen 2007; Cortright and Lopez 2002a, 2002b; Elliott 2002). Moreover, targeted sanctions may generate unintended spillovers, which deliver indirect economic, social, and political

effects to a broader population (Biersteker et al. 2016). Therefore, targeted sanctions appear to sacrifice effectiveness without offering a satisfactory solution to the problem of collateral damage (Early and Schulzke 2019).

Senders may tailor the coercive impact of sanctions not only to spare the target country's population, but also to minimize their own costs. While senders typically experience low economic costs in aggregate terms, individual firms and economic sectors may suffer significant losses. Hence, to shield established economic relations, the sender government can turn to sanctions instruments with a more restricted impact on bilateral economic exchanges, such as aid sanctions (Lektzian and Souva 2003; McLean and Whang 2014). Moreover, in some cases sanctions can generate substantial benefits to an important domestic economic constituency, such as banks and financial institutions (Hakelberg 2016).

Multilateral Approach to Coercion

The coercive impact of economic sanctions can be increased through coalition building. Typically, one sender country can generate a limited impact on its opponent's economy. An important exception is a target country's largest trading partners, which can impose immediate and substantial costs, thereby forcing the country to concede quickly (McLean and Whang 2010). In the absence of such a sizable economic leverage, bilateral sanctions tend to deliver only mild shocks to targeted economies because "black knights" take advantage of the opportunity to increase their economic exchanges with the target and, hence, weaken the impact of sanctions through sanction-busting (Early 2011; Hufbauer et al. 1990). Target states can also engage in illicit trade to obtain goods and services that are no longer available through regular trade channels.

To mitigate the problem of sanction-busting, senders can organize multilateral coalitions. The more countries join such a coalition, the harder it becomes for the target to find alternative markets and receive financial support (Ang and Peksen 2007; Early 2015). The resulting increase in sanction costs makes economic coercion more successful in extracting concessions from the target. However, multilateral coalitions can be significantly weakened by the incentive to free-ride: members of the coalition prefer to let others pay the costs of disrupted economic exchanges, sanction monitoring, and compliance enforcement, while reaping benefits from maintaining or even strengthening their relationships with the target. International institutions can counter this incentive and promote cooperation among senders by monitoring and reporting on compliance with sanctions, addressing bargaining and enforcement issues, and providing mechanisms for punishing sanction-busting behavior among coalition members (Bapat and Morgan 2009; Drezner 2000; Drury 1998; Early 2015, 2011; Early and Spice 2015; Martin 1992).

WHEN DOES ECONOMIC COERCION SUCCEED?

The early wave of quantitative studies of sanctions effectiveness concluded that economic coercion failed to achieve its stated objectives most of the time. Success estimates ranged from moderately pessimistic—34 percent success rate provided in Hufbauer et al. (1990)—to

extremely pessimistic—just 5 percent, according to Pape (1997). Yet, the consensus soon came under scrutiny in a new set of sanctions studies, which pointed out that success of economic coercion depends on state interactions prior to its initiation. Hence, analysts need to account for selection effects at the threat stage (Lacy and Niou 2004; Nooruddin 2002; Smith 1995). If a sender is capable of imposing significant costs on a target, coercion may succeed at the threat stage. The target may calculate that benefits from resistance are lower than sanction costs, and hence acquiescence to the threat is a better choice (Drezner 2003). Given this, we may only observe sanctions when targets are strongly committed to their disputed policies and anticipate sufficiently low economic costs, or have options for mitigating or avoiding such costs altogether. The low rate of success in these challenging cases does not mean that economic coercion is generally ineffective.

This important development in sanctions research led to a re-assessment of sanctions effectiveness. Morgan et al. (2014) calculated that up to 56 percent of sanctions episodes in their dataset end favorably for the sender if negotiated settlements are coded as successes and the sample is limited to completed episodes. In a study of nonproliferation sanctions, Miller (2014) finds that, despite the low success rate of observed sanctions (14.3 percent), coercion was in fact remarkably successful in deterring nuclear proliferation. Economic dependence on the US and, hence, vulnerability to US sanctions drastically reduce a state's willingness to pursue a nuclear weapons program in the period when the US began implementing nonproliferation sanctions.

Factors of Success

Subsequent research provides further evidence of selection effects and explores determinants of success at different stages of economic coercion. In their sensitivity analyses, Bapat et al. (2013) identify a set of sanction characteristics that are associated with sanctions success. Specifically, indicators for financial sanctions and multiple disputed issues have a positive effect on success rates of imposed sanctions, but the opposite effect on threatened sanctions. This indicates that the sender-target interaction changes as the dispute progresses from threats to actual coercion. The study also evaluates a broad range of results from previous research to test their robustness in a comprehensive setting. One key conclusion reinforces the notion that economic coercion critically depends on the sender's ability to impose substantial costs on its opponent: variables gauging this ability (i.e., institutional support for sanctions, and target costs) have a robust positive relationship with target concessions at both stages of sanctions.

Other results from the sensitivity models provide further evidence of the link between sanctions design and their success, and largely correspond to conclusions reported in other studies of sanctions effectiveness. When a sender initiates sanctions over a highly salient issue, such as a matter of national security, the target is more likely to concede, as Ang and Peksen (2007) argue. Bapat et al. (2013) refine this result to show that, if an issue is highly salient, the target prefers to offer concessions before sanctions imposition. Also, coercive efforts associated with complex disputes, which involve multiple issues, increase the likelihood of success at the imposition stage, according to Bapat et al. (2013). This result departs from a previous study showing that, unless they have institutional backing, multi-issue sanctions are more prone to failure (Bapat and Morgan 2009). At the same time, financial

sanctions succeed at higher rates than other types of sanctions because they impose more severe and immediate costs (Bapat et al. 2013; Dashti-Gibson et al. 1997; Early 2015; Hufbauer et al. 1990; Rosenberg et al. 2016). Despite their short-term effectiveness, financial sanctions may incentivize target governments to adjust by de-dollarizing their economies to reduce vulnerability to financial restrictions in the long run, thereby undermining senders' ability to use these coercive measures in the future (McDowell 2020).

At the threat stage, Bapat et al. (2013) identify additional determinants of sanctions effectiveness, although the results are less robust. When the US is the sender, and positive incentives accompany coercive measures, targets are less likely to resist sanction threats. In contrast, export restrictions increase resistance, as do democratic institutions in the target country. The positive relationship between democracy and the target's resistance raises an important question regarding the influence of democratic institutions on a country's willingness to resist economic coercion. While some previous studies suggest that democracies should be quick to acquiesce, this result shows the opposite: democracies are more resilient targets. In fact, a recent study provides further support for this conclusion: Kavakli et al. (2020) show that threats of economic coercion are less successful when issued against democracies, compared to autocracies, and imposed sanctions are no more or less effective when imposed against democracies. Therefore, high political costs borne by governments of democratic targets do not appear to translate into a higher likelihood of concessions. While further research is necessary to explain this intriguing puzzle, one study suggests that scholars may need to shift their attention from democratic institutions, broadly defined, to institutions aimed at constraining the executive power. When a political system imposes significant political constraints on the executive branch, economic coercion aimed at such a country tends to be more successful because the target government is more limited in developing and implementing policy responses to counter coercive measures (Jeong and Peksen 2019).

In addition to sanction design, presanction bilateral relations between target and sender countries serve as significant determinants of the sender's bargaining leverage and, consequently, the likelihood of extracting desired concessions. Economic exchanges are particularly important, especially when such exchanges create a highly asymmetric relationship that can make the more dependent party vulnerable to economic coercion in the future. The target's trade dependence on the sender significantly increases sanctions effectiveness, especially when the sender is the target's top trading partner (Bapat et al. 2013; Hufbauer et al. 2007; McLean and Whang 2010). Akoto et al. (2020) suggest that the importance of target trade dependence declines with growing intra-industry trade, which in turn increases the target's ability to resist economic coercion. Consequently, concessions become less likely as the share of target-sender intra-industry trade exchanges goes up.

Sender-target financial links vary in their effects on sanctions success. On the one hand, significant aid flows make the recipient of these flows vulnerable to coercion, which in turn increases the country's willingness to acquiesce to its donor's demands (Early and Jadoon 2019). On the other hand, foreign direct investment (FDI) can reduce the sender's bargaining leverage. Kim (2013) argues that FDI through cross-border mergers and acquisitions makes sender companies more vulnerable to sanction-related costs and target retaliation, which heightens domestic opposition to the sender government's potential coercive action. As a result, the sender is less likely to resort to sanctions.

While multilateralism in sanction imposition has been extensively investigated in the literature, the use of multilateral institutions as an additional source of leverage remains

a mostly uncharted territory with one notable exception. Peksen and Woo (2018) suggest that a powerful sender country, such as the US, can use its influence within international organizations to amplify sanction costs. The study focuses on International Monetary Fund (IMF) assistance, and shows that sanctioned countries are less likely to receive IMF funds. Since the IMF is the lender of last resort for countries facing economic challenges, the US government's ability to restrict a target's access to IMF support provides this sender with a particularly damaging coercive instrument.

Powerful senders can also wield their substantial structural power in international financial and communication networks (Farrell and Newman 2019). The US in particular benefits from asymmetries within these networks, which allows the sender to obtain informational advantage during a dispute with another country or cut off the opponent's access to these vital international networks. Such power dramatically increases sanctions effectiveness. At the same time, less powerful countries that are targeted by such coercive measures may attempt to address their vulnerabilities by seeking out alternative arrangements. In the long run, these adjustments could erode the ability of the US and its Western allies to weaponize economic interdependence and, hence, reduce the effectiveness of their coercive measures against other countries.

Sanction Enforcement

Once sanctions are in place, the success of coercion depends on the sender's willingness and ability to enforce the sanctions. For instance, the sender government may be reluctant to enforce sanctions if it is concerned that such an action would undercut its firms' competitiveness, given that third-party firms can continue trading with the target. The government must also consider costs of monitoring firms' compliance and punishing sanction-busters. Therefore, for successful enforcement, sender firms need to account for a significant share of the target's market but not so large as to make evasion too tempting for sender firms and enforcement too costly for the sender government (Bapat and Kwon 2015).

In addition, illicit activities that help the target circumvent sanctions can make enforcement costs prohibitively high (Early 2015). Opting for more limited, tailored sanction instruments instead of broad, comprehensive coercion does not appear to reduce these costs (Tostensen and Bull 2002). The sender's ability to enforce sanctions is constrained by the target's outside options. If the target can identify third parties that are willing and able to replace the target's reduced or lost relationship with the sender, the sender's enforcement capacity declines and the target's expected utility from resistance increases significantly. Existing research points to third parties' economic capabilities and prior economic relationship with the target as measures of their ability to replace the sender, while ideological or alliance ties provide the reason to engage in sanction busting (Early 2015, 2012; Peksen and Peterson 2016). A more nuanced argument links the target's high value to its trading partners and the partners' connectedness to the international trade network, to the target's ability to resist sanctions successfully (Peterson 2020). More broadly, in the absence of a coordinated coercive effort, global markets quickly adjust to the sender's restrictions, providing the target with a path for circumventing sanctions (Gholz and Hughes 2019).

Targets may also turn to international credit markets to obtain financing necessary to weather economic sanctions. When the cost of such credits is not too high, that is, when the

target's credit rating is strong, economic coercion is less likely to succeed and, hence, senders avoid using sanctions altogether (Cilizoglu and Bapat 2020). Furthermore, a country's existing relationship with a regional or global power can insulate the country from sanctions (von Borzyskowski and Vabulas 2019).

The key mechanism for enhancing third-party cooperation is by securing support from an international institution, such as the UN or the EU (Drezner 2000). Yet, the sender may be unable to obtain such support. When it does, the primary sender still needs to ensure that other states comply with sanctions. Martin (1993) shows that rates of compliance improve when the primary sender uses linkage politics, tying compliance to credible threats of punishment or offers of side rewards. In some cases, the sender can resort to secondary sanctions to ensure that third-party states comply with its sanctions against the target (Han 2018). Another factor shaping compliance rates is the size of a sanctioning coalition. Not all institutions are equally effective in enhancing the sender's coercive efforts. Early and Spice (2015) argue that smaller international institutions, such as the Arab League, tend to be more successful than larger ones, such as the UN. More restrictive membership allows countries to achieve deeper cooperation, while minimizing cheating due to less costly monitoring and enforcement in smaller groups. Consequently, sanctions backed by larger institutions allow more sanction busting to go undetected and unpunished, thereby weakening enforcement quality.

WHAT ARE UNINTENDED AND INDIRECT OUTCOMES OF COERCION?

The dominant approach to evaluating outcomes of economic coercion traditionally centered on the sender's ability to achieve stated goals. Yet, the recognition of the humanitarian toll of sanctions, especially of the UN embargo against Iraq, gave rise to a diverse and rapidly growing field of research into unintended and indirect outcomes of economic coercion. When an individual sender or a sanctioning coalition imposes economic costs on the target country, the losses limit the target's resources. This exogenous shock to the target's economy and government resources leads to a variety of adjustments, many of which have nothing to do with the dispute that produced sanctions, and may contribute to or cut against the sender's ability to achieve desired concessions. These unintended and typically indirect changes receive attention in a rapidly expanding line of research, which investigates how sanctions affect target countries on a range of dimensions, from civil conflict and economic inequality, to public health and human rights. Some of these studies also consider effects of sanctions on third parties, including third-party states, multinational corporations, and non-state actors. A much smaller body of research considers unintended and indirect effects of economic coercion on the sender country, which represents an overlooked area in this otherwise active research subfield.

Domestic Conflict

Most studies in this area focus on the direct effect of economic coercion applied with the goal of ending civil conflict. However, none of them restrict the empirical scope of their analyses

to sanctions threatened and/or imposed with the explicit objective of conflict termination. Hence, there is a gap between what these studies aim to analyze and what they can actually accomplish with existing data. Consequently, these findings are suggestive of the impact that economic coercion can have on civil war onset, duration, or termination, but do not serve as direct evidence.⁴

Existing studies conclude that sanctions do not affect the likelihood of civil war onset (Thyne 2006). At the same time, civil war duration is linked to sanctions, but the relationship is complex. On their own, sanctions may not have an effect, but when coupled with military force, they can reduce war duration (Lektzian and Regan 2016). Escribà-Folch (2010), in contrast, finds that sanctions do reduce the duration of civil conflicts, and the effect grows over time. Total economic embargoes appear to be particularly effective in terminating civil wars, through either negotiations or one side's victory. Yet, before a war comes to an end, conflict intensifies under economic sanctions (Hultman and Peksen 2017). This result holds not only for actual sanctions, but also for sanction threats. Only an imposition of an arms embargo leads to a reduction in battle-related fatalities, which can be attributed to the warring parties' reduced capacity for violence.

Economic Spillover Effects

While economic and human costs of sanctions, as well as greater political uncertainty, may temporarily reduce financial flows to the target country, existing research suggests that such negative effects may be short-lived. Specifically, economic coercion may only have a short-term negative effect on the target's attractiveness for foreign investors. Even though US firms are more hesitant to invest in countries targeted by US sanctions during the threat stage, the firms quickly reverse their stance after sanction imposition (Biglaiser and Lektzian 2011). Moreover, third-party investors fill any void left by sender country firms (Lektzian and Biglaiser 2013). If the political and economic environment in the target country remains risky, firms can adjust by shifting their investments to third-party countries with strong links to the target economy. This allows investors to take advantage of the established links, thereby minimizing relocation costs (Barry and Kleinberg 2015). Even when the sender designs sanctions to deliver a sizable economic impact on the target economy, foreign investments decrease only temporarily, without any significant long-term effect (Mirkina 2018).

A similar pattern holds for foreign aid allocations. Even in the case of sanctions backed by a multilateral organization, such as the UN, aid flows remain unaffected. Moreover, US bilateral sanctions improve the target's ability to attract aid because aid donors typically have political or commercial motivations for providing financial assistance. Sanctions rarely change these aid-giving motivations and, hence, donors continue (or even increase) their support for aid recipients during sanctions (Early 2015; Early and Jadoon 2016).

The target's financial stability, in contrast, appears to suffer from economic coercion. While the target's financial relations may return to presanction levels soon after sanction imposition, the temporary increase in political risk may generate doubts about the government's ability to maintain its exchange rate commitment. More costly sanctions enhance risk perceptions and make governments more vulnerable. Therefore, sanctions increase the government's susceptibility to speculative attacks, which often result in currency crises (Peksen and Son 2015). Similarly, deteriorating economic conditions in the target

country and doubts about its access to international capital markets can trigger banking crises under sanctions (Hatipoglu and Peksen 2018).

Economic disruptions caused by sanctions can incentivize subnational actors, such as individuals and companies, to shift more of their activities to the informal sector, as these actors seek to obtain or sell sanctioned goods and services, compensate for declining formal incomes, and cut costs (Early and Peksen 2019). Furthermore, such activities can bring these subnational actors in contact with criminal groups and contribute to an increase in crime and corruption in target countries. This unintended adverse effect can linger even after sanctions are lifted (Andreas 2005).

Public Goods Provision

Sanction-related economic damage leads to reduced government spending on public goods provision, including healthcare, education, and public safety. Target governments' adjustments can further exacerbate the impact of these reduced expenditures by reallocating resources to benefit the supporting coalition or protect themselves against increased internal and external risks (Escribà-Folch 2012; McLean and Whang 2021). Due to rapid drops in government funding, public goods are underprovided, and existing studies document adverse consequences of this underprovision in studies of public health and safety.

Scholars provide evidence of sanctions' detrimental effects on nutrition, infectious disease incidence, and the quality of public health infrastructure in individual target countries, such as Cuba (Garfield and Santana 1997), Haiti (Gibbons and Garfield 1999), and Iraq (Daponte and Garfield 2000). These country-specific studies show that, while mortality rates among vulnerable groups, such as women and children, remained mostly unaffected in Cuba, there was an increase in mortality rates among Haitian children in the age bracket between one and four years. Moreover, in Iraq, the mortality rate for children quadrupled. A more comprehensive large-N analysis suggests that child mortality may indeed increase under sanctions as target country residents lose access to health providers and treatment, and struggle to satisfy basic needs. Peksen (2011) reports that economic coercion is positively correlated with child mortality in target countries, and the size of this impact increases with sanction costs. A related study also suggests that non-democratic countries suffer from public safety underprovision under sanctions, as companies adjust to the exogenous economic shock by cutting safety spending, and governments fail to enforce safety standards. Consequently, more people are injured and experience other adverse impacts from industrial and transportation accidents (McLean and Whang 2020).

Inequality and Discrimination

While costs generated by restricted trade or other economic exchanges and experienced by the target country's population result directly from foreign governments' exercise of economic coercion, the target government could in principle shield less affluent and more vulnerable groups by redistributing some of its remaining resources in their favor. Yet, research shows that the opposite happens: these groups are disproportionately impacted by resource reductions in target countries. Sanctioned governments may seek to divert more resources

to their supporting coalitions to maintain their loyalty under challenging economic circumstances (Escribà-Folch 2012; Wood 2008). Such government support can increase economic strength and political influence of well-connected interest groups, which will demand continued redistribution through protectionist policies even after sanctions are lifted (Pond 2017). Furthermore, autocratic governments may seek to avert domestic protest and rioting by amplifying economic deprivation among the most impoverished groups to make it harder to organize and increase costs of overthrowing the government (Oechslin 2014).

Neuenkirch and Neumeier (2016) confirm that the poverty gap indeed increases under sanctions, and coercive measures that are comprehensive and multilateral are especially harmful in this regard. Similarly, income inequality increases substantially when sanctions are in place (Kirshner 1997). Financial sanctions, restricting investments and foreign aid, cause a particularly large shift toward a more unequal income distribution (Afesorbor and Mahadevan 2016). In addition to less affluent populations, ethnic groups can bear the brunt of shrinking resources and governments' deliberate resource reallocation. Peksen (2016) reports that minority groups are more likely to experience ethnic-based discrimination in multiethnic target countries, especially when governed by autocratic regimes. Multilateral and comprehensive sanctions deliver a more sizable impact than other types of sanctions.

Human Rights

The research on the link between economic coercion, on the one hand, and target countries' respect for human rights and democratization, on the other, presents contradictory findings. Future studies may settle the debate, but existing work provides evidence of negative and positive effects of sanctions on human rights, as well as results indicating the lack of a relationship between the two variables. A subset of studies identifies a clear pattern of deteriorating protection for a broad range of human rights, including physical integrity rights, and civil and political freedoms, during sanctions (Peksen 2009; Peksen and Drury 2010; Wood 2008). Surprisingly, targeted sanctions are no better in this regard than comprehensive sanctions: Carneiro and Apolinario (2016) find that the two sanction types have an equal negative effect on rights to physical integrity. For women, economic coercion leads to deterioration of their social, economic, and political standing in target countries, and women in less affluent countries experience the greatest damage (Drury and Peksen 2014).

Another subset of studies directly challenges these findings. In particular, Gutmann et al. (2020) show that, after accounting for selection effects, there appears to be no relationship between sanctions and respect for basic human rights or economic rights. Moreover, women's economic rights appear to be improving under sanctions, in contrast to the conclusion in Drury and Peksen (2014): women in sanctioned countries join the labor force to replace lost household income, thereby elevating their economic status. The negative relationship between sanctions and respect for human rights only holds in the case of civil and political freedoms (Gutmann et al. 2020). At the same time, sanctions that specifically aim to promote democratization appear to succeed in achieving this goal. Von Soest and Wahman (2015a) argue that the association between this type of sanctions and democratization hinges on destabilization of targeted authoritarian regimes, and show that EU, US, and UN sanctions against non-democracies in the post-Cold War period increase the probability of government and regime change. The positive impact may also serve as a lesson to authoritarian

regimes that do not experience sanctions, but are sufficiently similar to the target of sanctions imposed for human rights abuses. Economic coercion warns authoritarian governments about the dangers of indiscriminate human rights abuses, and results in improvements in human rights practices—or at least prevents them from further deterioration (Peterson 2014).

CONCLUSION

The research on economic coercion has come a long way. It emerged in early case studies of individual sanctions episodes (e.g., Galtung 1967), and developed into a broad field of studies exploring various aspects of economic coercion with nuanced theories and diverse methodological approaches. Throughout this enormous transformation, scholars of economic coercion have shared the same motivation: they want to understand this process, its determinants, and implications. Therefore, scholarship on economic coercion has always been problem-driven, and remains so, as researchers break new ground in the study of sanctions.

New research questions arise as the field seeks to explain rapidly changing instruments of economic coercion and political responses to coercive pressures. The rise of financial sanctions and other targeted instruments and a subsequent emergence of numerous studies on targeted sanctions illustrate how developments in the policy world create entirely new areas of research within the field. At the same time, at no point in the evolution of this International Political Economy (IPE) field was there any significant evidence of divisions over theoretical approaches or methodological tools. The field has grappled with a number of scholarly debates (e.g., do sanctions play an informational role?), but such debates encourage more engagement, not less. While early research on economic coercion was mostly comprised of detailed case studies of individual sanctions episodes, especially high-profile sanctions regimes against Cuba and South Africa (e.g., Galtung 1967; Schreiber 1973), the emergence of large-N studies in the 1990s did not change the problem-driven character of sanctions research. Scholars in this field continued puzzling over sanctions success rates and motivations for governments' use of economic coercion. Over time, studies introduced new methods to the field, including increasingly sophisticated large-N statistical tests, game-theoretic modeling, and survey experiments. Yet, the breadth of available research instruments has made the field richer and more sophisticated, rather than more compartmentalized, and a variety of qualitative and quantitative approaches coexist while providing this field with greater substantive depth and analytical power.

Future research on economic coercion will continue drawing its motivation from shortcomings in existing studies of sanctions, and puzzles linked to real-world events and processes. For instance, more work is required to understand the connection between sanction design and enforcement. Similarly, the role of non-state actors, such as domestic and multinational firms, individuals, and criminal groups, remains poorly understood. Another notable blind spot is the post-sanction period: researchers have mostly overlooked the issues of post-sanction recovery in target countries and in sender-target relations.

When it comes to research questions inspired by real-world developments, the rise of right-wing nationalism may shift the balance of costs and benefits from economic coercion for both potential senders and targets. If public opinion places an increasing priority on

showing strength internationally, there may be a rise in deadlock outcomes, as neither side will have incentives to budge and sanction duration will increase significantly. Such a development could magnify economic, political, and social costs of sanctions, without producing any concessions. The use of economic coercion may also change with technological transformations, which can provide new mechanisms for imposing costs on opponents, while minimizing coercers' exposure. Today, countries can engage in cyber-attacks against opponents, imposing economic damage on adversaries, while shielding themselves from retaliation through plausible deniability (Guitton 2017). Scholarly and real-world puzzles in the area of economic coercion will remain abundant in years and decades to come.

NOTES

1. In addition, I will use "a coercer," "a sender," or "a sanctioner" when referring to the state engaging in coercion, and "a target" or "a coerced/sanctioned state" when referring to the state experiencing coercion.
2. Existing channels for reducing informational asymmetries may also be of limited usefulness under sanctions: Bas et al. (2017) find that neither trade links nor joint membership in international organizations reduces uncertainty in sanctions episodes.
3. The three main datasets used in sanctions research are the Threat and Imposition of Economic Sanctions dataset, covering all sanctions episodes initiated during the 1945–2005 period; the Targeted Sanctions Consortium Database, which includes targeted UN sanctions between 1991 and 2014; and the Hufbauer, Schott, and Elliott dataset, with sanctions episodes from the 1914–2000 period.
4. A study by Radtke and Jo (2018) is an interesting exception: it relies on novel data on UN sanctions against rebel groups, rather than governments, and shows that UN sanctions can limit rebel groups' capacity, thereby increasing odds of successful conflict resolution.

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CHAPTER 17

POLICY TRANSFER

TIMOTHY LEGRAND AND DIANE STONE

INTRODUCTION

Never let any Government imagine that it can choose perfectly safe courses; rather let it expect to have to take very doubtful ones, because it is found in ordinary affairs that one never seeks to avoid one trouble without running into another; but prudence consists in knowing how to distinguish the character of troubles, and for choice to take the lesser evil (1513/2003).

If we set aside its autocratic ambitions, Machiavelli's *The Prince* is a foundational handbook in statecraft. The text traverses topics in public management, national security, foreign policy, fiscal settings, and statecraft, and for its best-practice principles draws examples variously from the good or ill fortunes of rulers across Europe and Italy's principalities. Machiavelli's work is, we hope, not quite so inspirational to today's policymakers, yet one part of the analysis holds as true today as in 1513. The world that Machiavelli observed was given to (mis)fortune, as with "raging rivers . . . everything flies before it, all yield to its violence, without being able in any way to withstand it." The modern policymaker and policy scholar might read and nod in sympathy with Machiavelli's observation in *The Prince* of the contingency of state management: "I hold it to be true that Fortune is the arbiter of one-half of our actions, but that she still leaves us to direct the other half, or perhaps a little less" (1513/2003). Today's state managers face a similarly turbulent outlook in a world that is beset by powerful economic forces that surge across borders, while the state has, itself, faced an ongoing "hollowing-out" that has hindered its sovereignty and capacity to achieve its ambitions.

Understanding these new internal and external contingencies, the fortunes of state, is the concern of modern public policy. As expressed in the Lasswellian iteration, public policy analysis is predicated on a concern for the advancement and preservation of democratic axioms and seeks to enhance the state-based mechanisms, decision-making, and institutions established to deliver outcomes in the public interest (Lasswell 1951). In so doing, the inputs, processes, and outputs of Public Policy are of interest to a broader array of fields that fall under the umbrella of political science, including prominently public administration, organization studies, and public management, but also mainstream political studies and

political sociology. Yet this wave of influences on policy that exist outside and beyond remain largely peripheral to Public Policy: this scholarship privileges, and is conceptually fused with, Westphalian versions of state sovereign authority inside cartographic borders. Hence the key unit of analysis is the state. With its origin early in the twentieth century, and emerging out of the “policy sciences” approach from the 1940s (Deleon 2006) the study of Public Policy (aka Policy Studies) has paralleled the growth of the modern (welfare) state, and consequently has been contained in a “Westphalian conceptual cage” (Stone 2008). By contrast, International Political Economy (IPE) has been far less state-bound due to its later academic blooming in the 1970s that was in tandem with growing economic interdependence, globalization, and regionalization (Broome 2014).

The apparent disciplinary “divide” between IPE and Public Policy presents us with concerns of both a disciplinary and methodological nature that we seek to surface and reconcile in this chapter. It is, first, a disciplinary concern insofar as the research priorities of Public Policy and IPE scholars intersect on shared research interests, and so recognizing these fields’ overlaps will soon be regarded as imperative to establish a common conceptual *lingua franca* that will help us to avoid replication and redundancy in parallel programs of research. Second, a methodological concern follows. The concepts developed by each operate on comparable theoretical assumptions and, by extension, *should* be inter-operable. Put simply, we should be able to borrow ideas from one another.

This chapter sets out to unpack the latent tensions between IPE and Public Policy with a view to promoting some interdisciplinary consonance. We will, first, briefly set out how global forces or “globalization” factors into contemporary policy studies’ analyses of decision-making. Second, we survey some examples of “academic trespassing” between IPE and Public Policy concepts with a specific focus on analytical approaches to the transit of policy across borders: that is, policy transfer which is closely related to established ideas of policy convergence and diffusion alongside more recent thinking on policy circulation and translation (Stone, Porto de Oliveira, and Pal 2020). This point of intersection with IPE provides a broader articulation of possibilities for future shared research and engagement. Third, we turn to recent IPE and Public Policy scholarship on “global policy making” and “transgovernmentalism” drawing out the clear interoperability and mutuality of these two approaches. The final section is focused on the conceptual tools and analytic frameworks that Policy Studies has to offer in understanding “global policy” and its governance, and specifically those that describe the transit of policies across national borders. This leads to our conclusion where we summarize the IPE–Public Policy interface and what IPE might “learn” from policy studies.

POLICY STUDIES IN TIMES OF ECONOMIC GLOBALIZATION

Since its postwar inception, policy analyses have largely been oriented toward within-state dynamics of policymaking. Yet, while there is a stream of comparative public policy that has pursued a transnational research agenda—focused, perhaps most prominently, on European welfare states, Organisation for Economic and Co-operation and Development (OECD) nations, “Anglosphere” polities, and developing countries—the mainstream of policy studies

has taken an introspective view of the state, assigning the “state” as a unitary analytical value. For this reason, we characterize policy studies as dominated by *methodological statism*, in which the state is the primary, or only, referent for scholarly analysis of policy authority, development, implementation, delivery, and outcomes. More generally, this has the tendency to lead to an *ontological statism*, that is, where citizenries and communities view the sovereign state as the “natural” and “normal” political authority and jurisdiction.

This methodological pre-emption is on clearest display in definitions of public policy. The most prominent textbooks center government as the referent for policymaking. Thomas Dye, for example, defines policy as “whatever governments choose to do or not do.” Likewise, Thomas A. Birkland’s (2011) *An Introduction to the Policy Process*, provides a list of definitions that portray public policy as the actions and intentions of government. Similarly in a UK text, Paul Cairney’s *Understanding Public Policy: Theories and Issues* (2011, 23–5), focuses on government. We are not critical of these scholars—the mainstream of policy analysis is a product of its time, and for much of the twentieth century the state was indeed the most powerful and relevant entity to decisions affecting the public. Yet even so, toward the end of the last century, evidence was beginning to mount that the longstanding concept of government—as an entity exerting sovereign power over nation-state policy processes—was unraveling.

The expansion and deepening of transnational economic, political, and social structural forces is frequently cast as the phenomenon of globalization. While its precise characterization remains the subject of debate, few resist the conclusion that the policymaking autonomy of the state is changing. While for some it is largely a transformation of perception and discourse (Hay and Rosamond 2002), globalization remains a referent of contemporary policymaking within and beyond the state and in tandem, contemporary public administration has evolved radically since its disciplinary establishment 60 years ago. Yet, the response of policy scholars has been primarily to evaluate or weigh the impact of global pressures on the functioning of the state, or adverse consequences for national economies or upon decision-making processes inside government.

Unlike Public Policy scholars, globalization is central to IPE’s disciplinary praxis. IPE scholars are distinct in framing power, and simultaneously economic and political decision-making, in their core themes of international trade, international development, international finance and global markets, and beyond. More recently, attention within IPE has broadened: André Broome describes IPE as an “evolving field” and says that “until the global financial crisis in 2008–09 made a mockery of such disciplinary limits, issues related to housing finance systems were largely seen as peripheral ‘domestic’ issues for IPE scholars” (2014, 7). As a result, IPE is turning attention increasingly to “everyday” politics and policy (Hobson and Seabrooke 2007) and, in so doing, now encroaches on issues more directly concerned with public policy. By the same token, many issues that were seen as the purview of public policy—education, health, pensions, social care, water, and sanitation—have become commodified and commercialized for international trade (such as education), or have become cross-border policy issues due to the movement of people, goods, and technologies (such as in global care chains). These developments, amongst others, are pushing the field of Public Policy to modify and update its analytical frameworks to go beyond simple comparative studies in order to account for the transgovernmental and transnational policy dynamics that result (e.g., Kaasch and Martens 2015).

The perimeter between IPE and Public Policy is porous and osmotic as a consequence of converging and overlapping academic interests. Both fields of study are fundamentally

multidisciplinary in addressing how and why the state(s) intersects with globalization. Unfortunately, for many scholars, and especially those in the early stages of their career, the imperatives of scholarly career advancement “requires immersion in a speciality with outputs (articles, books, talks) that win the approval of peers” (Marer 2016). Consequently, academic trespassing can be unattractive. Nonetheless, there are many scholars with anchorage in both Public Policy and IPE for whom “trespassing” in other scholarly waters is not only unavoidable but desirable (Hirschman 1981). We suggest that some prominent early figures have included Frank Biermann (2009), Phil Cerny (2001), Christoph Knill and Dirk Lehmkuhl (2002), Tony Porter (2005), Karsten Ronit (2006), Grace Skogstad (2011), Xu Yichong, and Pat Weller (2009), amongst many others. Though this cohort was a minority at the turn of the millennium, 20 plus years later, the “trespassers” are growing in scholarly strength and seniority.

This chapter cannot do justice to the full range of areas of common conceptual development. For instance, in the post-World War Two era, Policy Studies were central to the development of public goods theory, but today IPE scholarship has been ahead of both scholarly and international policy communities in its understanding of global public goods (GPGs) and “publicness” in the global sphere. It has been international (political) economists (e.g., Barrett 2001; Cooper 2001; Kaul 2005; Stiglitz 1999) who have driven this research agenda.

Public Policy has a long history (Rein 1976) and very rich and diverse literature on “knowledge utilization” (e.g., Weiss 1979), “evidence based policy” (e.g., Head 2008), and the role of expertise in policymaking (Fischer 2000; Halfman and Hoppe 2004) including journals such as *Evidence & Policy*. While there is a budding literature on the use of expertise inside international organization (e.g., Littoz-Monet 2017), IPE contributions of this ilk are less evident but growing fast (*inter alia*, Seabrooke and Tsingou 2016).

Other territory in common is network governance and policy network analysis. Analytical frameworks of “policy communities,” “policy networks,” and public-private partnerships (PPPs) have been “bread-and-butter” frameworks for Public Policy for over half a century. These concepts—such as “national elite policy networks” (Jack Walker 1969)—have been reinvented (in Industrial Relations (IR) as much as IPE) with new labels like “transnational executive networks,” “trans-governmental networks,” or sometimes “multistakeholder partnerships.” For example, the well-known “transnational advocacy networks” framework (Keck and Sikkink 1999) is an internationalized version of the longstanding Public Policy concept of “issue networks.” The engagement of IPE with notions of “network governance” is more limited but, as argued in the following section, is a rising concern for both fields of inquiry. There is already much inter-layering of IPE ideas and Public Policy. For reasons of space, however, we focus on the conceptual frameworks of policy convergence, diffusion, and transfer to discuss in depth one layer of mutual trespass.

SHARED NARRATIVES OF CONVERGENCE, DIFFUSION, AND TRANSFER

Scholarly developments regarding policy convergence (and divergence), policy diffusion and policy transfer (and more recently, policy circulation and translation) do not represent

a hiatus in the dialogue between Public Policy and IPE but rather a multi-faceted conversation that is already taking place. We suspect that Policy Studies has geared analysis primarily around “policy transfer” analysis while IPE approaches are more likely to be found utilizing diffusion and convergence approaches (e.g., Meseguer 2005; Simmons and Elkins 2004). We focus here on the different methodological ramifications of such approaches to suggest that IPE and Public Policy deviate not simply on levels of governance. Our discussion also focuses on the early thinkers in these fields. This means our discussion is Western centric. Nevertheless, we fully recognize that there has been a wealth of scholarly development in the past decade and more. This includes, but is not limited to, the Brazilian School of “policy circulation” (De Oliveira 2017; Milhorange 2017) and a vast number of Chinese and Asian studies on convergence, diffusion, and transfer (e.g., Zhu and Zhang 2020).

Convergence and Diffusion

Convergence refers to “the tendency of societies to grow more alike, to develop similarities in structures, processes and performance” (Kerr 1983, 3). Though its origins lie in industrial-economic development studies of the 1960s, a refocusing upon public policy occurred in the work of Inkeles (1980) and Inkeles and Sirowy (1983), and subsequently Bennett, who described processes of structural convergence occurring through four possible channels: policy emulation; elite networking and policy communities; policy harmonization; and policy penetration (Bennett 1991, 219).

By contrast, the overriding concern of diffusion theorists has been to illuminate *patterns*: “the spread of abstract ideas and concepts, technical information, and actual practices within a social system, where the spread denotes flow or movement from a source to an adopter” (Wejnert 2002, 297). Diffusion theory has a lengthy pedigree, with one scholar tracing it to a 1903 study of the dissemination of farming innovations (Wejnert 2002, 297). It is widely accepted that the diffusion of “innovations” (technological or social) underpins the research interest in this area (see Berry and Berry 1999; Berry 1994). In fact, the feature common to the dominant strands of contemporary diffusion theory is the role of communication and “the existence of transnational communication channels” (Tews et al. 2003, 572).

Diffusion theorists concentrate upon three features of policy innovation: its internal determinants; regional diffusion; and national interaction (Berry and Berry 1999; Berry 1994). Internal determinants are understood as those impulses, imperatives, or decisions that occur in isolation from the international context that give rise to certain policy innovations; that is, in such a case of diffusion *only domestic influences are at work*. Regional diffusion refers to the gradual uptake of a policy innovation based on the proximity of one country to others: in this form of diffusion the likelihood of policy being adopted increases the more other countries, or other states in a federal system, adopt that policy. National interaction is, by contrast, a more proactive notion of diffusion (similar to “elite networking” above), whereby the number of interactions between different national policy officials is seen to be proportional to the likelihood that one or more policies will be exchanged. Notwithstanding the differences, the latter two dynamics are elided and seen as the same process with slightly differing emphases. The ontological challenge of the former is to explain the passive spread of policy without referring to the actions of intentional actors,

which depicts a process without subjects. To resolve this methodological and ontological problem, the “national interaction” approach emphasizes agential interaction in explaining policy diffusion without agents.

To summarize, the policy convergence and policy diffusion literatures share three conceptual features: 1. They focus on the impact of international macro-political structures on domestic policy environments; 2. Policy transfer and policy learning are framed as drivers of convergence and diffusion; 3. They prioritize explanation of outcomes over process. Insofar as these are shared start-points, these are largely complementary literatures:

The literature on policy convergence shares the conclusions of those sociological diffusion studies emphasizing the importance of resonance between the exported ‘object’ and the cultural and institutional setting of the potential importer.

(Lenschow et al. 2005, 799)

Yet, despite these commonalities, a point of important difference on explanatory focus remains: whereas policy convergence analysis seeks to explain state trajectory resulting from policy settings (whether or not adopted from overseas), policy diffusion analysis focuses on the *precursors* of trajectory: the intra- or international spread of policy settings (see Legrand 2021, 26–39).

Policy Transfer

Policy transfer scholarship coalesced via critiques of the “policy innovation diffusion” and its preoccupation with the context—conditions and processes—by which innovative policy practices spread across jurisdictions (Dolowitz and Marsh 1996). Arguing that their “fascination is with process, not substance,” diffusion scholars sought knowledge of the *rate* of policy change at the expense of capturing understanding on the *amount* of policy change (Clark 1985, 63) and they were de-emphasizing the politics and people involved (Dolowitz and Marsh 1996). It was claimed that diffusion scholars failed to pry open the “black box” of policy development—who makes policies? under what conditions? with what information, authority, and political intent?—preferring instead to depict policy movement as a product of factors external to the policy itself. In this vein of research, it is competition with other institutions or the quest for legitimacy that drives institutions to reshape their structures and policy portfolios in the image of their peers.

Policy transfer analysis emerged partly as a critique of the diffusion literature’s failure to account for the role of agents in depicting how policies spread across borders. Dolowitz and Marsh were the progenitors of the policy transfer framework, which operated as a heuristic device to identify and understand the role of agency in the spread and adoption of policy ideas. This framework was influenced by work on lesson-drawing (Rose 1991) and policy convergence (Bennett 1991). It centered policy actors in a process by which “knowledge about policies, administrative arrangements, institutions and ideas in one political system (past or present) is used in the development of policies, administrative arrangements and ideas in another political system” (Dolowitz and Marsh 2000, 5).

Dolowitz and Marsh argued that policy transfer should not focus on the *patterns* of transfer—which was the concern of diffusion and convergence scholars—but rather focus

on decision-making agents, the “strategic decisions taken by actors inside and outside government” (1996). The policy transfer framework emphasizes:

1. the constellation of actors involved—“elected officials, political parties; bureaucrats/civil servants; pressure groups;” policy entrepreneurs; experts; and supra-national institutions (Dolowitz and Marsh 1996, 345);
2. the content of policy transferred, including—“policy goals, policy content, policy instruments, policy programs, institutions, ideologies, ideas and attitudes and negative lessons” (Dolowitz and Marsh 2000, 12);
3. the extent of transfer, including—*copying*: direct and/or complete policy transfer; *emulation*: a transfer of the underlying ideas of a policy; *combinations*: a fusion of two or more policies; *inspiration*: “where a policy in another jurisdiction may inspire a policy change, but where the final outcome does not actually draw upon the original the incentives of transfer” (Dolowitz and Marsh 2000, 13);
4. its constituent mechanisms and processes including whether transfer is voluntary or coercive, and which further relates to;
5. whether the process is related to success or failure.

Because of its ready adaptability, the policy transfer framework has been adopted by an array of scholars working across disciplines and exploring a global assortment of cases (see *inter alia*, James and Lodge 2003; McCann and Ward 2013).

The policy transfer framework has evolved in its application over the past two decades. In recent iterations, instead of *transfer*, scholars refer to policy *assemblages* (Prince 2010) resulting from a patchwork adaptation of policy from multiple sources. Others have sought to deepen understanding of how *policy content* is transferred, resulting in directions such as *policy translation* (Mukhtarov 2014) or *policy mutation* (Peck and Theodore 2010). Rather than signaling its demise, these approaches move the policy transfer framework away from its view of one-way trafficking of policy from exporter to importer jurisdictions and toward policy being transformed in and by transmission by non-state actors and various expert interlocutors resulting in hybrid policy that is once again re-interpreted and modified by local actors in constant “policy circulation” (Stone, Porto de Oliveira, and Pal 2020).

Policy Diffusion and Policy Transfer (in IPE and Public Policy)

Diffusion theorists regard policy transfer as an integral process of the global and regional dispersion of policies. An array of linkages between states operate as differentiated “channels” for the movement of policy ideas:

Economic, political and societal linkages between nation-states offer channels for the transfer of policies across countries. These channels differ with regard to the dominant mechanism by which policy transfer occurs.

(Tews et al. 2003, 572)

On this view, policy transfer is rendered as one of many possible mechanisms of diffusion, yet agents’ motivations for engaging in cross-border policy adoption is distinct from the operation of the channel itself since, on this perspective, transfer mechanisms can be triggered

for differing reasons. For example, policy transfer might occur when states seek to keep pace, and “up-to-date” with their peers (Drezner 2001), a dynamic often on display in international institutions: for example, the principles of “structural adjustment” and the “Washington Consensus” were developed and promoted by the International Monetary Fund (IMF) and World Bank in (Broome 2014). The OECD’s peer review mechanism is a paradigm example of ideational competition driving diffusion between states (Legrand and Vas 2014). The mechanism works by putting a state’s specified policy apparatus under review by fellow OECD member countries, who combine to produce an assessment with recommendations to “improve its policymaking, adopt best practices and comply with established standards and principles” (OECD 2003, 2). On this approach, moral pressure drives a policy-learning process (Pal 2014) in which the OECD can influence the policymaking logics and discourses of state managers (March and Olsen 1998, 961).

As a consequence, for diffusion theorists international institutions are pivotal in the international diffusion of policy ideas amongst their member states. Yet, notwithstanding the weight assigned to ideas in international institutions, the structural imperatives of the global economic climate remain at the forefront of diffusion analysis. Tews et al., for example, maintain: “National policymakers may be forced by considerations of competitiveness to adopt the innovative policy measures of pioneers in order to avoid significant economic or administrative adjustment costs” (2003, 575).

Accounts of policy diffusion and convergence hold to some common precepts in which agency is incorporated into their respective analyses: first, they recognize that states’ economic and political decision-making are interdependent; and, second, the likelihood that states are likely to share or transfer their policy approaches to peers with similar policy challenges is a direct function of the “channels” of communication between peer actors. Tews et al. argue that these “channels of diffusion” are “multipliers of knowledge dissemination and/or ideational catalysts of policy convergence,” through which “problem perceptions, ideas and policy innovations” transfer between countries and international organizations (2003, 575).

Are there ramifications from this digression into convergence, diffusion, and transfer for IPE and Public Policy understandings of “global policy”? We suggest, *in general*, that IPE is characterized more by analysis of diffusion, whereas Public Policy has been more engaged by transfer perspectives. As consequence, the two fields sometimes disconnect:

- First, IPE scholars tend to be methodologically oriented toward structural explanations whereas policy scholars have leaned more toward explanations of agency in transfer (although there are frequent intersections of these approaches) encapsulated in the idea of “systematically pinching ideas” (Schneider and Ingram 1988).
- Second, the upshot of this bifurcation on agency structure is that Public Policy Studies often focus on motivations and process in decision-making while IPE scholarship highlights the analysis of outcomes.
- Third, the levels of analysis tend to differ. Public Policy analysis often directs its gaze to meso-level processes and how these connect to micro-level processes and the mechanisms of transfer. IPE addresses macro-level structural considerations.

These are broad-stroke contrasts (to which there are exceptions). Even so, these contrasts help make explicit the distinct conceptual starting points that have led IPE and Public Policy in similar directions but with different routes.

GLOBAL POLICY, TRANSGOVERNMENTALISM, AND TRANSNATIONAL ADMINISTRATION

The preceding discussion outlines both common concerns and tension points between the two fields. However, an area of impaired analytical vision of both centers around the new administrative domains of transnational governance. As mentioned earlier, the propensity toward methodological nationalism has meant that Public Policy has been slow to lift its analytical gaze above the nation-state to engage with transnational policy dynamics. An exception is the growing school of work on “international public administrations” that has emerged in the past decade, including recent work on the “European administrative space” (*inter alia*, Bauer, Knill, and Eckhard 2017). Meso-level processes concerning public accounts and audit or the organizational decision dynamics behind matters such as human resource management has not been the forte of IPE. In this lacuna, however, we identify an opportunity for collaborations. Specifically, the de-synchronization of IPE and Public Policy can find conceptual recuperation by grappling with these nascent administrative domains.

The growth of new architectures of governance and patterns of administrative praxis within them, provide opportunity for the scholar of Public Policy to inform IPE understandings on matters such as policy design, regulation, and bureaucratic management. We highlight four modes of governance where the scholarly interests of IPE and Public Policy can intersect profitably. The first two modes are different manifestations of transgovernmentalism; the second two modes involve different degrees of private involvement in transnational governance:

- i. Horizontal dispersion of governance through regular cross-national coordination by government officials working in the same policy sector on shared policy problems. These national legislators and other public officials, judges, and regulators build transgovernmental professional networks to manage policy problems (Legrand 2019) and carry lessons along these networks. This coordination is mostly an intergovernmental dynamic of information sharing that recognizes the sovereign authority of state officials.

Although transgovernmental arrangements bring together officials with significant formal regulatory and legal responsibilities, these networks are “necessarily informal” arrangements. They do not have a legal or treaty-based existence. One prominent example is the global regulatory role of the Basel Committee on Banking Supervision composed of officials from central banks (Jordana 2017). Another is the Pharmaceutical Inspection Cooperation Scheme which is a non-binding, informal co-operative arrangement between regulatory authorities in the field of medicinal products for human or veterinary use. There are also a range of environmental enforcement networks such as the International Network for Environmental Compliance and Enforcement (Dawes et al. 2012). Transgovernmental networks are conduits for officially sanctioned “best practice” standards and soft law, whether viewed from a policy transfer or diffusion and convergence perspective.

- ii. Vertical decentralization of governance through the types of horizontal transgovernmental networks of public sector officials mentioned above but who act in

conjunction with international civil servants in international organizations to secure multi-level policy coordination.

A prime example is the joint United Nations Development Programme (UNDP) and OECD-initiated Tax Inspectors Without Borders (TIWB) which supports national authorities in building tax audit capacity and strengthening international cooperation on tax matters. In a policy transfer and diffusion dynamic, TIWB has helped to transfer lessons, standards, and policy tools to developing country administrations around international tax matters as well as general audit practices and risk assessment. In a reverse dynamic, the Brazilian state has been a policy transfer entrepreneur exporting its model of rural and agricultural development to Mozambique with back-up support from international bodies like the Food and Agriculture Organization (FAO) (Milhorange 2017).

- iii. Crosswise delegation of governance over the public-private divide. Official actors—whether they be government officials or international civil servants—build policy partnerships with private sector actors. These arrangements have varying names: for example, the UN's multi-stakeholder partnerships, the World Bank's "global and regional programs," "global public policy networks," or "transnational public-private partnership." They are quasi-public or semi-private (see Nielander 2020).

The Global Partnership for Education and the Global Water Partnership are two classic examples. Both bring together low-income countries, public and private donors, international organizations, business, civil society, and professional associations that are recognized actors in the specific policy sector to jointly deliver policy programs and in the process, share knowledge and lessons. Launched on the initiative of the Bill and Melinda Gates Foundation, which convened major pharmaceutical companies to work alongside both governments and international organizations, the Global Alliance on Vaccines and Immunisation (GAVI), delivers the public good of vaccines at low cost to communities in developing countries. GAVI shot into public recognition in the context of Corona virus (COVID-19) pandemic. GAVI has also played a central role in the establishment of another global partnership called COVAX for financing and delivery of COVID-19 vaccines.

These innovations in governance have captured the attention of scholars not only IPE and Public Policy but also from other disciplines like Law (Nielander 2020) and Development Studies (Wessal and Wescott 2019) who have also noted the burgeoning transnational forms of the dispersion, decentralization, and delegation of decision-making, financing, and policy delivery. New generations of scholars in both fields are noting the structural "fragmentation" of the architecture of governance that results (e.g., Moloney 2021). Yet, IPE (along with Legal Studies) has probably been more attuned than Policy Studies to our fourth dispersed mode of governance.

- iv. Independent initiatives or private regimes of transnational governance. Transnational private regimes emerge from common interests and the shared quandary of market-based problems. Transnational private regulators issue standards in areas as diverse as the environment, sustainability, anti-corruption and legality, human rights, data protection, product safety, and financial instruments, and are often complemented by the hard or soft law produced by international organizations and by nation-states.

A classic example is the private sector bond-rating agencies such as Moody's and Fitch. Other examples are environmental management systems or certification of sustainable fisheries that seek to bind multinational companies to standards of due diligence or codes of conduct in production processes or management of global supply chains. For instance, the International Social and Environmental Accreditation and Labelling Alliance now known as the ISEAL Alliance is one of the most important voluntary standard-setting and accreditation bodies promoting convergence and harmonization on matters as diverse as eco-labeling or workers' rights in global supply chains.

The various manifestations of these four governance dispersions have attracted scholarly attention leading to a plethora of case studies, comparisons of regulatory styles, and critical analyses on their (lack of) efficiency and effectiveness. Yet, the possibilities for a mutual dialogue between Public Policy and IPE lie in addressing common concerns across each of these four dispersions of governance, or alternatively, these four taken collectively as an agglomeration of transnational policymaking.

The quasi-public transnational policy communities that revolve around these four modes constitute a global public sector or a discernible "global administrative space." Not even recognized as a "jurisdiction," let alone featuring a sovereign authority, a clear "dichotomy between domestic and international has broken down" (GAL 2018; Moloney 2021). This "public sphere" and "administrative space" is incongruent with the norms of public authority and legitimate exercise of power that prevail for the conduct of sovereign states. Formal public actors in transnational governance—that is, international organizations and states—remain central to the design, delivery, and financing of global public goods and services. Yet, they are inextricably reliant on a range of non-state actors for implementation and monitoring, for legitimacy and accountability of global policy norms and rules. Depending on the policy sector or the character of a transnational problem, the relevant business and civil society groups or networks of experts and professionals are consulted and co-opted into governance processes. These processes are in constant evolution and provide ample opportunity for non-state actors to incrementally embed themselves in deliberations over policy design and reform.

The "administrative styles" and "executive powers and accountability" of transnational public-private partnerships or private regimes generate pressing new questions about power including concerns about the efficiency and transparency of their operations as well as the accountability, legitimacy, and representativeness of their management. Questions around the conduct of transnational bureaucratic elites, the expert-executive nexus, and the HR implications of transnational administrations are all puzzles that are opportunities for IPE and Public Policy scholars alike for theory development and policy practice.

CONCLUSION: CAN IPE ENGAGE WITH PUBLIC POLICY?

There is much that has already been cross-fertilized with IPE as our foray into the policy diffusion and transfer literature reveals. More interaction could be achieved but as we suggest in the opening section, academic incentive structures and sclerotic disciplinary

traditions can act against the propinquity of Public Policy and IPE. Other disciplines and fields—Organization Theory, Legal Studies, Political Geography, and Development Studies, amongst others—offer equally interesting insights and conceptual toolkits. There is already significant cross-pollination. That there may well be a “slow convergence” between of Public Administration, Public Management, and Public Policy with IR and IPE, as claimed in one study (Kim et al. 2014, xiv), is some considerable way from being fully realized. Despite the common territory that we identify between “global public policy” on the one hand, and IPE on the other, the historical legacy of differentiated trajectories in the disciplinary development of IPE and Policy Studies persists. However, in an era of international public administration (as understood in the policy studies lexicon) and global governance (in the *lingua franca* of IPE), greater dialogue is necessary in order to discern and understand the full dimensions of transnational problems. Once again, in Machiavelli’s words, *prudence consists in knowing how to distinguish the character of trouble*. This means not only learning about promising governance innovations and policy models developed in different contexts to tackle the “troubles” of today, but also learning and engagement across disciplines in order to come to a more fulsome understanding of the causes of these troubles.

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CHAPTER 18

REGULATION

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INTRODUCTION

THERE is a rich and diverse tradition of studying “regulation” in International Political Economy (IPE), and the study of this theme has played a significant role in the development of the field. Indeed, all topics of importance for international political economists involve regulatory standards or regulations of some kind. It is thus unsurprising that over the last 20 years, scholarly interest for this broad topic has grown steadily, a trend largely paralleled by its mounting popularity among students of domestic economic institutions (see Baldwin et al. 2012). In this overall context, it is therefore unsurprising, too, to observe in this particular domain some similar patterns as those identified by Seabrooke and Young (2017) in the wider discipline.

In effect, and until the end of the 1990s, regulation was essentially envisioned through the lens of post-Marxist theories. The term was mostly used in its broadest sense to designate a wide-ranging set of macroeconomic institutions linking the “production paradigm” and the “accumulation regime” of global capitalism (see, e.g., Jessop 2000). In spite of, or more probably due to its far-reaching scope, this body of research was more interested in offering a general overview of global capitalism and an account of its more recent developments. It was, symmetrically, less driven by a commitment to grasp the newer relationship “between those defining, implementing, recognizing and monitoring rules” and “those complying with them” such as global firms and global markets, states, as well as a variety of non-state actors (Graz 2006).

From the end of the 1990s onward, a new generation of research started to offer subtler and more specific analytical tools to grasp regulatory authority and practices in the IPE. Usually, these contributions were also concentrated on more definite issue areas, such as telecommunication, environmental regulation, air transportation, or the services industry (see, e.g., Nielson and Tierney 2003; Pollack 2003; Schmidt 1998). Rational-choice scholars arguably played a critical role during that period. Using the frameworks of neoclassical economics and international trade theory, they modeled regulation as a process through which rival actors work politically to shape regulatory rules or bodies according to a range of self-regarding goals (Lake 2009). As such, this strand of research put a strong emphasis on the motives behind the design of regulatory rules and the interests actually served by regulators

once rules were implemented. Firms and more broadly, special interest groups, were typically seen as being both the drivers and the habitual winners of the regulatory game in the international political economy—often thanks to the ostensible support of bureaucrats or reelection-seeking politicians at the domestic level (see Vaubel 2006 for a discussion).

This approach was more or less directly challenged by a number of contributions. The most significant departure from its underlying conceptions came after the second half of the 2000s, and in an even more pronounced way after the 2007–2009 financial crisis. Despite strong initial expectations, changes in financial regulation after the crisis were widely perceived as incremental and limited in scope. While rational-choice authors (with whom some post-Marxists could agree) explained this relative status quo by the capture of regulatory institutions by powerful regulated interests (see further), a new generation of scholars cast doubt on this broad rush picture. Instead, they have offered a deeper and more realistic account of actors' way of operating in global regulatory settings using sociologically-informed approaches (Thiemann and Lepoutre 2017, for an overview see Harrington and Seabrooke 2020). On the basis of a number of empirical studies of post-crisis financial institutions as well as of international organizations charged with regulatory powers or mandates, these contributions literally unpacked the regulatory game, namely by looking at what was happening between competing interests and regulatory outcomes (Pagliari and Young 2016). This includes features as diverse (and arguably, as important) as the complex interrelations between global markets and (trans)national regulators, the organizational structures of regulatory institutions, and the role of ideas in the choice of certain regulatory instruments. In sum, light was shed on a sizable array of individual and collective actors largely ignored by positivist accounts. These contributions were thus able to demonstrate that regulation also rests on complex organizational structures, in which actors operate through the lens of various beliefs and expectations. More fundamentally, they revealed that regulatory outcomes were highly contingent, and this notably because they always involve ambiguous concepts, communicative structures, and rest on various institutional dependencies. This overall shift in the literature ultimately provided a strong alternative to the regulatory capture thesis, interest group, and distributive politics approaches.

Though diverse in its theoretical and methodological orientations, this series of contributions arguably paved the way for a renewed understanding of the mechanisms accounting for regulatory orders institutionalization, reproduction, and change. It is with highlighting these innovations that the present essay is primarily concerned. To do so, we first examine the positive theory of regulation by briefly summarizing its promises and pitfalls. The next section of the article reviews the foremost insights of recent developments in the study of regulation in IPE. As already suggested, these latter contributions essentially developed after the 2007–2009 financial crisis—and are, for this reason, primarily focused on post-crisis financial regulatory institutions. We reflect on the current state of the field in a concluding section.

THE POSITIVE THEORY OF REGULATION

How should international political economists interpret the spread and the diversity of environmental regulations? How should they analyze the emergence of safety rules in international aviation markets? Or solvency requirements in the insurance industry? How can

we explain the choice for a certain amount of core capital that banks and bank holding companies must retain as a buffer against their own risky behaviors? Why is compliance with food standards more demanding for some categories of products? From a theoretical perspective, all of these questions relate to the formation and to the outcomes of regulatory orders in transnational settings—with regulation hereinafter understood as “the organization and control of economic. . . activities by means of making, implementing, monitoring, and enforcing of rules” (Mattli and Woods 2009).

At the domestic level, the institutionalization of regulatory orders seems relatively simple. Regulation is usually implemented by elected officials to restore the (nominally) public interest where there exists a market failure, or any kind of distortion considered as detrimental to the people’s welfare. This process generally sees a legislative body delegating the implementation of regulation to a supervisor (typically a regulatory agency), in charge of monitoring the enforcement of rules by a regulated industry. There is an obvious, twofold issue when this broad understanding is transposed to international settings. On one hand, states (and not a single’s country legislature) are here the collective political principals of global regulators (Hawkins et al. 2006); on the other hand, these transnational settings involve a larger and significantly more diverse array of firms or side-markets that are usually found in a given country.

Such a rather simplistic statement of the problem also poses another twofold issue, irrespective of the level of governance where it applies. How can we locate the public interest for a given matter—is the fact that regulation is decided by a democratically elected body sufficient to infer that it has been preferred against any kind of special interest? The second and related issue is the role actually played by the regulated industry or interest groups. Needless to say, these actors are capable of shaping both the implementation of regulatory rules and the course of regulatory decisions—at least certainly better than the wider citizenry.

These different issues have been amply discussed and documented, initially by students of domestic regulatory institutions. Since George Stigler’s seminal article, a large body of literature has notably come to suggest that regulation was better understood as a commodity that politicians or regulators were able to sell to the most influential interest groups. Indeed, the latter perpetually compete “to improve their economic status,” and are thus the main demander for regulation, including subsidies, price-fixing mechanisms and a variety of standards to protect dominant firms against new entrants (Stigler 1971). In turn, bureaucrats and politicians might expect a number of symbolic or material rewards from engaging in such a transaction.

This construal of the problem was certainly one of the most influential in the study of economic regulation, and it largely guided research on this topic during the next decades (see Dal Bó 2006). From the end of the 1990s onward, it was gradually imported by international political economists seeking to explain the political determinants of transnational regulatory orders formation. Most of these early importers were rational-choice scholars influenced both by neoclassic economics (from which the bunch of regulation theories originate) and international trade theory (Lake 2009). Adapting these broad approaches to the study of regulation in a variety of sectors, they progressively laid the foundations of the positive theory of international regulatory institutions. Its basic assumptions have been clearly expressed by John Richards in his study of international aviation markets. Global regulations, according to Richards, should be first and foremost viewed as “interstate regulatory bargains created by national politicians to serve the interests of domestic political

constituents” (Richards 1999). Drawing on regulatory theory, Richards considers indeed that “the fundamental commodity at stake in international regulation is the transfer of wealth, with constituents demanding regulations favourable to their interests and politicians supplying regulations designed to secure electoral success.” Thus understood, a major source of conflicts over the shape and the priorities of global regulatory orders relate to the fact that domestic politicians have divergent preferences due to their different constituencies.

A second, related implication is that the public interest is rarely the one actually served by regulators or by their domestic principals. Instead, dominant firms would be much more likely to be both the main demanders for and the main beneficiaries from regulation. In effect, firms or interest groups would be usually more effective at influencing politicians or at capturing regulators due to the small size of their group, the usually superior amount of resources they possess and their relatively small array of per capita stakes—three factors allowing for reduced risk of collective action problems (Chang 1997).

On this broad basis, a number of international political economists have compared the interests of dominant firms in given sectors to the design of rules and regulations at the international level (see, e.g., Bernauer and Caduff 2004 on environmental regulation). Crucially, these contributions have repeatedly identified statistical correlations between industry interests and regulatory outcomes (see, e.g., Broz and Hawes 2006 on US banks and the International Monetary Fund (IMF)). Overall, the positive theory of regulation in IPE has thus sought to establish that regulation was “acquired by the industry, and designed and operated primarily for its benefit” (Dal Bó 2006) just as regulation theory did for domestic regulatory institutions (see Noll 1989).

Admittedly, there is a non-negligible diversity of views among scholars using the positive theory of regulation in IPE. Yet, a number of features—and arguably, pitfalls—are identical from one author to another. Explanation based on regulatory capture was indeed rarely tested against an alternative model that could account for similar outcomes. Might particular firms receive regulatory support even when the regulator is not captured by the industry (Carpenter 2004)? For most regulatory theorists, it seems that actual effects of regulation prove sufficient to establish that regulatory capture or distributive politics of some sort are at play where such correlation was found (see also Benoît 2021a). A similar response was frequently offered to critics pointing out the fact that this approach hardly proved the industry’s intent at capturing the political or the regulatory process. In the same vein, the public interest against which special or industry interest was supposedly pitted remains scarcely discussed (see Carpenter 2013 for an extensive critique of regulatory theory). Overall, and under this approach, the regulatory process, regulators, or even firms’ behaviors remain largely unobserved, and the respective interests of the different players involved are usually defined exogenously.

UNPACKING REGULATION: THE CASE OF THE POWER OF FINANCE IN THE POST-CRISIS ERA

During the 2000s, a number of prominent contributions have sought to challenge rational-choice accounts in regulatory theory on the basis of fine-grained empirical studies of

transnational regulations. Woll, for example, suggested that in many cases the perceived success of industry could be an outcome of a wider regulatory strategy to advance the public interest. This statement was amply confirmed by her study of regulatory change in the telecommunication and air transportation sectors (Woll 2008). Yet, it is essentially after the 2007–2009 transatlantic financial crisis that the positive theory of regulation was more extensively disputed—paradoxically at a time where capture was a widespread interpretation of regulatory responses to the crisis (Baker 2010). After this critical event, a commonly shared questioning among international political economists has thus been to account for changes in financial regulation, largely perceived as incremental (Moschella and Tsingou 2013).

More fundamentally, the essential paradox here is that the status quo was broadly maintained (Helleiner 2014) while the silent politics of financial regulation (Culpepper 2011) was severely disturbed. In a context where the power of finance suddenly became highly salient and heavily politicized, the common wisdom according to which politicians are usually trading regulation “for re-election in the light of interest-group demands for regulation” (Baldwin et al. 2010) therefore appeared to provide a rather limited explanation. Sharing this overall diagnosis, a number of international political economists have tried to uncover other sources of finance’s influence on decision-making, often on the basis of new research methods allowing for the analysis of venues largely unexplored by existing accounts. Together, they have significantly improved our understanding of regulation, usually thanks to more sociologically-informed approaches. By doing so, they literally “unpacked” regulation by providing empirical insights on crucial processes that were happening between agency design and regulatory outcomes.

Three distinct sets of contributions have been made on this basis. The first relates to the structural dependency of the state on finance and on firms’ (infra)structural power (Braun 2018). If some arguments made in this respect were not necessarily at odds with capture theory, they shed light on the key role played by broader institutional dependencies on the reproduction of regulatory orders and this, even in critical times. A second body of literature has focused on the institutional set-up of markets, based on global financial markets coupled with national regulation. It has notably emphasized a recurrent mismatch between rules developed by global actors (such as the IMF or the World Bank) and policy-making at the national level, which tends to recurrently exhibit “implementation gaps” (Halliday and Carruthers 2009). The last—but certainly not the least—series of contribution has examined the role of ideas and concepts forged by a number of professionals in international organizations, particularly in the area of financial surveillance. These different sets of contributions arguably provided a more realistic account of the power of finance. More generally still, they have also significantly influenced how regulatory outcomes could be pictured.

States, Firms, and Their (Infra)Structural Power

Over the last 10 years, political economists have increasingly acknowledged the fact that the stability of “any law or regulation and the lack of opposition to these policies, could be due to capture, but also potentially to inertia” or to a range of institutional dependencies that render policy change harder in certain areas (Carpenter and Moss 2013). Recent findings in IPE on finance have amply illustrated that claim. In the literature, establishing these various claims

has often required the development of more inductive methodologies, typically informed by ample documentary analysis and qualitative interviews with key participants in these processes.

This was notably the case of contributions that have focused on the structural dependency of the state on finance, and its related implications for regulation in this sector. Independent of the pressures of financial intermediaries such as banks and their representatives positioned between global markets and national regulation, states and capitalist economies depend on financial institutions that persistently generate new credit to fuel growth. In this context, banks are of outstanding importance. While long acknowledged in the literature, the Bank of England has publicly emphasized quite recently the large extent of inside credit money generated by banks which is responsible for much of the growth dynamics in capitalist societies (Braun 2016). As such, states are susceptible to claims of impending credit crunches should certain structures of the banking industry be disrupted. One aspect of such threats can be seen in the negotiation over bank structural separation post-crisis, where interest groups of the banking industry claimed that any measures reducing market liquidity would lead to a reduction in credit growth (Endrejat and Thiemann 2018).

More fundamentally, the power of finance and the state's dependence on it expands to crucial market infrastructure, such as the repo-market (Gabor 2016). Central banks around the developed world, impressed by the supposed security of the repo-market, transformed the latter into a vital piece for their monetary transaction mechanism—ultimately linking the functioning of this market to central banks' capacity to implement monetary policies. There was little choice for central bankers but to stabilize these markets, once a run on the repo-market had occurred. This dependence on smoothly functioning collateral markets has, if anything, increased post-crisis, as collateral has become an ever more important ingredient for securing financial stability (Gabor and Vestergaard 2016). This had a significant impact on subsequent regulatory reforms, which were repeatedly hampered by concerns over market liquidity and its negative impact on the implementation of monetary policies (Thiemann et al. 2018). As such, and by looking at broader dynamics than the positive theory of regulation usually did, these different contributions were able to show that when state actors transact on different markets (and, particularly, on financial markets) to serve a variety of governance purposes, they often create “infrastructural entanglements” (Braun 2018). As demonstrated by Braun, infrastructural factors may, in turn, exercise a distinct source of private-sector power for which explanations based on interest group politics and regulatory capture provide a limited account—especially since in such settings, public-sector actors are not just regulators but are direct participants in financial market activities.

A Structural Mismatch between Global Markets and National Regulations

Timidity by regulators to impose stricter regulation is often linked to their incapacity to impose similar rules and hence similar regulatory costs on a global level. This is largely due to a structural mismatch between global markets and norms and limited (often national or even sub-national) reach of regulators. Such dynamic has been clearly expressed by Halliday and

Carruthers (2009) in their work on the recursivity of law in global settings. The globalization of regulatory rules, they argue, often proceed through different cycles—first at the national level “through recursive cycles of lawmaking,” secondly at the global level “through iterative cycles of norm making,” and thirdly “at the nexus of the two” (Halliday and Carruthers 2007). In turn, this process can result in limited regulatory change and even favor industry or special interests, especially since it is usually driven by “the indeterminacy of law, contradictions, diagnostic, struggles, and actor mismatch” (Halliday and Carruthers 2007). As such, it is marked by a high level of uncertainty and contingency—two additional and arguably crucial features of regulation largely ignored by positive theorists.

In the area of financial regulation, this logic has been further scrutinized by Thiemann (2014) who has shown that due to the different speed of innovation between bank activity and bank regulation (Kane 1988)—especially of bank activities surrounding existing global banking regulation—regulatory action is often limited. This repeatedly leads to the futility argument, in which all that national regulation is able to achieve is the regulation of national banks out of a market in which foreign banks can continue to operate (Thiemann 2018). This perception of futility combines with an understanding by national governments of large domestic banks as an important asset to national economies to produce a particular kind of industrial policy (Story and Walter 1997): regulatory forbearance, which lowers the regulatory costs for domestic champions. These national champions, which often have strong ties with their national administrations and entertain a network of mutual exchanges of favors beneficial to both sides (Jabko and Massoc 2012), are supported by state administrations in their struggle for survival.

The negative impact of these domestic concerns over the fate of national champions was recognized by the regulatory community post-crisis. Arguably, the persistent appeals for common implementation of new global rules by the G20, and the creation of the Financial Stability Board as a fourth pillar of global governance of finance (Helleiner 2010) with its main task of ensuring global implementation of reform efforts, managed to reduce these concerns. Furthermore, the writing of a common rule book in the European Union (EU) through the European Banking Authority and the transfer of enforcement of these rules for the largest banks in the Euro-zone to the European Central Bank have been important institutional innovations in this respect. Nevertheless, even if many of these concerns could be mitigated through institutional innovation, the dynamic nature of the financial system, often itself provoked by financial regulation, ensures that these pitfalls prove enduring (Friedrich and Thiemann 2017).

Regulatory Ideas, Professions, and Concepts

Many features of regulation are well-captured by the analysis of multi-scalar institutional dependencies and through a careful examination of its propensity to direct the behavior of regulated entities, sometimes unexpectedly. Yet, regulation is arguably more than that. In essence, it also comes through a variety of texts encompassing a given order of concepts, which provide common meanings and categories. As such, it legitimizes a set of ideas expressed through metrics and calibrations accompanied with a conception of what should count, what should be viewed as a reasonable behavior or as appropriate conduct, and what should not.

Such feature—clearly expressed and defined by Carpenter (2010) as the “conceptual” appearance of regulatory power—was arguably a third and significant area of inquiry for international political economists over the last decade, with two broad ramifications. The first derives from the study of ideas and their role in regulatory change. Here, a major insight of international political economists has been to better connect ideational factors to the behaviors and perceptions of a variety of professionals involved in regulatory politics—usually through interview data and network analyses. Another and more recent set of contributions have used new research techniques such as automated text analysis and topic modeling to understand script-writing and concepts generation in international organizations charged with regulatory mandates or standard definitions. These two strands of research are now increasingly interdependent.

An area of interest that received extensive coverage was the role of financial economics in shaping financial regulation—and more precisely, in justifying the move toward more and freer financial markets in the pre-crisis context (MacKenzie 2006). Emphasis has notably been placed on how financial economics contributed to the formation of an overall consensus among regulators around the efficiency of private risk management and the capacity of industry to measure and manage (better than regulators) the risks they were facing in markets (Lockwood 2015). The prevalence of this view in financial economics was an important factor in what Kwak has described as “cognitive capture” of regulators by bankers, as they both spoke the common language of economics (Kwak 2013). Driven by public-private forum, such as the G30 which brings together CEOs of large financial firms and the senior management of national and international public regulators (Tsingou 2015), the idea of the governability of risks through these numbers quickly spread. It was adopted by regulators in their attempt to modernize risk weights in Basel I. This happened even though the regulators’ embracing of this idea was always conditional and not completely uncritical (Young 2013, Baud and Chiapello 2014).

In a similar vein, a notable contribution by Seabrooke and Tsingou (2020) demonstrates how international financial regulation is guided by groups that can align the career incentives of actors in three professional ecologies of regulators and public officials concerned with financial regulation, academics working on these issues, as well as risk managers in private industry. According to this analysis, private risk management provided a common language and was valued in all three professional spaces, generating incentives for policy entrepreneurs tied to the Basel Committee on Banking Supervision to push forward this particular way of viewing the world. The argument about private risk management as a hinge connecting these different ecologies is based on the widely observed phenomenon of revolving doors, which provides incentives to professionals to invest their skills into regulatory objects and devices which are part of all three realms, as they foresee a shift into a related area during their career.

After the crisis, significant attention has been paid to similar trends in the attempts at re-regulating financial markets. Marcussen (2009) has notably shown that economists were occupying increasingly prominent positions in the design and conduct of banking regulation, in a context where regulators were seeking a new rationale for post-crisis rules and standards. With central banks plainly in charge of financial regulation post-crisis, it was hence up to economists to provide input into how and why financial regulation should proceed. The fundamental pillars of mainstream economics were to play an important role in this regard.

In another respect, approaches connecting both intellectual and professional dimensions have also proven particularly heuristic when applied to ideational change in international organizations. On the basis of extensive bibliographical and network analyses, Ban (2014) has, for example, been able to trace the evolution of the IMF's doctrine "to staff politics, more diverse thinking in mainstream economics, and a careful framing of the message through the use of mainstream macroeconomic models." Focusing on the role played by the Bank of International Settlements, the IMF and the Financial Stability Board in shadow banking regulation, Ban and co-authors (2016: 1001) have shown how experts intervened in global regulatory debates, finding in their case that "technocrats reinforced each other's expertise, excluded some potential competitors (legal scholars), co-opted others (select Fed and elite academic economists), and deployed measurement, mandate, and status strategies to assert issue control."

In part through these various developments, regulation was increasingly understood through its more symbolic and rhetorical dimensions—that is, not only as a body of rules, but as an order of concept coming with its own meanings and hierarchies, and expected to become ingrained in everyday routines of a range of public and private organizations (see Benoît 2021b). Halliday and co-authors (2010) have, for example, shown that international organizations' texts "draw upon a finite repertoire of rhetorical devices to propagate legitimation strengths. . . to amplify or compensate for legitimation weaknesses and to express rhetorical repertoires which convey their own intrinsic merits." This finding was echoed by Kentikelenis and Babb (2019) in their study of the IMF. International organizations, they argue, often engage in a sustained "symbolic work" to bolster the legitimacy of institutional change, drawing on a range of cultural and linguistic materials. The idea that regulation was essentially made with words encapsulating a range of ideas and normative assumptions through various communicative structures was further extended by Kentikelenis and Seabrooke (2017). Focusing on "the codification of norms in prescriptive behavioral templates," they show that "script-writing is a function of both world-cultural frames and material interests, held by different intra-organizational actors: scientific inter-governmental organization staff and state representatives in governing bodies, respectively" (Kentikelenis and Seabrooke 2017, 1065). The conceptual facet of regulatory power, for which there arguably exists a mounting interest in the literature (Benoît 2019), is also of prime importance for authors examining the power of standards and their propensity to shape transnational markets (Graz 2019).

CONCLUSION

The study of regulation in IPE became considerably more complex in the last two decades. This has helped to provide more realistic understandings of its many features—arguably more than existing accounts usually did. Unpacking regulation, these contributions have notably exposed the necessarily broader institutional dependencies shaping regulatory behaviors, the various implications of recurrent mismatches between global markets and national regulations, and the crucial role played by ideas conveyed by a wide set of professionals in regulatory processes. Together, these various insights have established that in such settings, regulatory authority was necessarily hybrid and shared across private and

public actors (see Baldwin et al. 2010; Graz 2019). They also amply documented the fact that regulation is not solely an authoritative intervention on the market, but often has market-making capacities due to its provision of common norms, metrics, and standards (see the various contributions reunited in Balleisen and Moss 2012 on this later point).

Admittedly, taking a higher degree of empirical accuracy has inevitably resulted in a certain fragmentation of research in the field, now much more focused on narrower topics and essentially driven by issue-related questions. Yet, and as this essay has hopefully suggested, this is not necessarily preventing progress in the field. Indeed, the broad series of factors that we have identified in our review and that are now widely considered in the literature—institutional dependencies, structural mismatches, and ideational-professional dynamics—are all issues of interest for international political economists. The fact that these different series of factors may combine in multiple and connected ways is not detrimental to a cumulative approach (see Steinmetz 2004). Indeed, it could hardly be otherwise: what unpacking regulation has ultimately offered is a revelation of the inherent contingency of formation, reproduction, and change in regulatory orders.

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CHAPTER 19

ACCUMULATION

DAVID BAILEY

ACCUMULATION

As a conceptualization of a concrete process, “accumulation” probably owes most intellectually to the Marxist tradition. In the first volume of *Capital*, Marx devoted the final two parts (out of eight) to a discussion of accumulation—both the “process of accumulation of capital,” and what he referred to as ‘so-called primitive accumulation.’ Likewise, the second volume of *Capital* ends with a chapter on “accumulation and reproduction on an expanded scale.” Accumulation was central to Marx’s efforts to theorize the social relations, and resultant processes, that constitute global capitalism. In the Marxist account, “accumulation” refers to the process whereby capitalists absorb the excess, or surplus, that is produced as a result of the exploitative capital-labor relations that characterize capitalist production. This surplus, once produced, must also be *realized*, as profit, through the sale of commodities produced during the exploitative production process. This profit is then re-used, now as an increased amount of capital, thereby returning to the (now expanded) process of production. It is, therefore, only when capital begets *more* capital—through the production and sale of profitable commodities—that accumulation has occurred:

In so far as accumulation actually takes place, the capitalist must have succeeded in selling his commodities, and in reconvert[ing] the money shaken loose from them into capital.

(Marx 1867, 710)

For Marx, this process of capital accumulation is necessary for capitalist society to reproduce itself. Without the possibility of accumulation there becomes no reason for capital to be invested in the process of production; and, without production, society cannot reproduce itself. Where there is capital, therefore, Marx also saw the need for both capitalist production and, therefore, capital accumulation. Equally necessary is the process of exploitation upon which accumulation rests. Accumulation also explains capitalism’s expansionary tendencies. As the amount of capital expands through a process of accumulation, so the scope and scale of capitalist production as a whole grows larger: accumulation witnesses “the production of capital on a progressively increasing scale” (727). It is for this reason that Marx described what he termed the “law of capitalist accumulation”: a process whereby capitalist relations exist on an ever-expanding scale—seeking new opportunities for investment in exploitative

production, increasing the population of workers that are subordinated to the wage relation, and expanding in the direction of a fully integrated world market. For Marx, this drive for capital accumulation results from the need for individual capitals to compete against each other:

The battle of competition is fought by the cheapening of commodities. The cheapness of commodities depends, all other circumstances remaining the same, on the productivity of labour, and this depends in turn on the scale of production. Therefore the larger capitals beat the smaller.

(Marx 1867, 777)

Marx's conceptualization of the process of capital accumulation has prompted a number of debates within Marxist political economy (Norton 2017). It informed Luxemburg's major work, *Accumulation of Capital* (Luxemburg 1913), which as Worth (2012) points out has significant potential to inform critical approaches to International Political Economy (IPE). Marx's account also highlighted the increasing reliance upon the means of production, and especially mechanization, that results from the drive for accumulation. The increased use of machines (and other means of production, such as buildings and transport) tends to be the most straightforward way in which to increase productivity and therefore to have a chance of surviving the "battle of competition." As a result, capital accumulation tends also to witness ongoing moves toward mechanization, and an increased reliance upon machines in proportion to the reliance upon waged labor:

The relative magnitude of the part of the price which represents the value of the means of production, or the constant part of the capital, is in direct proportion to the progress of accumulation, whereas the relative magnitude of the other part of the price, which represents the variable part of the capital, or the payment made for labour, is in inverse proportion to the progress of accumulation.

(Marx 1867, 774)

It is this tendency for an increased proportion of the means of production, in relation to the amount of wage labor, that later in Marx's analysis informs his controversial law of the tendential fall in the rate of profit (Marx 1894, 317–38). For many Marxists, it is this law which forms the basis of Marxist political economy, as the reduced reliance upon wage labor (ultimately the source of exploitation, and therefore profit) explains the ongoing tendency for capitalism to experience crisis (Kliman 2012). For others, the indeterminate nature of the relative prices of different parts of the production process, as well as the fact that no human system (including the capitalist socioeconomic system) can be considered a "closed system," results in a reluctance to attribute law-like tendencies to capitalism. Marxists skeptical of invariable "laws" have therefore considered capitalism to be an unstable, or crisis-prone, socioeconomic system, but not one that is necessarily and invariably destabilized by a single ("mono-causal") process, such as a falling rate of profit (Harvey 2016).

REGIMES OF ACCUMULATION

For those political economists who are willing to adopt Marx's concept of capital accumulation, but are nevertheless wary of law-like predictions linked to the rate of profit, there has

been an ongoing attempt to conceptualize the different ways in which efforts are made to stabilize the unstable nature of capitalism as a socioeconomic system of production. One of the most developed such attempts is French Regulation Theory (Boyer 1990). For regulation theorists, therefore, the concept of accumulation leads us also to consider different ways in which the conflicting pressures that arise from capital accumulation might be managed. These are typically understood as different “regimes of accumulation,” or institutional forms designed to contain capitalism’s crisis tendencies (Boyer 2018). The institutional forms that enable accumulation to occur are, however, themselves subject to destabilizing pressures arising from the unstable nature of capital accumulation: “the contradictions inherent in accumulation manifest themselves by the endogenous destabilization of any accumulation regime: this implies a major crisis” (Boyer 2018, 285).

For Regulation Theory, the key sources of destabilization that emerge in the process of capital accumulation, and which must therefore somehow be (temporarily) stabilized are: (1) the role of money, which prompts an untenable commodification of an ever-increasing range of social relations; (2) the related drive for competition between different capitals, which creates an inherent process of change, uncertainty, over-accumulation, and the (growing) potential for crisis; and (3) the capital-labor relation, which is inherently conflict-prone due to the inequality and exploitation that marks it. Efforts to stabilize these three destabilizing elements result from political conflicts between different social groups, the outcome of which tends to be absorbed and institutionalized within the state, with each social group having their own connections to different positions within the socioeconomic process of accumulation. This creates (4) a new destabilizing contradiction, between the demands of ongoing accumulation and the established political compromises and exclusions which characterize the state. Finally, (5) the state itself exists in a contradictory relationship with its territorial outside, as the expansionary tendencies arising from accumulation come into tension with the sovereignty of the state, as a result of the latter’s place within a global inter-state system (Boyer 2018; see also Boyer 1990).

For Regulation Theory, the way in which each of these five key contradictions are (temporarily) managed will determine and characterize the particular “regime of accumulation” that exists in any one place and time within global capitalism. This therefore opens up the possibility for *different* regimes of accumulation, each evincing different social compromises, and different (related) crisis tendencies. Initially, Regulation Theory focused on the Fordist regime of accumulation that emerged in the immediate postwar period (Aglietta 1979), in which wage labor was partly incorporated through productivity-linked wage rises, and the Keynesian welfare state served to regulate and manage the tensions of capitalist growth. This subsequently underwent a transition into a more finance-focused regime of accumulation, in which the finance sector is prioritized as a source of speculation and growth (Boyer 2000). Each of these two regimes of accumulation have tended to incorporate different actors, with different institutional compromises; although other commentators have also noted a broader variety of regimes of accumulation, echoing debates within the more mainstream comparative political economy literature regarding different varieties of capitalism (Amable 2003).

Accumulation regimes are not, we should note, guaranteed to secure capital accumulation. A common criticism of Regulation Theory relates to its alleged functionalism, in which capital accumulation is assumed to arise from a particular regime of accumulation, which in turn is assumed to have come into existence as a result of its ability to secure that accumulation. The “function” of the regime of accumulation becomes (implausibly) the reason

that it came into existence. In Bonefeld's (1987) terms, it represents a "functional interpretation of social phenomena [that] presupposes a definite logic of capitalist development" (121). Yet there is nothing within Regulation Theory which assumes functionality. As Matt Vidal (2013) argues, we should seek to identify *both* functional *and* dysfunctional accumulation regimes. For him, the financialized post-Fordist accumulation regime which emerged following the crisis of Fordism is a dysfunctional accumulation regime, characterized by a declining profit rate, underconsumption, and/or debt-led growth, all of which are associated with an *inability* to generate capital accumulation.

PRIMITIVE ACCUMULATION, SOCIAL REPRODUCTION, AND OTHER "BACKGROUND CONDITIONS"

Much of what has been discussed thus far has focused on the development of accumulation as a concept used to describe the process whereby capital, and capitalist societies, organize social relations and processes, so that capital can beget more capital, and thereby reproduce on an expanding scale. In this sense, we have been engaged in a relatively economic discussion. Such notions of accumulation, however, ignore what Nancy Fraser (2018) has termed "background conditions," meaning the "extra-economic" conditions upon which capital accumulation rests. Such economic accounts also ignore the dependence of capitalism upon an initial condition of scarcity, necessary (at least) in order that workers can be coerced into maintaining their part of the capital-labor relations. This condition of scarcity needs first to be created—a process described by Marx as "primitive accumulation"—and then needs to be constantly re-created, which Harvey has termed a process of "accumulation by dispossession" (Harvey 2003).

As Marxist feminists have perhaps done most to highlight, therefore, capital accumulation does not exist solely in the sphere of production, is not only the produce of waged labor, and does not only occur through the exchange of commodities (Gimenez 2019; Mies 1986). Each of these aspects of the "economy" require, and depend upon, their existence within much broader relations of social reproduction. This includes the care work which is necessary to produce humans as workers, and which is often unwaged, outside of the sphere of commodified production, and omitted from much of Marx's analysis in *Capital*. Any analysis of accumulation, therefore, needs also to consider the way in which these aspects of social reproduction are achieved (Ferguson 2016).

For Fraser, these "background conditions" include: the gendered sphere of social reproduction that is typically ignored by analyses of the formal sphere of commodity production; the non-human nature which is transformed into a mere resource to be used for capitalist production, without consideration for the harmful effects that such processes might have upon it; and the expropriation suffered by racialized "others," upon which much of global inequality rests. In seeking to understand the different ways in which accumulation occurs, therefore, we need also to consider the different ways in which these background conditions are dealt with. In Fraser's (2018) words, within any particular regime of accumulation, "the accumulation dynamic is shaped and channeled by a specific organization of its background

conditions.” This includes “a specific organization of social reproduction, including family forms and gender orders,” and a “a specific ecological organization, including characteristic ways of generating energy, extracting resources, and disposing of waste” (64–5). It also includes “regimes of racialized accumulation,” including both the process whereby primitive accumulation occurs in a necessary step to dispossess (and therefore bring into existence) members of the proletariat (and thereby ensure that they necessarily adopt the role of waged workers), *and* the racialized forms of inequality associated with colonization, postcolonialism, and associated patterns of unequal exchange, all of which contribute to the ongoing poverty experienced by racialized minorities across the Global South and within the Global North (101–8). These exclusions, Fraser rightly points out, are necessary for accumulation to occur, and must therefore also be considered in our efforts to understand and conceptualize the process of capital accumulation.

ACCUMULATION IN INTERNATIONAL POLITICAL ECONOMY

To summarize the discussion so far: capital accumulation is a concept developed with roots in the classical political economy literature, and especially Marxism. It describes the central process whereby capital is driven by competition to accumulate more capital. This occurs through: the reproduction of exploitative capital-labor relations in the sphere of production; and the sale of commodities in the sphere of the market. It rests upon a number of background conditions: a gendered sphere of social reproduction; an extractive relationship with nature; and racialized forms of expropriation. We turn now to address the question of how this conceptualization of “accumulation” features in the discipline of international political economy. In doing so, however, we immediately face the question of how IPE itself should be defined. As Pevehouse and Seabrooke (this volume) discuss, such discussions commonly trace the emergence of IPE as a discipline back to the end of the Bretton Woods system and the crises of the 1970s, and especially the shifting international relations that emerged within that context. This highlighted the importance of the global economy for understanding changing political relations, and as a result: “IPE was thus born out of a recognition that politics mattered for economics and vice-versa.”

Of the key founding contributions to the discipline of IPE that emerged in this context, those that focus most on the process of capital accumulation have their roots within the critical and/or Marxist strands of the discipline, and more broadly within the “British School” (if such a School exists), and its greater openness to qualitative, critical, and heterodox approaches (Cohen 2008). Of Cohen’s (2008) “pioneers” of IPE, accumulation was a concept considered by both Susan Strange and Robert Cox, both of whom are typically considered to sit within the “British School.”

As early as 1971, Susan Strange sought, in one of her seminal contributions, to develop a “political theory of international currencies.” Here, she sought to show how the currencies of dominant states were able to become international currencies within the global economy, as a result of those states’ capacity to display global leadership. For Strange, this was explicitly, “leadership *primarily in the accumulation of capital*” (306, emphasis added). Despite

her sympathy toward the use of the concept of capital accumulation, however, Strange was ultimately dismissive of what she considered to be a standard Marxist approach. This, she claimed, rested on “the old-fashioned notion,” that “before you invest you must accumulate capital by piling up this year’s profit on last year’s, that capitalism somehow depends on the accumulation of capital” (Strange 1994, 30).

For Robert Cox, the most critical (and Marxist) of Cohen’s (2008) “pioneers” of IPE, accumulation was far more central to his contribution to the field. Chapter 9 (of ten) of Cox’s *Production, Power, and World Order* is dedicated to discussing “mutations in the social structure of accumulation.” It is also made clear from the start of the book that production relations, which create the conditions within which political power operates, are themselves “considered within the framework of the changing structure of accumulation” (Cox 1987, 2). Indeed, for Cox, modern (capitalist) society is characterized by social relations of production, which imply “an economy driven forward by capital accumulation” (Cox 1987, 52). Likewise, global power relations were shaped by (and in turn shape) the pressures that emerge from a particular (global) mode of accumulation within global capitalism. Specifically, the Fordist mode of accumulation generated pressures for change that were derived from the increased (and increasingly excessive) bargaining power that Fordism had offered to organized wage labor. This prompted changes in the sphere of production, in political power, and in the relationship between different social groups. In Cox’s own words:

In order to grasp whether and in what way the global accumulation process may be changing, it is necessary to ascertain the mutations taking place in the relationships among different technologies of production and the modes of social relations associated with them.

(Cox 1987, 318)

This focus on accumulation has continued to inform many of the critical contributions to IPE.

We can see this influence in many of the key critical contributions to IPE of the 1990s and 2000s. Jeffrey Harrod, Cox’s closest collaborator, focused on the way in which the changing patterns of global capital accumulation that marked the late twentieth century were especially characterized by changes to the nature of work and the removal of protections for organized labor, creating an increase in the number of unprotected workers. This was associated with new patterns of accumulation which were (and still are) less dependent upon “the continuing employment of the same persons” (Harrod and O’Brien 2002, 12). Work by Stephen Gill sought also to highlight the way in which neoliberal globalization represented a new mode of accumulation based around more flexible patterns of production and driven by “large-scale,” “internationally-mobile” transnational capital (Gill 2008, 76). Also drawing on the work of both Cox and Harrod, Matt Davies and Magnus Ryner (2000) compiled a collection of studies exploring changes to the global political economy, highlighting the way in which post-Fordist patterns of accumulation are characterized by flexibilization and in turn produce poverty amongst a growing proportion of unprotected workers.

Many of the critical contributions to IPE have also sought to stress the role of extra-economic conditions in enabling capital accumulation. Isabella Bakker (2003) has highlighted the way that the move to neoliberalism was accompanied by a silencing of questions of (gendered) social reproduction, social provision, and care, all of which were increasingly privatized. Sylvia Federici has highlighted the impact of the neoliberal transformation upon the Global South, creating a situation in which reduced public spending

has heightened gendered patterns of global poverty (Federici 2006, 121). Finally, Matthew Paterson (2000) has highlighted the role of capital accumulation in understanding global environmental politics, drawing attention especially to the ecological damage generated by the drive for accumulation and expansion.

ACCUMULATION AND THE IPE OF CONTEMPORARY GLOBAL CAPITALISM

Contemporary contributions to IPE continue to draw upon the concept of accumulation in order to grasp the key power relations, pressures, processes, and outcomes that constitute contemporary global capitalism. Again, perhaps unsurprisingly, this has tended to be the focus predominantly of more critical scholars, who have sought to highlight the way in which the contemporary process of capital accumulation has generated a number of observable trends noted within the recent IPE literature. What follows is a summary of some of these key claims, drawing especially from those publications that Seabrooke and Young (2017) note are more sympathetic toward the more critical, qualitative, and “British” branches of the discipline, including journals such as *Review of International Political Economy*, *New Political Economy*, *Globalizations*, and *Capital & Class*, and to which we might also add two book series—Routledge/RIPE Series in Global Political Economy, and the Palgrave International Political Economy series.

Post-Fordism, Neoliberalism, and Flexible Accumulation

In those approaches within IPE which consider the process of capital accumulation, perhaps the central focus (as we have already seen above) is on the specific global model of accumulation that has been adopted since the crisis of the 1970s. Here, the move toward a post-Fordist, or neoliberal, mode of accumulation is central to the analysis. This refers to the way in which a more global mode of accumulation was adopted following the crises of the 1970s. For Dunn, this inaugurated “new forms of accumulation, which were now more extensive than intensive.” It relied in part on a turn to austerity in order to construct renewed opportunities for capital accumulation, especially in high-income countries (Dunn 2014), and an increased subordination of various elements of public life to the demands of capital accumulation, including public institutions such as universities (Rikap and Harari-Kermadec 2019). It has also included a greater reliance upon liberalized labor markets, a heightened incidence of precarious work, and a generalized decline in the wage share (Bengtsson and Ryner 2015), all of which has tended to be gendered in its impact (Roberts 2017), and especially onerous for immigrant labor (Pradella and Cillo 2015). All of these processes, moreover, have been advanced further still by the more recent move toward the digitalization of work, which has been heightened by the experience of precarity (Shibata 2019), and fostered by a concentration of digital capital (Upchurch and Moore 2018, 56). This increased subordination of both labor and public policies, to the demands and interests of capital, has been described in terms of being a form of “flexible accumulation” (Carroll et al. 2019). From the perspective of

global capital accumulation, it represents a “new international division of labour,” and therefore an expression of “the essentially global contradictory dynamics of the accumulation of the total social capital” (Charnock and Starosta 2018, 335).

Financial Accumulation

Alongside the move toward a post-Fordist, neoliberal, and/or flexible mode of accumulation, accumulation-focused accounts in IPE have done considerable work to delineate the role and impact of finance as central to global patterns of capital accumulation (Woodley 2020). For many, financialization represents a new way of securing accumulation, achieved through financial means rather than productive investment (Krippner 2005; Boyer 2000). For Soederberg (2014), this is a system of “credit-led accumulation” (8), in which accumulation rests on a condition of “debtfare,” where debt is both necessary for those in poverty in order to “augment and/or replace the living wage or government benefit cheque” (3), whilst at the same time serving to impose overwhelming disciplinary pressure upon those who are indebted. This reliance upon debt for contemporary capital accumulation, moreover, acts to construct and consolidate gendered patterns of inequality (Young 2019). A growing role for microfinance has also been noted, in facilitating the incorporation of marginalized populations, especially in the Global South, into contemporary processes of accumulation (Langevin 2019; Clarke 2019; Roberts and Zulfiqar 2019). Finance-led accumulation is not (and cannot be) entirely divorced from the sphere of production and the conditions of global labor, however, as finance represents one element in the wider circuit of capital accumulation upon which it necessarily rests (Bernards 2019).

Much has also been done to highlight the territorially-specific political institutions which have enabled, and shaped, the expanded role of finance within the neoliberal mode of capital accumulation. Benquet and Bourgeron (2020) show how the post-dirigiste French state enabled the private equity sector to become a center of accumulation in French capitalism. The six Arab states of the Gulf Cooperation Council (GCC) similarly facilitated the growing role of Islamic Finance in capital accumulation rooted in the GCC countries (Hanieh 2019).

The holistic perspective that arises from an accumulation-focused account also acts to re-orient our attention toward the *wider* circuit of financialized accumulation, and especially its continued dependence upon labor, in order to highlight the *limits* of financial accumulation (Bernards 2019).

Neoliberal Accumulation and the Global South

Neoliberal capital accumulation has had a significant impact upon global inequalities, and especially the relationship between the Global North and South (including the peripheral, lower income countries, or “emerging markets”). As Alami (2020) notes, financialization has produced a propensity for financial bubbles to emerge as financial capital seeks flight from lower growth (more developed) regions of the global economy, and a subsequent bursting of those bubbles when over-speculation, over-accumulation, and economic instability prompt a return to safer and secure investments. All of this necessitates state policies in order to pursue efforts at “managing class relations and fostering capital accumulation” (6), which are

themselves part of a global process of “capital valorisation” (meaning the attempt to secure the accumulation of capital) (12).

Contributions to IPE have highlighted the way in which the globalized neoliberal model of accumulation has seen the insertion of the Global South into global supply chains in such a way that ensures that countries of the South remain dependent upon their role as suppliers of raw materials, serving to consolidate North-South inequalities (van der Merwe and Dodd 2019). This also highlights the importance of adopting alternative capital accumulation strategies for states seeking to implement developmental policies in emerging markets, otherwise the risk is that development strategies act to maintain positions of global structural weakness (Wylde 2017). For instance, the left-leaning government of Morales’ Movement Towards Socialism (MAS) rested upon an extractive model, in which “the export and extraction of natural resources have come to drive capital accumulation” (4), and which as a result consolidated the dominance of transnational capital over the Bolivian working classes and indigenous people (McNelly 2019). Similarly, in China, the Communist regime’s domestic growth strategy has served to accommodate and consolidate “global capital’s flexible accumulation” (Lee 2019, 141). Finally, the dependence on natural resources for many of the modes of accumulation witnessed within the Global South also has an effect upon the international relations of those countries. For instance, Claar (2018) discusses the role of South Africa’s accumulation regime, based around a “minerals-energy complex,” and the way this influences South Africa’s trade relations.

Governing and Legitimizing Contemporary Patterns of Accumulation

Accumulation-focused accounts in IPE have also drawn our attention toward the broader political requirements of contemporary global capitalism, including those methods of legitimization and patterns of supranational/global governance which oversee global capital accumulation.

Political regimes governing the contemporary neoliberal mode of accumulation have become increasingly authoritarian—both in the Global North, and amongst the so-called “Rising Powers” (Bruff 2014; Tansel 2019). This also includes increased disciplinary mechanisms being imposed on women, as a way of enforcing the gendered nature of precarization (Roberts 2017). Others have noted the role of changing patterns of global and regional governance in accompanying and promoting the move toward transnational patterns of production, for instance in the sphere of European Union (EU) competition policy (Buch-Hansen and Wigger 2011), and as advanced by the Organisation for Economic Co-operation and Development (OECD) (Cammack 2019). The shift in decision-making to supranational institutions, such as the EU, has also acted to depoliticize unpopular decisions and thereby “reproduce effective conditions for capital accumulation while sidestepping the political obstacles undermining it” (Moraitis 2019, 4). Legitimation strategies specific to the heightened importance of finance capital have also witnessed a resort to the use of (relatively insubstantial) bank fines, as a means by which to seek political legitimization in a form that is compatible with ongoing financialized accumulation (Macartney and Calcagno 2019).

Social Reproduction

In the sphere of social reproduction, accumulation-focused accounts in IPE have continued to assert the importance of the “need to widen conceptions of capitalism to include the social relations of gender *and* the feminized relations of social reproduction, which are *integral* to capitalist reproduction yet rendered invisible in most literature in the social sciences” (Roberts 2017, 5; see also Bakker and Gill 2019). This includes a focus on the role of (mainly) women in performing underpaid and often unfree labor in global value chains (especially in the Global South) (Gore and LeBaron 2019). Social reproduction theory also highlights the way in which inequalities between households are not simply a process of reproducing existing inequalities, but rather are related to (and should therefore be understood as part of) a broader pattern of global capital accumulation. As Nunn and Tepe-Belfrage (2019) describe, for instance, we can see in education policy conscious attempts by the state, “to ensure that parenting behaviour raises aspiration and activity designed to reproduce effective labour power” (630).

Contemporary Accumulation and the Environment

In terms of considering the impact of contemporary accumulation processes upon the environment, several recent contributions to IPE have noted the contradictory relationship between neoliberal accumulation and climate change. Thus, beginning with a starting assumption that “all states in market economies maintain the first-principle goal of actively fostering capital accumulation” (6), MacNeil (2017) shows how climate policy has developed *despite* the prevalence of a neoliberal, deregulatory agenda, witnessing various attempts by policymakers to promote clean energy innovation (itself necessary for successful capital accumulation) (see also Lachapelle et al. 2017). This also speaks to a broader observation, that larger firms are increasingly using their dominant position within the market, acquired through the process of competitive accumulation, to gain an advantage over other firms in terms of bearing the costs, and reaping the rewards, of a sustainability agenda. As such, sustainability is “leveraged for capital accumulation and to ensure competitive advantage” (Ponte 2020, 75). Similarly, for some, finance—key to the neoliberal mode of accumulation—has the potential to facilitate the transition “from a carbon economy to a climate capitalism where new and attractive sites of accumulation are created in a lower carbon economy” (Newell 2019).

Resistance and Accumulation Strategies

Particular modes of accumulation can also be expected both to prompt particular types of resistance, and in turn to adapt and change in such a way that they are affected by those forms of resistance that they encounter (Bailey et al. 2021). As such, a number of recent studies have highlighted the way in which neoliberal accumulation, and the post-2008 crises of neoliberal accumulation, have prompted a range of forms of resistance. Powell and Yurchenko (2020) consider this relationship in terms of it being a “state-society-capital

complex,” in which “capital must be treated as a force that is shaped by and shapes the state and society alike” (97). In doing so, they chart the way in which particular types of social pressure arising from different types of civil society mobilization have produced different variations of neoliberalization. In the case of water privatization, Powell and Yurchenko show how a strong civil society (as witnessed in Berlin) might result in a reversal of privatization, albeit in a way that (partially) accommodates the ongoing requirements of capital accumulation. Huke et al. (2015) have also sought to describe how the austerity agenda that was adopted across much of Europe following the global economic crisis has nevertheless witnessed an impressive array of different types of resistance. Whilst neoliberalism has tended to coincide with the erosion of power of some of the key institutions, such as trade unions, that in the past enabled organized labor to resist the more intense effects of capital accumulation; the capacity for resistance has not been entirely eradicated and instead is perhaps better understood as having undergone a process of change (Ribera-Almandoz et al. 2020; Hesketh 2017).

CONCLUSION

Accumulation remains a central concept to IPE. In zooming in on perhaps the central process through which global capitalism has been driven toward expansion, at the same time, it opens up the space for a critical and holistic account of core processes of central interest across the field of contemporary IPE: neoliberalism, financialization, mechanization, global inequality, racialized and gendered exclusions, ecological destruction, changing patterns of resistance, and the inter-connected global relationship between each of these phenomena. It is for this reason, perhaps, that we should expect accumulation to continue to feature in contemporary IPE.

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PART IV

FORUMS

CHAPTER 20

INTERGOVERNMENTAL ORGANIZATIONS

ANDRÉ BROOME

INTRODUCTION

INTERGOVERNMENTAL organizations (IGOs) structure processes and patterns of decision-making in defined areas of transnational governance between different actors in the international political economy. Created by formal treaty agreements between states, the reach of IGOs' authority and the impact of their policies and practices can subsequently shape decision-making processes undertaken not only by states themselves but also non-governmental organizations (NGOs), firms, households, and informal organizations. This sometimes leads to an assumption that IGOs constitute a homogenous category of institutionalized agents in world politics, which share similar organizational characteristics, operate under similar political pressures, are characterized by similar political accountability problems, and are driven by similar bureaucratic imperatives to defend their own policy turf and to maximize their own organizational interests (Vaubel 2006). Yet, while it is widely recognized in contemporary scholarship in political science and international relations that states differ markedly in their histories, capacities, problems, interests, norms, and preferences, dissimilarities among IGOs are potentially even greater than they are among states despite their shared institutional attributes.

While International Political Economy (IPE) is a young field (Editors' Introduction), the study of IGOs formed an integral part of its early development as the formal structure of the Bretton Woods international monetary system staggered then collapsed in the late 1960s and early 1970s (Krasner 1968; Cox and Jacobsen 1973).¹ As the field has evolved and expanded following watershed events such as the suspension of dollar convertibility into gold at a fixed rate of exchange with the "Nixon shocks" in 1971 and the oil price shocks in 1973, IGOs have remained a core research focus throughout successive waves of new IPE scholarship. Among many other scholarly developments after the breakdown of the Bretton Woods system, this has included the rise of regime theory (Krasner 1982), the study of international norms (Finnemore 1993), the expansion of the membership and global scope of IGOs following the collapse of centrally-planned economic systems and the demise of the Soviet Union

(Epstein 2008), the dynamic challenges of increasing levels of interdependence and market liberalization associated with economic globalization (Woods 2006), and the increasing complexity and density of many areas of transnational governance as IGOs engage in “orchestration” to enhance their governance capabilities (Abbott et al. 2015) or are forced to collaborate in areas where their objectives and responsibilities overlap (Kranke 2020). In each of these areas of inquiry, IPE scholars have examined the distinct role IGOs play in fostering policy coordination via international regimes, defining and promoting international norms, overseeing fundamental processes of structural transformation toward market-based economic systems, and promoting sweeping liberalization of policy restrictions to encourage international economic integration and the growth of global trade and capital flows.

Despite occasional attempts at bridge-building between different theoretical approaches in the field, the study of IGOs is largely structured by what the editors of this volume term “constructive non-engagement.” Scholars work along parallel tracks, often addressing similar questions using distinct conceptual and methodological toolkits. For this reason, after a brief discussion of the differences in how scholars conceptualize the ontological status of IGOs, the following discussion identifies three core areas of IGO scholarship, which examine how IGOs function as agents of control, coordination, and expertise in the international political economy. These three areas of inquiry all treat IGOs as important agents, but scholars disagree about the key drivers of action, the primary sources and dynamics of change, and the political and economic consequences of the roles that IGOs play. Each set of research problems represents an analytic focal point in current scholarship; they are not fixed research agendas developing in isolation within exclusive boundaries. The primary analytic focal point in each problem set also cuts across the others in important ways.

THE ONTOLOGICAL STATUS OF INTERGOVERNMENTAL ORGANIZATIONS IN WORLD POLITICS

Scholars in IPE rely on different ontological starting points to investigate the roles and influence of IGOs. An important area of debate hinges on the question of whether IGOs should be analyzed a priori as agents of their member state principals (in rationalist approaches), or as independent and autonomous agents in world politics (in constructivist and sociological approaches) (Barnett and Finnemore 2004; Hawkins et al. 2006). In a special issue of *International Organization* on the “rational design of international institutions,” Koremenos, Lipson, and Snidal (2000, 762) provide a terse summary of the distinctions between three different perspectives on the status of IGOs, according to which rationalist approaches assume “states use international institutions to further their own goals”; constructivist approaches assume “international institutions play a vital, independent role in spreading global norms”; and realist approaches assume international institutions “are little more than ciphers for state power.”

These theoretical starting points have produced very different analyses of the same processes of structural change. An example is the rich body of studies examining processes of “legalization” in world politics. Here, analyses of the same research problems differ

depending on whether states are assumed to choose to use IGOs to improve the credibility of their policy commitments for markets (Simmons 2000), or whether these choices are assumed to be shaped by IGOs' efforts to push states to rethink the limits, modes, and objectives of economic policymaking in the first place (Finnemore and Toope 2001, 751–2). As this example illustrates, these disagreements are not simply ontological debates about the status of IGOs as agents in world politics, but reflect different sets of assumptions about causal sequences, the factors that influence how national decision-makers interpret their exogenous environment and envision the scope of their policy choice set, and the dynamics of political change.

Koremenos, Lipson, and Snidal argue against constructivist approaches on the basis that “it is misleading to think of international institutions solely as outside forces or exogenous actors” (Koremenos et al. 2000, 762). This overstates the assumptions made by scholars who draw on constructivist and sociological frameworks for the study of IGOs, which seldom conceptualize IGOs as being capable of operating with complete autonomy from powerful member states. At the same time, this claim underplays the specific circumstances in which IGOs do function as “outside forces” in their interactions with weaker states in the international political economy. These may include less-developed countries, low-income countries with very high public debt levels, newly formed states and new membership applicants, and distressed economies seeking external financing in a crisis. Moreover, the notion that states pursue their own objectives through IGOs is not incompatible with the idea that IGOs can *also* be powerful diffusers of global norms that are not always subject to control by individual states or groups of states.

In practice, scholars' ontological assumptions about the status of IGOs as agents vis-à-vis their member states do not usually structure their research in deterministic way. Scholars applying principal-agent models frequently investigate empirically the conditions that allow IGOs to exercise greater latitude and autonomy from their member state principals through the concept of “agency slack” (Copelovitch 2010). Those working within constructivist and sociological approaches similarly investigate the conditions in which the autonomy of IGOs is tightly circumscribed by powerful states (Momani 2004). Meanwhile, bridge-building scholarship has attempted to combine the insights of both rationalist and constructivist perspectives (Chwieroth 2013; Clegg 2010; Nielson et al. 2006). These contributions tend to place each approach side-by-side as different analytic cuts on the same research problem rather than integrating them into a holistic approach.

AGENTS OF CONTROL

How do intergovernmental organizations, created by sovereign states, exercise influence over national economic decision-making? The most common analytic focal points for the study of IGOs within the field of IPE over the course of its comparatively short history are the first two: IGOs as *agents of control* and as *agents of coordination*. In the first focal point, prominent scholarly contributions have long engaged with research problems connected to how IGOs exercise direct forms of power in world politics (Barnett and Duvall 2005). Regardless of whether IGOs are conceptualized as being closer to independent, autonomous agents or more like agents tightly controlled by major power principals, IPE scholars

have concentrated on understanding and explaining how they exercise influence via direct relations of power with states (Momani 2004; Dreher and Jensen 2007). These power relations are based on an implicit social hierarchy in the international system, in which the evolution of different IGOs is often characterized by high levels of path dependence whereby their respective organizational roles, policies, and practices continue to be shaped by their foundational designs (Hanrieder 2015). Arguably, the only attribute they universally share is that “IGO’s have ambitious governance goals but moderate governance capacity” (Abbott et al. 2015, 3). IGOs cannot exercise uniform authority to enforce compliance with common rules and normative standards across member states with diverse capacities that occupy sharply different structural positions in the international political economy.

The principal example of IGOs as agents of control exercising direct relations of power in the international political economy is *policy conditionality*. Policy conditionality is prominently associated with International Monetary Fund (IMF) and World Bank (the Bank) loan programs for borrowing member states (Babb and Carruthers 2008; Kentikelenis et al. 2016), as well as, less prominently, the lending practices of regional and subregional multilateral development banks (Park and Strand 2015). As a direct infringement on borrowing states’ sovereignty to make economic policy choices and on democratic processes of political accountability, conditionality has remained highly controversial throughout the history of its use by both the Fund and the Bank (Best 2012). IPE scholars have repeatedly shown that most states borrow from the Fund only in circumstances of economic distress when they have few or no alternative options available (Roos 2019), while low-income borrowers from the Bank struggle to access unconditional development financing and debt-service financing from other sources (Morrison 2011). These studies are empirically rich and theoretically diverse, ranging from theories of structural power to game theory. They provide important analyses of how the financial resources of two of the most prominent IGOs in the international political economy are used to control national economic decision-making, sometimes with disastrous effects on domestic welfare, economic and social inequalities, and growth (Kentikelenis 2018; Vreeland 2002).

Relations between the Fund, the Bank and their borrowers are dynamic rather than static. They are subject to change over time based on the evolving organizational cultures, governance arrangements, policy paradigms, and operational practices of the IGOs themselves (Best 2014; Güven 2018; Weaver 2009). In addition, they evolve based on changes in economic governance, policy orientation, and the relative gains or losses in status of individual states as they move up or down a social hierarchy and shift position in structures of wealth accumulation in the international political economy (Ferdinand and Wang 2013; Hameiri and Jones 2018). For these reasons, while the analysis of the tensions inherent in the efforts of IGOs to exercise direct control over national economic decision-making continues to advance with each new generation of IPE scholars, it often does not aim to produce “generalizable” theories of IGO–state relations. Regardless of theoretical and methodological choices, such scholarship is most productive when research problems are examined within clearly defined organizational contexts, patterns of interaction, modes of governance, and political processes. This has led to the accumulation of rich empirical knowledge about how IGOs serve as agents of control in the international political economy that provides a key starting point for future studies. Yet, for the reasons noted above, earlier scholarship may not have a high predictive capacity when analytic insights are applied to different IGOs (such as the majority of IGOs that lack the resources, tools,

and status of the Fund and the Bank) or even from one instance of a class of events (such as economic crises) to another.

Research on IGOs as agents of control in the international political economy expanded further after the Asian financial crisis in 1997–1998 starkly revealed the fallibility of the Fund as an international crisis manager (Wade 1998). Over the decade between the Asian financial crisis and the onset of the global financial crisis in 2008 scholars developed new insights that built on research into external sources of dysfunction in IGOs by shifting their focus to internal sources of dysfunction, what Barnett and Finnemore (2004) term “pathologies.” This provided the impetus for a new wave of scholarship that sought to reveal how the internal workings of IGOs are integral to explaining their roles, sources of influence, and the causal impact of their actions in the international political economy. These contributions helped to refine more nuanced explanations for IGOs’ influence over states (Lombardi and Woods 2008), the political conditions that limit their influence (Cooper and Momani 2005), and the complex mechanisms through which key decisions, policy preferences, and operational practices within IGOs have endogenous as well as exogenous drivers (Clegg 2014).

AGENTS OF COORDINATION

Closely related to the roles IGOs perform as agents of control in the international political economy are their policy coordination roles, which can serve to “lock-in” and “stabilize” patterns of cooperation between states. As Martin and Simmons (1998, 745) have suggested, “To be effective, institutions cannot merely solve collaboration problems [between states] through monitoring and other informational functions. They must also provide a mechanism for resolving distributional conflict.” Extensive IPE scholarship has examined the mechanisms through which IGOs perform a coordinative function by formulating, negotiating, and monitoring the implementation of rules, and diffusing normative standards, reform templates for institutional design, and “best practice” policy models (Berge and St John 2021; Park and Vetterlein 2010; Kentikelenis and Babb 2019; Steffek and Wegmann 2021). At the level of national policy elites, research has demonstrated how coordination occurs when reform-oriented policymakers serve as “sympathetic interlocutors” in interactions with IGO officials that share common economic beliefs (Chwioroth 2009; Nelson 2017; Woods 2006). For junior and mid-level officials, especially those serving in policy environments more resistant to liberalization reforms, coordination is an incremental process of generational policy change. This is often centered on socialization mechanisms embedded within capacity building programs, which revolve around technical assistance missions by IGO staff and training courses for national officials in international policy training institutes (Broome and Seabrooke 2015).

Such mechanisms of coordination impose constraints on national economic decision-making, even if these are less rigid, less enforceable, and less amenable to empirical observation compared with the greater degree of control afforded by policy conditionality. When conditionality goes together with coordination processes, however, this can significantly amplify the long-term influence IGOs exercise over the economic policy settings of their member states. For example, scholars have shown how efforts by IGOs to coordinate national economic policies toward an “open economy” or “neoliberal” model and to reconfigure the

design of key economic institutions such as central banks around isomorphic templates have had the effect of “shrinking” the policy space available to decision-makers in many developing countries (Ban 2016; Johnson 2016; Kentikelenis et al. 2016).

The impact of IGOs on limiting national policy space is also observable for member states of the World Trade Organization (WTO) (Wade 2003). Scholars have investigated how policy adaptation and liberalization takes place through variegated degrees of membership conditionality linked to the WTO accession process (Pelc 2011), surveillance practices via the Trade Policy Review Mechanism to systematically monitor member states’ compliance with their WTO obligations (Laird and Valdés 2012), and, most prominently, the policy trade-offs integral to ongoing multilateral negotiations over international trade rules (Narlikar 2003). Yet, despite the political controversies surrounding the WTO as a forum for trade negotiations and the high level of public and scholarly attention it receives, Odell’s (2009) research suggests the organization has limited influence over the outcomes of negotiations. Instead, negotiation analysis has shown how deadlocks in multilateral trade talks are most likely to succeed or fail based on the negotiating strategies and tactics of country delegations (Odell 2009). Other contributions have examined how the WTO system has created an arena for the use of discursive power to frame the scope and objectives of trade negotiations, the logic of trade liberalization, and the classification of trade policy issues and problems (Eagleton-Pierce 2013; Wilkinson 2009).

At the same time as the international trade regime has undergone an intensifying process of “legalization” in the twenty-first century, both old and new major power states have effectively crafted strategies to maximize their own policy discretion to gain national advantages within existing multilateral rules (Weiss 2005; Hopewell 2019). Indeed, many states now share a preference for conducting bilateral and regional trade negotiations outside the auspices of the WTO (Hopewell 2021; Ravenhill 2017). This reveals the limits of conceptualizing the organization as the “third pillar” of contemporary global economic governance alongside the Bank and the Fund, given the fundamental distinctions between the WTO and the two Bretton Woods institutions in terms of their mandates, powers, resources, and the degrees of autonomy and influence they can exercise independently from member state control. The organization can potentially exercise greater influence as an agent of coordination in the international political economy via the one avenue of influence not available to either the Bank or the Fund: the dispute settlement mechanism of the WTO. Through highly institutionalized procedures for adjudicating trade disputes between members, the WTO not only restricts the scope of members’ trade policies but also enables new classifications of trade issues and the creation of legal precedents, functioning as “an interpreter of the balance between rights and obligations” for states in terms of trade governance (Mortensen 2012, 88).

In contrast to the WTO, the Fund, and the Bank, many other IGOs serve as agents of coordination via mechanisms that cannot rely on financial resources, loan conditionality, or adjudicatory power. Without a lending facility or a mandate and resources to undertake crisis management responsibilities, the Organisation for Economic Co-operation and Development (OECD) relies heavily on informational functions to facilitate policy coordination among member states. Yet, as IGO scholars have shown, the OECD’s knowledge practices and surveillance processes have been highly influential in homogenizing and standardizing policy settings, governance practices, data collection systems, and administrative procedures among its members in areas ranging from tax governance (Sharman 2012) to labor market and social policies (Mahon and McBride 2009) and development aid

(Nay 2014). These contributions have demonstrated that—despite the OECD lacking more coercive instruments of leverage—IGOs that are embedded in the “construction, standardization, and dissemination of transnational policy ideas” (Mahon and McBride 2009, 83) enjoy an important position as nodes in transnational governance networks.

Non-lending IGOs such as the OECD cannot directly function as agents of control over their members states like the Fund or the Bank, and most cannot exercise adjudicatory power over state disputes like the WTO. However, through these mechanisms of coordination the OECD can potentially gain control of how key issues are defined, understood, and governed by states (Seabrooke and Henriksen 2017, 5). This grants the organization significant leverage to shape patterns of change and reforms in national systems of economic governance and to facilitate international policy coordination, as well as directly influencing the evolution of international development aid practices and the policies of major donor agencies. Indeed, in circumstances where an IGO can develop and maintain a position of “network power” over time, this potentially affords greater influence over how markets are governed by states than when policy reforms result from short-term bargaining games incentivized via conditionality mechanisms during an episode of crisis. However, assigning relative causal weight to different mechanisms of coordination is difficult to establish and test empirically.

AGENTS OF EXPERTISE

The final analytic focal point in contemporary IPE scholarship on IGOs discussed in this chapter is the role they play as *agents of expertise*. Organizations such as the Fund, the Bank, and the OECD maintain large professional bureaucracies that perform influential functions in the international political economy as producers, curators, and diffusers of “transnational economic knowledge.” Although it has often been relegated below more direct and coercive mechanisms of control and coordination, their role as agents of expertise is fundamental for understanding how IGOs exercise power in the international political economy. Despite the financial resources it controls and the capacity to enforce strict policy conditionality, for example, Barnett and Finnemore (2004, 50) argue that “The ability to use expertise and knowledge is the Fund’s most important source of autonomy.”

The extent to which IGOs can influence national economic policymaking therefore depends to a significant degree on their “standing as sources of expertise” (Herold et al. 2021, 671). Because an important source of their authority over states and other actors flows from an ability to develop and sustain their status as “experts” within specific policy domains, IGOs actively construct, nurture, and maintain deliberate boundaries to demarcate their governance jurisdictions and to thereby “craft perceptions of exclusive expertise” (Kranke 2022a). An IGO has the potential to construct expertise as a form of “cognitive authority” in world politics when officials possess high levels of formal academic qualifications and are recognized as members of an elite transnational professional community (such as economists), and when subordinate actors perceive that it enjoys an asymmetric institutional position, can draw on unique practical experience, or has privileged access to information, models, and other exclusive tools of governance knowledge (Broome and Seabrooke 2015).

When IGOs function as agents of expertise in the international political economy they can draw on the recognition of their cognitive authority as a means of exercising power to

classify and define what constitutes “good” economic policy settings, objectives, and institutional designs (Barnett and Fennemore 2004, 32). This may enable them to establish the “causes” of economic crises (Hall 2003), and to construct a narrative around historical analogies that frames and limits policy debates over “crisis prevention, crisis management, and crisis resolution” (Samman 2019, 99–100). In addition, the production of transnational knowledge on the comparative policy settings and economic performance of their member states can also enhance the effectiveness of IGOs’ roles as agents of control and coordination. By increasing the “legibility” and the “transparency” of national economic policymaking, this can enable IGOs to achieve greater traction in their provision of policy advice through surveillance mechanisms, capacity building programs, the negotiation of loan conditions, or monitoring domestic implementation and compliance with international rules. The WTO’s Trade Policy Review mechanism, for example, reduces information asymmetries about member states’ trade policies and practices, which can be used to assess compliance with their obligations (Laird and Valdés 2012).

Governance techniques such as “blacklisting” or “global benchmarking” represent the sharp end of the stick in how IGOs make use of expertise to shape national economic decision-making. Here, transnational knowledge constructed by IGOs is used coercively to not only label economic policies “good” and “bad,” but to establish an international hierarchy among states that challenges sovereign reputations and uses social pressure to “stigmatize” those deemed as poor performers (Sharman 2009). The practice of benchmarking state performance through country rankings and ratings has become increasingly prevalent for organizations ranging from the OECD and the World Bank to the International Labor Organization (Broome et al. 2018; Doshi et al. 2019; Weisband 2000).

The expertise claims of IGOs do not go unchallenged (Kramarz and Momani 2013), and their cognitive authority must be recognized by target audiences if it is to be influential in shaping national economic decision-making. The willingness of state actors to recognize and defer to the authority of an IGO’s expertise in particular governance settings is a relationship of authority that remains contingent and can potentially be undone (Sending 2015, 5). For instance, IPE scholars have shown how prominent international economic statistics produced and disseminated by IGOs routinely suffer from important defects (Linsi and Mügge 2019). Moreover, some IGOs operate in a highly competitive environment, where states may receive conflicting policy advice from different IGOs in the same area of governance (Breen et al. 2020) or where interactions between IGOs become dysfunctional and are characterized by interorganizational insulation rather than collaboration (Kranke 2022b). This can potentially create opportunities for national decision-makers to engage in epistemic arbitrage (Seabrooke 2014), or to selectively draw on the authority of an IGO’s policy advice only when this is convenient for advancing their existing preferences in domestic policy contests.

CONCLUSION

IPE scholars have shown how intergovernmental organizations perform a wide range of governance roles in world politics. This chapter has selectively discussed three core areas of scholarship on IGOs, focusing on their functions as agents of control, coordination, and

expertise. Like other areas of inquiry in IPE, scholars' interests in studying IGOs are driven by research problems requiring different types of answers than those available from other subfields of inquiry within political science and international relations or from international economics. Most IPE research on IGOs is theory-informed rather than theory-determined, with scholars aiming to tease out conceptually how findings from their cases may hold important implications for others. This trend is common to both large-n and small-n studies of IGOs in IPE, as well as to normative scholarship on how IGOs ought to be reformed to improve global economic justice and global democracy (Brassett and Tsingou 2011; Dellmuth et al. 2019). However, there is a clear orientation toward addressing empirical problems and puzzles about IGOs. This problem-driven approach cuts across different research communities in IPE, with theory most often used as a tool to provide analytic leverage and gain knowledge about how IGOs are formed, how their governance architectures, operations, and relations with other actors in the international political economy change over time and why, how they exercise power and authority in world politics, and what limits exist (or should exist) on how IGOs seek to implement and operationalize their mandates.

NOTE

1. For an extensive historical overview of the study of "international institutions" in international relations, see Martin and Simmons 1998.

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CHAPTER 21

INFORMALITY IN INTERNATIONAL POLITICAL ECONOMY

FELICITY VABULAS

INTRODUCTION

THE field of International Political Economy (IPE) has largely focused on *binding* international agreements that govern interstate flows of goods, capital, and people. Yet at the turn of the twenty-first century, political scientists and international lawyers called attention to *informal* cooperation because it increasingly defined how states were interacting (Goldstein et al. 2000; Lipson 1991; Abbott and Snidal 2000). From international finance to monetary policy, state now often use non-binding or informal arrangements rather than formal international treaties to govern the global economy (Vabulas and Snidal 2013, 2020).

This chapter therefore focuses on the following question: How does informality affect outcomes in the international political economy? While IPE is indeed a fledgling field—as the editors of this volume assert—the study of informality specifically related to IPE is downright green. Scholars examining informality have started from a generalist’s perspective. They have sought to understand how and why actors cooperate in ways that contrast with “hard rules on the book,” but there is by no means a coherent body of “informality in IPE” work nor comprehensive answers to this research question. Moreover, echoing the editors’ larger claims that research in IPE has been “problem focused,” the emerging “informality in IPE” research agenda tends to align with topics like international trade, finance, and banking. While this narrower focus has begun to uncover important details, a downside of this problem-centric approach is that if the study of IPE appears disjointed, the application of informality to IPE appears doubly disjointed. This chapter therefore brings various strands of “informality in IPE” together and highlights ripe areas for more cohesive work going forward.

Informality has infused IPE research agendas in at least three different ways. First, informality may refer to *soft law*, where countries codify their intentions but in a non-binding way such as through the Basel Agreement (Wilf 2016). Second, informality may refer to

informal governance such as the norms and procedures that govern decision-making beyond formal rules (cf. Stone 2011, 2013). Third, informality may refer to *informal intergovernmental organizations* (IIGOs) such as the G7 that do not have a treaty or a secretariat that can gain agency (Vabulas and Snidal 2013, 2020). This chapter identifies these three ways that informality enters mainstream IPE research in order to emphasize the breadth and importance of informal arrangements so that readers can be more nuanced when thinking about informality.

As the editors also claim, research in IPE is largely apologetic; the study of informality in IPE mirrors this trend because it is largely written in the shadow of studies centered on formal arrangements among states. This means that literature on informality in IPE (this chapter included) often starts in a defensive tone with formal, hard law standing in as the assailant we wish to sidestep. But the upside to applying the dual lenses of problem-centricity and an albeit sometimes deprecatory approach to informality is that scholars have gained deeper, richer insights about the changing and differing nature of political cooperation in our global economy. Nonetheless, the inspiring work that is carving a future in our field also reveals that there is still much work to be done.

THE THREE TYPES OF INFORMALITY IN CURRENT IPE RESEARCH

Soft Law

Soft law forms the basis for understanding informality in IPE. Abbott and Snidal (2000) elevated international relations (IR) scholars' attention to why actors may sometimes prefer the reduced precision, less stringent obligation, and weaker delegation of soft law in international interactions. If hard law is analogous to formality then soft law is tantamount to informality (cf. Pauwelyn et al. 2012). Soft or informal law can be beneficial for several reasons. First, soft law has lower transactions costs, so states can form commitments more quickly and efficiently (Abbott and Snidal 2000; Abbott et al. 2000; Lipson 1991; Slaughter 1997, 2009). Soft law can also bypass domestic constraints because there is rarely a ratification process that narrows the list of veto players. Soft law usually frames agreements in aspirational tones ("parties intend to" or "strive to achieve") (Brummer 2010, 632) which allows states to retain some control over when they will actually adopt policies. Last, soft law helps states mitigate uncertainty but still allows for forward-movement, even when details are not clear. This is particularly relevant in today's international political economy with many interconnected parts that drive financial success and collapse.

This is not to say that soft law is the only hedge against uncertainty both in IR generally and IPE more specifically. There is also prolific literature in IPE regarding how states mitigate risk through flexibility provisions (Koremenos 2001; Kucik and Reinhardt 2008; Rosendorff and Milner 2001) such as writing vague treaties, including reservations or opt-out clauses (Koremenos 2013), and even allowing signatories to exit (von Borzyskowski and Vabulas 2019). Moreover, soft law is certainly not an elixir for all challenges in IPE. As Pauwelyn (2014) emphasizes, soft law and informality more broadly raises issues of accountability

and legitimacy. Without formal disclosure rules, meetings that occur off the record, and agreements that escape domestic approval, informality is less transparent than much of its formal brethren (Vabulas and Snidal 2013, 2020). Relatedly, informality may lead to weaker oversight, effective involvement of civil society, and less just outcomes.

Informal Governance

A second aspect of informality in IPE is informal governance: the uncoded rules of behavior running parallel to the formal treaty rules (Kleine 2013; Stone 2011, 2013). Helmke and Levitsky (2004) define *informal institutions* as “socially shared rules, usually unwritten, that are created, communicated, and enforced outside of officially sanctioned channels.” This definition (while created by comparativists and mostly used outside IPE) is useful because of its universality. Colgan and Van de Graaf (2015) remind us that informal governance runs parallel to formal *procedures*. Christiansen and Neuhold (2013) and Kleine (2013) show a number of ways in which European Union (EU) decision-making procedures are informal, including how the European Parliament and the Council of Ministers legislate. Colgan and Van de Graaf (2015) show that informality—and not just power asymmetries, as Realists would have us believe—provides a compelling argument for how rules and procedures emerge in international organizations (IOs). A study of the International Energy Agency—which advocates for reliable, affordable, and sustainable energy—shows that when states have myriad outside options to the formal rules within one IO, informal governance can keep states operating within the current organization. Indeed, each time the IEA has authorized emergency releases of oil, it has done so outside the formal crisis response system.

Stone (2011) shows that while IPE-related institutions often have formal rules that benefit weaker states, powerful states have an uncanny ability to use informal governance to achieve their desired preferences. Stone’s work is a perfect example of the editors’ thesis that IPE has moved beyond epistemological debates: he combines both institutionalist and realist theories to show that powerful countries maintain a high degree of informal control in international financial institutions (IFIs) because other members informally give hegemons control when their interests are strong. Less powerful states may have formal power vis-à-vis codified structures, but these back-door channels of informality are how powerful countries really guide entities like the International Monetary Fund (IMF). Chwiero (2013) goes further to show how the IMF staff also exercise informal governance over IMF lending but Morrison (2013) shows how board members may have a declining informal influence in IDA.

Informal institutions and governance are particularly important in IPE because markets often revolve around mechanisms such as trust, reputation, and networks—which are considered informal in comparison to mechanisms like contracts. Research shows that parties tend to abide by these informal mechanisms because they value reputation and are conscious of future repeated interactions. So where does informality come from? Since a large body of work in IPE examines regional agreements, it is worth asking whether informality has a regional component. Peng (2002), for example, recognizes the importance of informal institutions and their effects on formal organizations in the context of emerging markets in Asia. Acharya (2009) takes this concept further by coining “the ASEAN Way” to articulate that more communitarian, consensual, and informal mechanisms that define organizations like ASEAN are largely aligned with Asian social norms. However, recent work

by Vabulas and Snidal (2019) largely refutes the idea that informality is regional or even an Asian phenomenon. They show that informality has become an increasingly important organizational form both globally and across *all* regions, not just in Asia. Moreover, European international institutionalization has also become markedly less formal in recent years, following the overall changes in how states are cooperating internationally. Informality is therefore not a property of particular regions but instead reflects a growing and general demand among states to protect their sovereignty and coordinate during uncertainty—both in IPE and across other issue areas.

Informal Intergovernmental Organizations (IIGOs)

As these definitions highlight, informality can operate *within* formal intergovernmental organizations (FIGOs) (Stone 2011).¹ Nevertheless, Vabulas and Snidal (2013, 2020) elucidate that informal governance can also operate in stand-alone entities called informal intergovernmental organizations (IIGOs). IIGOs are institutions where high-level officials from three or more states meet recurrently to coordinate behavior but have neither legal personality codified through a treaty nor any permanent independent institutional apparatus such as a secretariat (Vabulas and Snidal 2013, 2020).² It is worth emphasizing that IIGOs and FIGOs are ideal types occupying intermediate levels of institutionalization along a broad spectrum of international institutions ranging from anarchy to supranational organization. Moreover, while scholars have gone to great lengths to point out the differences between informal and formal institutions, they often complement each other and influence each other's direction and content (North 1990; Vabulas and Snidal 2013, 200).

States are increasingly using IIGOs while the creation of new FIGOs has plateaued (Vabulas and Snidal 2020). Some IPE-related IIGOs include the BRICS,³ the Paris Club, the G7 and G20. Even the GATT had informal roots. IPE-related IIGOs tend to operate as forums where states can agree on best practices (oftentimes through standards and rules as in the Basel Committee on Banking Supervision) but allow states to adopt policies autonomously at the domestic level. IPE-related IIGOs also often serve as coordinating forums where states work in smaller groups to come to consensus—which is often documented in communiqués—which can then be used as bargaining leverage in FIGOs. However, only 26 percent of IIGOs are economically focused, so the informality trend has not been as marked in the economic area as in political, security, and social issues (Vabulas and Snidal 2020). Formal treaties still tend to be the dominant form of cooperation in IPE with 50 percent of FIGOs being economically focused (Pevehouse et al. 2019). This is likely because finance and trade need clearly articulated rules with binding conditions, processes for monitoring, and codified mechanisms for enforcement.

The G7 and G20 are two of the most well-known IPE-related IIGOs. The G7's roots start in the early 1970s when finance ministers from the five leading industrialized nations (US, France, Germany, Japan, and UK) met periodically as the Group of Five (G5) to handle crisis situations. These informal gatherings started to include heads of state or government, as well as two additional countries—Italy in 1975 and Canada a year later—and the G7 emerged.⁴ While the original mandate included economic, monetary, and financial issues, the G7's purview has since expanded to include almost all important international problems. The informal structure allows leaders to get to know each other better through in-person summit

dialogues, build prestige, set the international agenda, and attain broad political bargains (Gstohl 2012).

In late 1999, the G8 created the G20 forum of finance ministers and central bank governors in response to increasing criticism that the G8 had become an unrepresentative group of global elites. The G20 is comprised of nineteen developed and developing member states (plus the European Union (EU)) whose leaders meet in recurring diplomatic summits but with neither a legalized treaty nor a secretariat. Instead, countries rotate who will chair and host summit meetings where members address topics of mutual concern. The financial crisis of 2008 upgraded the G20 from a financial meeting to a broader summit of world leaders. The G20 now acts as “a hub of a global network” (Kirtan 2013, 46) by engaging other key IOs like the Financial Stability Board, IMF, World Bank, World Trade Organization (WTO), and Organization for Economic Cooperation and Development (OECD).

The main question that scholars of “informality and IPE” try to address surrounding the G7 and G20 is how these institutions shape international relations. Few would argue that these IIGOs are meaningless, but putting a finer point on the exact ways in which they shape states’ incentives is key. Skeptics claim that leaders’ declarations from summit meetings can look like “compromise texts full of banal niceties,” and summit meetings can cost host countries hundreds of millions of dollars, not including the exorbitant insurance claims arising from damages from protests (Narlikar 2017). Advocates of the G7 and G20, on the other hand, argue that their declarations include complex negotiations and concessions which could shape the future global order. A more nuanced perspective is that the power of these IIGOs might be their role in deliberation because their informal set-up allows states to come together to articulate goals as imperfect and unfinished pieces of work, and to jointly commit to moving in the “right” direction. Still, both the G7 and G20 face severe criticism for representing only the powerful elites of rich countries, removed from the people affected by their decisions. While developing countries have long argued for more voting rights in formal IFIs—a topic that has garnered significant attention in IPE—at least they have a voice. Just as the editors of this volume lamented that studies of remittances have lagged because most IPE scholars live in countries not reliant on remittances, so too lags the perspective of what developing countries need in terms of financial regulation.

The rest of the chapter looks at how informality fuses with the various “problem-centric” topics of IPE including international finance, trade, and other issues.

INFORMALITY AND INTERNATIONAL FINANCE

When compared with international trade and money—which is often regulated by “hard” international law or formal IGOs (as articulated above)—it is striking how often the world of international finance *is* regulated by informal institutions. The G7 and G20 are prime examples of these consensus-based inter-agency forums that reach non-binding recommendations for best practices (Brummer 2010; Singer 2007). Other examples include the Financial Stability Board, which oversees systemic risk, and the Basel Committee on Banking Supervision, which is the standard setter for regulating banks. Together, these informal institutions provide coordinating mechanisms to handle macro-prudential oversight while attempting to minimize collective action problems or conflicts of interest among

various states. They also establish voluntary rules that (perhaps counterintuitively) constrain various actors in both the public and private spheres (Newman and Posner 2018). Informal international finance bodies can also provide a clearinghouse of data and statistics for national regulators, with reports taking on commitment-like qualities (Brummer 2010, 629). This helps to facilitate a better flow of information between various national authorities, often through memoranda of understanding. This soft law in international finance is often reinforced by hard law (Pratt 2018).

Informal financial organizations also play a powerful role in agenda-setting. For example, Puetter (2004) highlights that the Eurogroup—comprised of the finance ministers of the euro area—plays a widely unnoticed but essential role in economic governance. In this IIGO, members pre-agree to Council decisions and is where ministers decide on the overall orientation of economic governance in the euro area. Eurogroup members do all of this without any formal role assigned in the EU treaty or formal decision-making structures. Its emphasis on consensus formation and member states' voluntary commitments to commonly agreed policy objectives is a key role shared across other IIGOs.

Brummer (2010) shows that states frequently adhere to informal international finance laws even when following through on commitments may have steep distributional implications. Wilf (2016) similarly finds that informal international financial regulatory networks can promote a high degree of credibility. Basel III—an international agreement about bank capital minimums negotiated by the bank regulatory network in 2009 and 2010—affected regulated US firms even though it was not a binding, legal agreement. More specifically, stock market returns (which measure investors' perceptions) significantly differed from expected returns on days when new information about Basel III became available. This shows that informal IPE arrangements can lead to credible commitments because IIGOs can report and publicize non-complying jurisdictions. This can make it more costly for those states to compete in the marketplace by inferring that the miscreant state is more risky. Actors may therefore have a vested interest in complying with international finance-related IIGOs because they do not want to face higher costs of capital or more difficulty in attracting foreign business.

INFORMALITY AND TRADE

So why does informality dominate international finance but not international trade? Ferran and Alexander (2010) argue that informality benefits international finance because financial strategies are constantly being innovated, market participants are regularly gaining new technological capabilities, and macroeconomic environments are always being updated. This means that oversight and regulation of financial markets also needs continuous adaptation (Hardacre 2012, Singer 2007) whereas trade-in-goods is less fungible, so there should be fewer ways to skirt core provisions of trade agreements. Most international trade scholars have thus looked at formal treaties and the bureaucracies that carry them out. For example, IPE scholars have paid significant attention to the proliferation of preferential trade agreements (PTAs) which skyrocketed in the 1990s and 2000s including numerous regional free trade agreements (RTAs) like the North American Free Trade Agreement (NAFTA). As other chapters in this volume highlight, IPE research has thus focused on the real world:

who was likely to join these formal treaty-based organizations, how accession criteria varies across members, and whether these entities are successful in bolstering global and regional free trade.

Still, there are important aspects of informality in international trade. While the WTO is one of the most institutionalized FIGOs, its predecessor, the GATT, was much closer to an IIGO. The GATT did not rest on a formal treaty but was instead drafted as an interim agreement pending entry into force of the Charter of the International Trade Organization (ITO) (which never materialized) (Winham 1998). The GATT was not given any institutional structure—it simply evolved through practice. Moreover, even though dispute settlement was informal in the GATT (i.e., it was not compulsory and could be blocked by individual states), Pauwelyn (2014) argues that in many ways it was more successful than today's automatic, compulsory WTO dispute settlement mechanisms. While the WTO is highly formalized, there are myriad ways that informality seeps in. Arato (2013), for example, shows that many of the WTO's transformations have not occurred through constitutional amendments, but have instead been more informal through the judicial body's constitutional interpretation and practice. Moreover, Shaffer, Wolfe, and Le (2015) make the case that WTO states can improve discipline on subsidies through informal law.

Steinberg (2002) also reminds us that while the WTO is highly formal, the rules of the game that dictate day-to-day operations are largely informal and significantly depart from the WTO's formal treaty provisions. For example, while the WTO has an elaborate set of formal committees, the need for consensus at ministerial meetings creates a large need for representatives to brainstorm and engage in informal groups ahead of official votes. Informal "minilateral" working groups (which now far exceed formal, official meetings⁵) like the Quad (US, EU, Japan, and Canada), Friends of Fish, and the Cairns Group⁶ can help structure pre-dialogues and allow states to create informal bargaining coalitions (Wolfe 2004; Prantl 2005). Private and/or unofficial meetings can be helpful to the advancement of consensus particularly because they do not often have to go on the written record, can be ad hoc, and unscripted (cf. Lydon 1998). Indeed, Canadian Prime Minister at that time, Paul Martin, nicely articulates "the most fruitful exchanges between leaders often take place in the corridors of great meetings, one on one, far removed from the actual agenda. When leaders do meet in international fora, it is difficult to break free of the 'Briefing Book' syndrome and get down to brass tacks, to thinking outside the box."⁷

Informal groups might be useful, therefore, as instruments of power bargaining for the weak, but as we see them proliferate alongside the heterogeneity of developing country preferences, too many meetings may be counter-productive. Moreover, these informal meetings can be unrepresentative and not transparent. "Some ministers are on a perpetual circuit of meetings, and . . . smaller meetings constitute a self-selected ad hoc steering group with no legitimacy" (Wolfe 2004).⁸ Most of the WTO-based coalitions have eluded record until recently (Narlikar 2003) so scholars should continue to pay attention to how informal working groups affect the transparency, exclusivity, and accountability of IPE-related organizations. Moreover, IPE scholars should also pay more attention to how "informal trading" (outside of the WTO) affects our world economy, and not just the effects of formal trade agreements.

Indeed, we have witnessed a stagnation in the formation of formal trade treaties since the turn of the millennium.⁹ So why has the design of international trade agreements largely

shifted since the early 2000s? Again reacting to this “problem,” IPE scholars have turned their attention to understanding *why* these formal trade agreements seem to be slowing down. Their answers largely point to saturation and an increasing difficulty in reaching agreements among a broader and broader group of states (highlighted in other chapters of this volume). Moreover, there has been a shift in public opinion (and support) toward trade, which has been shaped by changing definitions of who the winners and losers are. Since this chapter focuses on informality, though, it is worth noting that much less work has focused on the concomitant role of the rise in informal rules and standards in international trade just as formal treaties have slowed down. It is likely that these are not unrelated. Pauwelyn argues that informal trade standards may be more legitimate and coherent than formal ones in terms of input and content. Especially at the international level, where centralized enforcement is absent, actors comply for reasons that go beyond legal constraints including reputation, reciprocity, retaliation, or perceived legitimacy, all of which may be just as activated (with fewer costs) in informal rules as in formal treaties.

INFORMALITY AND INTERNATIONAL FINANCIAL ISSUES

Informality also plays out in obvious ways regarding leadership at key IFIs. Informal norms, for example, have ensured US leadership of the World Bank and Western European leadership of the IMF since their outset. States seeking to reform the international political economy regularly target these appointments. Manulak (2017) shows that this pattern of powerful states designing secretariats (informally) in a manner that enhances their unilateral influence in institutions extends even beyond IFIs.

Informality in IFIs also plays out in studies of donor influence (cf. Thacker 1999; Stone 2011; Barro and Lee 2005; Andersen, Harr, and Tarp 2006; Dreher and Jensen 2007; Dreher, Sturm, and Vreeland 2009; Andersen, Hansen, and Markussen 2006). But Kilby (2013) notes it is difficult to disentangle formal versus informal pathways of donor influence because the details of executive board decisions are rarely made public. Using variation in the World Bank's group of 24 Executive Directors (EDs) who represent 186 member countries, Kaja and Werker (2010) show that informal influence can guide World Bank lending. Since EDs usually represent more than one country at a time, an informal sort of rotation takes place; when a country's national is an ED, that country receives on average an additional \$60 million in International Bank for Reconstruction and Development (IBRD) loans per year. They surmise that this is due to informal rules and norms in the boardroom.

Kilby (2013) also shows that donors can influence World Bank allocation decisions in two distinct periods: up through loan approval and during the post-approval stage related to the speed and size of the loan. The US in particular exerts informal influence in the disbursement of World Bank loans; countries that vote with the US in the UN get much more favorable terms on their loans versus non-aligned countries who receive structural adjustment programs that are stricter about adhering to macroeconomic indicators. Moreover, this informal influence permeates to day-to-day investment project disbursement decisions.

These patterns of informal influence are also evident at regional financial institutions such as the Asian Development Bank (Kilby 2011), suggesting that it might be interfering with the efficiency-promoting goals of a wide number of IFIs. Moreover, the omnipresence of informality suggests that updating formal governance structures (such as voting shares) alone will not provide comprehensive reforms to governance structures in IGOs where informality can play a significant role.

Informality has also been the protagonist in the problem of money laundering and illicit finance (Sharman 2009). Soft-law entities like the Financial Action Task Force (FATF) (and their myriad regional affiliates) have emerged to play a significant role in making recommendations about state policies to combat money laundering and illicit financing. The FATF has a noteworthy design feature: for a long time, it did not have a permanent charter but instead, member states periodically extended its mandate.¹⁰ Despite being soft law-based, it is powerful because it issues global recommendations that market actors respond to which in turn pushes states to make changes (Morse 2019; Sharman 2008). In particular, the FATF issues a non-complier list which Morse (2019) shows has successfully generated policy change in states: 90 percent of listed countries in 2015 had adopted FATF-compliant laws on terrorist financing versus the baseline in 2009 when not a single soon-to-be-listed country had a compliant law. Moreover, Morse (2019) shows that listed countries experience a 16 percent average decrease in cross-border bank-to-bank lending, compared to when they are not on the FATF list. These findings show that compliance does not have to come through strict legal-based criteria but instead that global performance indicators (GPIs) can have a powerful effect on countries' compliance through decentralized mechanisms like market responses, particularly when they are highly integrated into the global economy.

Informal institutions also affect legitimate financial flows including foreign direct investment (FDI). Seyoum (2011) finds that certain informal institutions—such as those based on high levels of trust and reputation—exert a positive and significant effect on attracting FDI. Operationally, these informal institutions are manifest as the ethical behavior of firms, efficacy of corporate boards, the strength of auditing and accounting standards, and the importance of corporate social responsibility in a country. These informal institutions help foster better contract enforcement, provide the basis for better information-sharing, and lower transactions costs related to screening and monitoring. Seyoum (2011) further argues that these informal institutions may be even more relevant in developing countries because they can play a profound role when there might otherwise be a weakly regulated business environment, ailing legal systems, or poorly enforced laws.

Holmes et al. (2013) similarly emphasize the importance of informal institutions—such as the cultural dimensions of collectivism and future orientation—in shaping inward FDI. They show that these informal institutions matter because they shape formal institutions such as a country's regulatory, political, and economic institutions. This research therefore reminds us not to necessarily separate the study of formal and informal institutions but instead to better understand their interrelationships. For example, when secretaries of commerce or the treasury get together, they may not obsess with the “stark” differences that IR scholars see between treaties and memoranda of understanding. Unfortunately, we have little knowledge of whether policymakers are consciously choosing informality or whether they are instead using a boundedly rational approach of muddling through. Getting in the

heads of policymakers, and better understanding how they think about informality in IPE, is another important next step on our agenda.

One aspect of IPE that has not received much attention—at least by political scientists—is the taxation of global e-commerce. While there is much to be celebrated by our subfield’s problem-centric guide for research, it means that the “dogs that don’t bite” often go under studied. Because there has not been significant global tax legislation or administrative guidance passed at the national level, it has escaped a lot of IR scrutiny. Where the lens of informality has been helpful is to understand why there might have been a lack of action on e-commerce taxation at the national level. Cockfield (2005) argues that the OECD has played a leadership role in informally spreading guiding principles and tax rules to confront the e-commerce tax challenges. While the OECD itself is a formal IGO, it has structured debates regarding taxation like many IIGOs: emphasizing deliberation, consensus-building, and using non-binding mechanisms rather than legally enforceable treaties. Cockfield (2005) also points out that the OECD’s informal role here is not a second-best solution but rather, its success shows that calls for a formal “world tax organization” structured through binding tax rules could be misdirected. IPE scholars should use the lens of informality to better understand other aspects of international taxation including tax evasion and tax havens in the figurative “race-to-the-bottom” of income tax competition that scholars of globalization have noted as a possibility.

While this chapter has made a convincing case that informality is pervasive in IPE, it is less obvious in some “problems” than others. Certain political-economic flows lose their power—or cannot even take place—when not done formally. For instance, the efficacy of international sanctions relies on them being overt, public, and formal. This does not mean that informal “eye-for-an-eye” types of sanctions do not occur; instead, these have implicitly upheld tit-for-tat types of bargains for centuries (Snidal 2002, 78). Nonetheless, very few scholars have examined informal sanctions, probably due to the difficulty in analysis. An exception is Portela (2010) who examines the efficacy of informal sanctions in the EU—those that are adopted outside the legal frameworks of the CFSP, ACP, and GSP Regulations. Portela’s (2010) study includes informal sanctions announced in EU Presidency statements, in conclusions of the European Council, or the General Affairs and External Relations Council. The only evidence of these sanctioning decisions therefore occurs in these letters. She acknowledges that the notion of “informal sanctions” is neither an established category in sanctions research, nor does it belong to the EU’s legal terminology. Moreover, while once commonplace, informal sanctions have become less frequent as more elaborate legal instruments have formalized sanctions in the EU. Despite this, informal sanctions have continued in a parallel complementary fashion.

IPE scholars could also benefit from better understanding the informal negotiations that shape consultations before, during, and after sanctions take place. However, most of these discussions take place off the record in a way that makes them hard to rigorously examine. As this chapter continues to emphasize, though, this does not mean they are not important, but that we should look for creative ways to study them. Another avenue for budding research on informality and sanctions focuses on non-state actors including non-governmental organizations (NGOs), multinational firms, and even terrorists. For example, under what conditions are “informal” consumer boycotts effective in shaping the power structures of our international political economy?

BRINGING INFORMALITY AND VARIOUS IPE “PROBLEMS” TOGETHER

The previous sections highlighted how informality affects the various problem-centric fields that dominate IPE. Despite highlighting different dependent variables, many commonalities arise. First, scholars emphasize that informality does not necessarily mean that outcomes are less effective. Similarly, the sober findings heretofore remind us that informal and formal institutions are not normatively better or worse but instead that they have different effects on international relations. A big part of the informality-IPE agenda has therefore focused on when and why we might see effects (and the conditions under which we will not). Last, this research agenda shares a concern for how informality affects transparency, accountability, and justice.

The editors of this volume also argue that IPE scholarship has had a limited impact on policy debates, a conclusion that is accentuated in the study of informality. However, as the editors underscore, this may not be negatively interpreted. For example, if scholars of IIGOs advocate that more organizations should be registered with Congress (as the Case Act mandates for formal organizations), this could provide greater visibility to budgets, resources, and meeting decisions. This might solve challenges related to non-transparency and accountability, but it might also have the unintended consequence of taking away the gains that informal organizations bring in the first place.

The newness of informality in IPE scholarship is not due to ignorance of the informal processes that can be influential in our global economy. Instead, the recent turn to the study of informality may be due to the IPE field's overall bias toward things that are quantitatively measurable. While the editors of this volume argue that methodological divisions are largely overplayed in debates on IPE, there still exists a slight bias toward statistical studies in IPE which has made it difficult until recently to study informality. Informality, by its nature, is hard to measure which makes it often difficult to include in statistical analysis. Indeed, as recently as 2003, Nielson and Tierney argued that to study delegation in IOs, they would evaluate “formal notions of power because they could measure them through constitutions, articles of agreement, or charters.” While they acknowledged that many IO scholars believe an IO's real distribution of power and rules of the game may be different to its formal rules (cf. Middlemas 1995), Nielson and Tierney argued that this methodology would allow them to deduce clear sets of expectations and aggregate outcomes. Their approach should be heralded because particularly at the time, critics disparaged much IO research as being too soft and fuzzy. Bringing precision and math to the otherwise “flimsy” topic of IOs was therefore more than welcome. Yet this reality has meant that, for a while at least, IPE scholars have swung the pendulum in the direction of crisp measurement, largely leaving out informality.

While there has been rich, qualitative work in IPE, the bias toward quantitative analysis has also been reinforced by the fact that economics lends itself (not exclusively) to the analysis of numbers. This slender primacy toward quantitative work in IPE creates a strong, but not insurmountable hurdle for better understanding the real world of IPE which is infused by informality in many ways. It is worth noting that there does not seem to be a big divide between North American and European communities within the informality in IPE research tradition. Another reason that informality may have taken so long to work itself into IPE is

the simple fact that it has taken the IR field decades to embrace the importance of formal institutions (Abbott and Snidal 1998); so until now, the idea that informal institutions were important would have been laughable.

While this chapter heralds a basic idea, that informality is an important factor in driving outcomes in IPE, it is also worth reminding careful readers of selection bias in these published journal articles. Of course scholars will celebrate the seemingly counterintuitive finding that informal law can result in changed political economy outcomes. But what of all the times when informality in IPE *doesn't* alter states' behavior? As a research community, we should seriously grapple with the criteria for determining if and when informality results in real change.

CONCLUSION AND FUTURE RESEARCH

This chapter identifies three types of informality and shows various areas of IPE where informality is part of research debates. Excluding informality—whether it be soft law, informal decision-making procedures, or intergovernmental organizations—from our study of IPE may create biased results. Future IPE scholars should incorporate both formal and informal forms of cooperation when trying to understand the politics of and effects on our global economy. While this is a hard marching order—as already noted, informality is not easy to measure—the various articles cited in this chapter show that informality in IPE is a far stretch from nailing jello to a wall. Instead, with careful research design, we can better unpack how informal and formal rules intersect, overlap, or even outdo each other to create the dynamic interplay in our international political economy.

Future research needs to hone in on the role of informality in budding aspects of our global economy. For example, shadow economies play a large role in developing countries' day-to-day work with over 60 percent of the world's employed population in the informal economy.¹¹ While the informal economy is a different intellectual space from notions of informality in this chapter, it has important questions for students of international institutions.¹² Many IGOs' mandates (including the United Nations (UN)'s sustainable development goals) suggest that states will not be able to grow without a massive conversion to the formal economy, but we do not know much about the collective action challenges entailed with transition. Better understanding the informal sector—where most workers lack social protections, rights at work, and decent working conditions—might help us to dissect and diagnose solutions to the backlash toward globalization in new ways. Moreover, IPE scholars are well equipped to study which WTO measures might have the potential to encourage traders to switch from informal to formal cross-border trade.

Similarly, block chain technologies and cryptocurrencies—which largely try to work around the formal role of central banks—need to be more carefully folded into our studies of IPE (Maftei 2014). We also need to know more about the politics of counterfeit markets, illegal drug trading, and the massive (informal) economy on the dark web. Migration and refugee scholarship should look at the informal consultation that often underlies (or even stands in for) intergovernmental agreements (Von Koppenfels 2001). And perhaps some of the most exciting areas for future scholarship on informality and IPE include the intersection of these topics. For example, given the promise but also the headaches of migrant

remittances, could electronic payment systems that use cryptocurrencies rather than formal money improve international money transfer, especially for the unbanked? On quite the opposite end of the remittances spectrum, how does the informal system of migrant remittances used in the Muslim world—Hawala—affect formal international finance. The irony is that informal IGOs are perhaps best suited to handling these topics going forward because informal rules are so well suited to the emerging but highly disruptive issues that often characterize our complex world.

The irony is that perhaps these topics *are* already attracting significant scholarly attention among scholars of IPE. Yet because of our problem-centric study in IPE—and the fact that these “problems” are not in the mainstream nor in the traditional IPE wheelhouse—the typical IPE scholar is not studying them. This presents an opportunity for more collaborative work between political scientists and economists, anthropologists and sociologists (among others) to better understand how informality is affecting our global economy. And maybe in the end, this increasing appreciation for informality—and a need for more connected research that also looks beyond the OECD world—will aid IR scholars in their pursuit for more “big” IPE theories which many navel-gazers in our discipline lament has still not occurred.

NOTES

1. Pevehouse et al. (2019) describe formal intergovernmental organizations as having three distinct characteristics: a formal existence established by an international treaty; three or more states as members; and a permanent secretariat or headquarters.
2. Some IIGOs do have small secretariat functions but qualify as IIGOs because they are neither permanent nor independent. E.g., staffing these secretariats on secondment from member governments often signals lack of agency.
3. In 2014, the BRICS, an informal intergovernmental organization, created a formal intergovernmental organization, the New Development Bank (previously called the BRICS Development Bank).
4. Russia was first present in 1994 and gained full membership into what became the G8 in 2006; however, Russia was kicked out of the club in 2014 for annexing Crimea and the G7 has remained the focal institution.
5. E.g., in 2001, there were nearly 400 formal but more than 500 informal meetings.
6. https://www.wto.org/english/tratop_e/dda_e/negotiating_groups_e.htm Accessed May 22, 2019.
7. Address by Prime Minister Paul Martin on the occasion of his visit to Washington, D.C. April 29, 2004. <http://pm.gc.ca/eng/news.asp?category=2&id=192>, accessed July 7, 2004.
8. <http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.113.3493&rep=rep1&type=pdf#page=35>
9. See, e.g., Pauwelyn (2014). For each decade since the 1950s, the number of new multilateral treaties deposited with the UN Secretary General was around 35. In the 10 years between 2000 and 2010, this number dropped quite dramatically to 20. In the preceding five decades it had never been below 34. Between 2005 and 2010, only nine new multilateral treaties were deposited; in 2011, 2012, and 2013, not a single one. The broader UN Treaty Series database confirms this downward trend as of the 2000s, both for bilateral treaties (12,566 concluded in the 1990s; only 9484 concluded in the 2000s) and multilateral treaties (406 entries in the 1990s; down to 262 in the 2000s).

10. In April 2019, the FATF adopted its first-ever open-ended mandate to begin in 2020. <https://www.fatf-gafi.org/publications/fatfgeneral/documents/fatf-mandate.html>. Moreover, while the FATF uses the OECD for secretariat functions, it is permanent and quite independent with its own FATF budget and website. The secretary is also tasked with autonomous functions including leading the FATE, understanding risks, and conducting analysis. Staff are not on secondment from member governments. Together, these factors disqualify the FATF from the IIGO ideal type, even though it is still considered “soft law.”
11. https://www.ilo.org/global/about-the-ilo/newsroom/news/WCMS_627189/lang--en/index.htm. Accessed May 22, 2019.
12. See Milner and Rudra (2015) for more on IPE and the informal economy.

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CHAPTER 22

INTERNATIONAL FINANCIAL INSTITUTIONS

Forms, Functions, and Controversies

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INTRODUCTION

FEW intergovernmental organizations attract as much attention and controversy as international financial institutions (IFIs). Depending on the account, they are described variously: as averting or deepening economic crises; as lifting people out of poverty or devastating livelihoods through ill-conceived advice; and as aiding climate change mitigation or promoting projects that harm the environment. Of course, it is possible that many of these accounts are simultaneously true: it depends on the geographical, temporal, and economic context, and hinges on the selection of relevant indicators or time horizons to evaluate success or failure.

The purpose of this chapter is to provide background and context for these ongoing debates, which we lay out in three sections. We begin with mapping out the universe and functions of IFIs by sorting them into two categories: financial firefighters and development promoters. The second section addresses the IFIs' varied governance structures, and looks at how different IFIs address the trade-off between resources and autonomy. In the third section we examine two of the leading controversies surrounding the IFIs' activities: the problematic impact of these activities on social and environmental outcomes; and the charge that they impinge on developing countries' policy sovereignty. We conclude by outlining fruitful directions for future research.

MAPPING THE FIELD: ECONOMIC STABILIZATION AND DEVELOPMENT FINANCING

Among the multitude of multilateral bureaucracies, IFIs are distinguished by their core focus on acquiring and investing or disbursing large amounts of money. Beyond this common pursuit, IFIs fall into two categories, depending on the ends this money is supposed to serve. First, there are those charged with helping countries stabilize their exchange rates, and with preventing currency instability from threatening the global economy. Second, there are IFIs charged with the promotion of growth and economic development, known as multilateral development banks (MDBs). Both stabilization and growth were on the minds of the participants of the Bretton Woods conference in 1944—the foundational moment for postwar global economic governance, and were conceived as the core missions of the two organizations sprung out of that meeting: the International Monetary Fund (IMF) and the World Bank. These organizations remain the two biggest and most influential IFIs today. However, as we show below, today the world is increasingly populated by a host of other IFIs—some complementary to the Bretton Woods twins, and some having parallel or competitive operations.

Stabilizing the Global Economy

In a global economy, no nation is an island: each depends for many of the things it consumes on imports from other countries, and on revenues from the goods it exports to other countries. There is also a constant migration of capital flows (in the form of investment or loans) back and forth across national borders. When a country's export revenues and financial inflows add up to less than the cost of imports and capital outflows, the country is said to have a balance-of-payments deficit. This causes its central bank to lose reserves, and perhaps ultimately to a devaluation in its national currency. Orderly, incremental currency devaluations are routine events. Sometimes, however, there are big destabilizing shocks—such as a spike in the price of imports, or a crisis in the national financial system—that can cause large, disorderly devaluations, with a host of undesirable side-effects: escalating debt, the mass exodus of nervous investors, spiraling inflation, and the contagious spread of financial instability to other countries.

Enter the financial firefighters: international organizations charged with helping promote global financial stability. The IMF—the largest of these financial firefighters—provides loans to governments to manage and contain balance of payments crises. Today, the IMF has an estimated \$1 trillion to devote to this purpose (Gallagher et al. 2021). In principle, all member states can turn to it for financial support, but in practice high-income countries rarely do any more—with the notable exceptions of Iceland, Greece, Ireland, Portugal, and Cyprus in the aftermath of the global financial crisis that started in 2008. The main purpose of the IMF's support is to prevent financial contagion of crises, forestall sharp or disorderly devaluations, and allow countries to keep servicing their external debts. In doing this, the organization is infamous for using these resources as a lever for inducing governments to implement policy reforms, a practice called “conditionality.” This has changed substantially over the years, from the advocacy of a narrow set of reforms—mostly on fiscal and monetary policy—until the 1980s, to a much wider remit: taxation, trade policy, privatization, and labor issues became

targets of loan conditions by the early 1990s, and this list subsequently expanded further to include rule of law and governance issues (Kentikelenis, Stubbs, and King 2016). The augmentation of the IMF's remit was seen as evidence of "mission creep," whereby the organization veered away from its treaty-mandated responsibilities to take on a role in new policy areas and activities (Babb and Buira 2005). Unsurprisingly, the inclusion of an ever-greater number of policy reforms in IMF programs was highly controversial, as we discuss below.

Beyond lending, the IMF also has two additional roles: conducting economic surveillance of member states and the global economy, and aiding "capacity development" in developing countries. In the case of the former, surveillance is institutionalized through so-called "Article IV consultations" (their name referencing the IMF's founding treaty) and Financial Sector Assessment Programs. These entail regular visits to an evaluated country for IMF staff to assess the quality of its economic and financial policies—for example, Article IV reports are published annually for most large economies (Broome and Seabrooke 2007). In addition, the IMF's monitoring of the global economy is conducted through a range of high-profile publications—most notably, the biannual *World Economic Outlook* and *Fiscal Monitor*—as well as various regional reports (Kentikelenis and Stubbs 2021). The third pillar of the IMF's work is capacity development, an activity that has grown within the IMF over the years. It is composed of both training programs and technical assistance to economic policy civil servants around the world (Broome and Seabrooke 2015). The provision of such services is at the request of the recipient country, even though often financed by donor countries, and entails highly practical advice—for instance, on setting up statistical procedures for collecting economic data or on administratively managing tax policy. Together, the three main aspects of IMF operation—lending, surveillance, and capacity development—interlock to make the organization one of the world's most powerful and influential international organizations, that has had a central role in shaping the policy response to economic crises.

Until the end of the 1970s, the IMF had a more or less total monopoly on the job of international financial firefighting. Subsequently, however, a number of other organizations and arrangements emerged to serve the same purpose—some nested within the IMF's framework and operations, and others entirely separate (Kring and Gallagher 2019; Kring and Grimes 2019). The leading example of the former is the Chiang Mai Initiative Multilateralization (CMIM), formed by East Asian countries in the aftermath of the major regional financial crisis of 1997–1998. The CMIM has extensive resources at its disposal—\$240 billion—but it has never been utilized by its member states, in part because a pre-requisite for its assistance is a parallel IMF program which East Asian countries—following their bruising experiences with the IMF at the end of the 1990s (Wade and Veneroso 1998)—want to avoid. In contrast, the smaller Latin American Reserve Fund (FLAR), with its more limited resources of only \$2.9 billion, has had a flexible lending policy that is not contingent on IMF assistance. At least until the onset of COVID-19 and ensuing rapid demand for large-scale IMF support, FLAR had substantially replaced the IMF in balance-of-payments lending in the region (Kring and Grimes 2019, 74).

The IMF and regional financial arrangements together form the two key pillars of what is known as the Global Financial Safety Net, a web of "financial resources and institutional arrangements that provide a backstop during a financial or economic crisis" (Hawkins, Rahman, and Williamson 2014). The third pillar is central bank bilateral swap lines and countries' own foreign currency reserves. The former is available only to a handful of large economies. The latter amount to a form of self-insurance against destabilizing crises, and many developing countries have accumulated high levels of such reserves, partly in order to avoid

needing to resort to the IMF and its mandated policy reforms—described below—in the event of a crisis (Gallagher and Shrestha 2012). Together, the IMF, regional financial institutions and swap lines and reserves are intended to underpin global financial stability and halt domestic or regional crises tipping over into becoming major threats to global financial stability. There is persistent debate as to whether this is indeed adequately achieved, and proposals on how to reform this international monetary architecture abound (see recent review by Ocampo 2017).

Promoting Growth and Development

Compared to the handful of global financial stabilizers that exist in the world today, the MDBs are far more numerous and varied. All bear a family resemblance to the World Bank—the original and leading MDB. They are multilateral organizations controlled by member governments, and derive their operating resources from three separate sources: member government contributions, the interest on bonds that they issue on international capital markets, and loan “reflows” (i.e., repayments of principal, plus interest). The MDBs can issue bonds and borrow at favorable interest rates because they are insured by “callable capital”—pledges by wealthy member governments to back them up in case of financial distress (Clifton, Díaz Fuentes, and Howarth 2021). Financial assistance is disbursed in the form of loans or grants that are received by the governments of low- and middle-income countries, or sometimes by private firms. In addition to financial resources, MDBs also provide member countries with technical assistance.

The core rationale for the existence of the MDBs is that development requires financial investments that private-sector actors will not provide on their own initiative (Helleiner 2014; Kindleberger 1986; Kring and Gallagher 2019). However, the activities they use to pursue these ends have evolved over time. The earliest World Bank loans were oriented toward supporting infrastructure upgrading and capital development projects (such as building roads, bridges, and dams), and this kind of lending remains a focal MDB activity today (Mason and Asher 1973; Mazzucato and Penna 2016). In recent decades, a greater portion of MDB resources have been devoted to social projects in areas such as health and education (Lyne, Nielson, and Tierney 2009). Perhaps most controversially, since the 1980s the World Bank and some other MDBs have provided a significant number “program” or “development policy” loans—once associated with the infamous term “structural adjustment”—to support policy agendas of economic transformation (Babb 2009).

The World Bank is a *global* MDB, with government membership and clientele from different parts of the world. It is the largest MDB in terms of lending, as well as one of the most authoritative actors in international development. This authority does not only emanate from its financing capacity, but also its extensive ideational or epistemic power: its annual *World Development Report* as well as its other publications influence the thinking of development policymakers and economists around the world. Like the IMF, the World Bank also exerts a powerful influence in its role as capacity builder: the World Bank Institute continuously trains senior government functionaries from a host of developing countries, which further bolsters the influence of its ideas (Stern and Ferreira 1997).

In addition to the World Bank, there are a number of other smaller global MDBs (see Table 22.1). For example, the European Investment Bank (EIB) provides funding both to European Union (EU) member states as well as low- and middle-income countries outside

Table 22.1. Global Multilateral Development Banks

Organization	Founded	Headquarters	Instruments	Priority sectors	Shareholders	Top two shareholders	New commitments 2019 (\$ bn)	New disbursements 2019 (\$ bn)
European Investment Bank	1958	Luxembourg	Loans, lines of credit, technical assistance, guarantees and equity	Banking and finance, Transport, Energy	28	Germany, France, Italy, United Kingdom (equal top contributors)	7.9 (non-EU projects only)	4.3 (non-EU projects only)
International Fund for Agricultural Development (IFAD)	1977	Rome, Italy	Loans and grants	Agriculture, Cross-cutting, Industry	176	US, Italy	1.67	0.85
International Investment Bank (IIB)	1970	Moscow, Russia	Loans, grants, lines of credit, technical assistance, guarantees and equity	Industry, Other productive sectors, Banking and finance	9	Russia, Bulgaria	0.143	N/A
New Development Bank (NDB)	2014	Shanghai, China	Loans, grants, lines of credit, technical assistance, guarantees, and equity	Transport, energy, urban development	5	Brazil, China, India, Russia, South Africa (equal top contributors)	7.2	0.92
OPEC Fund for International Development (OFID)	1976	Vienna, Austria	Loans and grants	Banking and finance, Energy, Transport	13	Saudi Arabia, Venezuela	1.8	1.5

(continued)

Table 22.1. Continued

Organization	Founded	Headquarters	Instruments	Priority sectors	Shareholders	Top two shareholders	New commitments 2019 (\$ bn)	New disbursements 2019 (\$ bn)
World Bank Group:								
a) International Bank for Reconstruction and Development (IBRD)	1944	Washington DC	Loans, grants, guarantees, equity and technical assistance	Governance, Energy, Transport	189	US, Japan	27.98 (FY 2020)	20.2 (FY 2020)
b) International Development Association (IDA)	1959	Washington DC	Loans, grants, guarantees, equity and technical assistance	Governance, Transport, Education, Energy	173	US, Japan	30.1 (FY 2020)	21.2 (FY 2020)
c) International Finance Corporation (IFC)	1956	Washington DC	Loans, equity investments, and guarantees	Financial markets, infrastructure, and manufacturing	156	US, Japan	11.1 (FY 2020)	10.5 (FY 2020)

Source: Table expanding on Engen and Prizzon (2018) and using latest available annual reports of each organization (2019 or 2020).

the EU, mostly in northern Africa. Established in 2014, the New Development Bank stands out as an MDB that was established by middle-income countries (the so-called BRICS countries—Brazil, Russia, India, China, and South Africa) as an attempt at “South-South cooperation,” in contrast to the other, Northern-led MDBs (Griffith-Jones 2014; Suchodolski and Demeulemeester 2018).

The *regional development banks*, summarized in Table 22.2, are also major development actors, providing financing to countries in their respective regions. Many of these banks emerged in the final years of the decolonization era (late 1950s and 1960s), as a growing number of independent states had extensive financing needs for infrastructure and development. The newest organizational actor in this class is the Asian Infrastructure Investment Bank (AIIB), established in 2015 with core financing provided by China, in order to provide or catalyze financing for “infrastructure gaps” in its region. Like the New Development Bank (see above), the AIIB was initially conceived as an institution of South-South cooperation, but gradually membership was expanded to a growing number of high-income countries, with Germany now being its fourth-largest member with 4.6 percent of its subscribed capital.

There are also several smaller, *sub-regional development banks*, like the Arab Bank for Economic Development in Africa, the Central American Bank for Economic Integration, the Eastern and Southern African Trade and Development Bank, and the Eurasian Development Bank. They all provide resources to countries in their regions, generally to the tune of several hundred million dollars annually. These banks often face difficulties accessing finance on international capital markets, linked to the low credit rating of their members, but nonetheless have found ways to provide important support to their borrowers. This increasingly comes in the form of short-term trade finance to support imports of energy or basic goods (Humphrey 2019).

The larger MDBs have complex, differentiated structures. For example, the entity commonly known as the “World Bank” is actually part of the “World Bank Group,” made up of distinct sub-units. The World Bank Group’s lending arms are the International Bank for Reconstruction and Development (IBRD) which provides financial support to middle-income countries, the International Development Association (IDA) which offers subsidized-interest loans and grants to low-income countries (the IBRD and IDA are commonly jointly referred to as the World Bank), and the International Finance Corporation (IFC) which is focused on private-sector lending. Like the World Bank, the Asian, Inter-American, and African development banks all have subunits devoted to providing subsidized loans and grants to the poorest countries. Similar to the World Bank’s IFC, the IDB also has a dedicated private-sector lending window, the Inter-American Investment Corporation (IIC).

Finally, over the decades, the larger and better-financed MDBs have expanded into activities far beyond the original lending mission. This is especially true of the oldest MDB, the World Bank. Beyond the IBRD, IDA, and IFC, the two remaining entities that make up the World Bank Group are the International Centre for Settlement of Investment Disputes and the Multilateral Investment Guarantee Agency. These tie into the World Bank’s development mandate, respectively, by facilitating dispute settlement between investors and states and by providing guarantees to help investors access cheaper financing terms and insure against non-commercial risks (like a war or expropriation of the investment by a government).

Table 22.2. Regional Multilateral Development Banks

Organization	Founded	Headquarters	Instruments	Priority sectors	Shareholders	Top two shareholders	New commitments 2019 (\$ bn)	New disbursements 2019 (\$ bn)
African Development Bank (AfDB)	1963	Abidjan, Côte d'Ivoire	Loans, grants, lines of credit, technical assistance, guarantees and equity	Transport, Energy, Banking and finance	80	Nigeria, US	5.1	2.5
African Development Fund	1972	Abidjan, Côte d'Ivoire	Grants	Transport, Energy, Banking and finance	32	UK, US	1.2	1.3
Asian Development Bank (AsDB)	1966	Manila, Philippines	Loans, grants, lines of credit, technical assistance, guarantees and equity	Transport, Energy, Non-sector allocable	67	Japan, US (equal top shareholders)	35.5	16.5
Asian Infrastructure Investment Bank (AIIB)	2015	Beijing, China	Loans, guarantees, and equity	Energy, Finance, and Transport	54	China, India	12.0	2.9
European Bank for Reconstruction and Development (EBRD)	1991	London, UK	Loans, lines of credit, technical assistance, guarantees and equity	Banking and finance, Energy, Industry	67	US, France, Germany, Italy, Japan, UK (equal second-largest Contributors)	10.1	7.2
Inter-American Development Bank (IADB)	1959	Washington DC	Loans, grants, lines of credit, technical assistance, guarantees and equity	Banking and finance, Transport, Governance	48	US, Argentina & Brazil (equal second-largest Contributors)	13.4	10.9
Islamic Development Bank (IsDB)	1975	Jeddah, Saudi Arabia	Loans, grants, technical assistance and equity	Transport, Energy, Education	57	Saudi Arabia, Libya	7.8	8.2

Source: Table expanding on Engen and Prizzon (2018) and using latest available annual reports of each organization (2019 or 2020).

THE GOVERNANCE AND POLITICS OF IFIs

Given IFIs' abundant resources and important roles, most countries have a keen interest in the setup and operation of their governance structures (Helleiner 2014; Kapur, Lewis, and Webb 1997; Koremenos, Lipson, and Snidal 2001; Mosley, Harrigan, and Tøye 1995; Woods 2006). A key feature that sets IFIs apart from other multilateral organizations is that their governance structures do not follow the more common one-country-one-vote or qualified majority voting procedures (Martinez-Diaz 2009). Instead, member states are also IFIs' "shareholders," and their relative power in decision-making depends on their financial contributions, which—in turn—are commonly linked to the country's economic size. This means that larger economies tend to have a much greater say in the running of these organizations compared to the countries receiving loans or grants, and, in many IFIs, the largest shareholder has veto power over decisions.

The system of unequal voting shares finds embodiment in the context of the governing boards that run these organizations day-to-day—often known as "executive boards." This governance structure is akin to private companies' boards of directors: they meet regularly and decide on the range of activities of their organizations—from lending to economic surveillance, and from organizational policies to selecting the leader of the organization. In smaller IFIs, often each member state has a seat on the board. In the larger IFIs, some directors represent single countries (the largest shareholders), while others represent a broader constituency of countries. In the case of the 25-strong board of the World Bank, it has six directors who are each appointed by a single country (the US, Japan, China, Germany, France, and the UK can do so), but the remaining members represent groups of countries. For instance, the constituency headed by an Indian also represents Bangladesh, Bhutan, and Sri Lanka. Even grouped together many countries still have very low voting shares: there are two Board members representing 45 Sub-Saharan African countries—core clients of the World Bank—and, combined, the votes they command are less than Germany's. Similarly, the IMF is also underpinned by unequal voting shares that limit the voice of poor countries in the organization, even though they are the main recipients of its financial assistance. Such imbalances have led to persistent debates around governance reforms within IFIs (Vestergaard and Wade 2013, 2015; Buira 2003), as a constant criticism leveled at them targets their highly skewed representation.

Such unequal voting rights are enshrined in the legal foundations of the IMF and World Bank (Mikesell 1994). Yet, curiously, voting in these organizations very rarely takes place—for example, in the case of the IMF we have not been able to identify a single such vote over the 1980–2009 period. Instead, decisions are routinely reached through consensus, defined as "the absence of explicit, significant and strong dissent" (Portugal 2005, 90–1). In the IMF and World Bank, consensus is gauged by the number of board members objecting, not the voting shares they command. In turn, this can give developing countries—commanding the fewest votes—a larger voice than they would have if votes were taking place (Portugal 2005). In cases of highly contentious issues, the leadership of the organization can go to great lengths to bridge contrasting policy positions and generate the widest possible agreement about the road to be taken, which would—in turn—confer larger legitimacy to decisions. This elaborate system of consensus-making does

not give less powerful countries equal input into organizational policy. Nevertheless, they can influence important decisions at the margins, or have a more prominent role in issues that are of less political salience for the more powerful shareholders. Moreover, when developing countries can form wider alliances, combined with broader political maneuvering, they can block or stall major policy decisions they object to (Kentikelenis and Seabrooke 2017).

Beyond boardroom politicking, there are more ways through which powerful shareholders shape the policy direction of IFIs. One glaring example is the opaque selection processes for the management of these organizations. In the case of the Bretton Woods twins, there is a “gentleman’s agreement” that a European would head the IMF, while a US citizen would lead the World Bank. Despite growing pushback against this (especially given the legal troubles of several IFI leaders in the past two decades), this informal agreement remains. Leading shareholders commonly have the largest influence over the overall policy direction, as well as day-to-day operations, through a range of formal and informal channels. In the IMF, World Bank, and IDB the top shareholder is the US, which enjoys veto power over major policy decisions in all three organizations. All are conveniently located within walking distance of the US Treasury—the branch of government in charge of US policy toward IFIs—and US officials enjoy an unparalleled level of regular contact with the management of these three organizations.

Much of shareholder influence occurs behind the scenes, in what has come to be known as “informal governance” (Stone 2011, 2013). For example, evidence on the IMF, the World Bank, and the Asian Development Bank have revealed that powerful countries steer IFIs toward preferential treatment of strategically important countries, like UN Security Council temporary members or major trading partners (Dreher, Sturm, and Vreeland 2009, 2015; Kilby 2006, 2011; Fleck and Kilby 2006; Lim and Vreeland 2013; Clark and Dolan 2020). This is commonly attempted through informal avenues through which shareholders seek to build broader coalitions for their preferred policies, inform IFI management about their priorities and desires, and even lobby IFI staff directly.

Informal governance is greatly enhanced during periods of negotiation over large contributions to IFI resources. Such negotiations occur among shareholder governments and between shareholders and IFI management, and fall into two categories. First, there are negotiations over increases in the overall size of IFIs, known as “general capital increases” in the MDBs and “quota increases” in the IMF. Second, there are negotiations over restoring the resources of the subsidized MDB lending windows that provide loans and grants to the poorest countries, such as IDA in the World Bank, and the Fund for Special Operations (FSO) in the IDB. Capital increases are rarer than replenishments: for example, the World Bank has had six capital increases since its founding (in 1959, 1979, 1988, 2010, and 2018), but 20 replenishments of IDA, its concessional window.

Because capital increase and replenishment negotiations are about financial contributions, they naturally exclude the voices of poorer member governments. Among wealthy governments, the US stands out as an especially effective activist shareholder. Enabling US activism is not only the large size of the country’s contributions to the most important IFIs, but also the divided structure of its political system. In contrast to other leading shareholders in the major IFIs (that is, large economies in the Global North), the US has a system in which authority over IFI policy is divided between the Executive and Congress. This constrains the Executive, but also allows it to use Congress as a bargaining chip—in

effect telling IFI management and other shareholders that failure to concede to US demands will lead to Congress failing to appropriate the needed funds (Babb 2009).

One particularly striking instance of US shareholder activism occurred during the mid-1980s, when the US urged the Washington-based IFIs to engage in coordinated lending for “Washington Consensus” free-market policy reforms (Babb 2009; Kentikelenis and Babb 2019). In the case of the IDB, whereas major shareholders, the IMF and World Bank backed this plan, the IDB’s management and its Latin American directors resisted. In response, for more than two years the US held up an IDB replenishment and capital increase, and even suspended its previously promised contributions. The funds were only unblocked once the IDB agreed to participate in the Washington Consensus, to engage in significant organizational restructuring, and to change its voting rules to be more receptive to US influence (Babb 2021).

The story of the IDB’s failed resistance illustrates a fundamental trade-off IFIs face between autonomy and broad member participation, on the one hand, and dependence on wealthy shareholders, on the other (Krasner 1981; Ray and Kamal 2019). The smaller, poorer, and less influential an IFI is, the more autonomous and democratic it can be. On one end of the autonomy-resources spectrum are the “minilateral” sub-regional banks, which have limited funds, and are mostly controlled by the countries that also borrow from them (Humphrey 2019). On the other end of the spectrum are the IMF and World Bank—well-financed behemoths that work closely with leading shareholders.

Occasionally, an IFI may move along this spectrum over time. For example, both the IDB and the African Development Bank (AfDB) began as relatively autonomous banks in which developing-country members had considerable influence (Tussie 1995; English and Mule 1996). As described above, the former was reined in during the 1980s: when given the choice between being a “small bank” with autonomy or a “big bank” that was more responsive to shareholders, the IDB chose the latter (Tussie 1995, 31). The AfDB moved in the same direction, but in more incremental stages. At its founding in 1964, the bank had only African members, and its resources were therefore quite limited. In the 1970s and early 1980s, the AfDB began to let in non-regional members as a way of gaining access to wealthy-country resources (Sanford 1982, 53–4). In the 1990s, with its finances devastated by the Third World debt crisis, the AfDB was bailed out by a group of wealthy donor countries, conditional on major reforms, which, among other things, gave its wealthiest members a veto over major decisions (Babb 2009, 31).

Sometimes the IFIs’ wealthiest shareholders cooperate among themselves to approve particular loans or promote broader policies (Kentikelenis and Babb 2019). In other cases, however, shareholder agendas may diverge. In the MDBs, an increasingly popular strategy to manage such divergence is to channel resources through “trust funds” earmarked for particular activities, which may be financed and governed by a smaller subset of shareholder governments. By 2017, the World Bank was managing more than a thousand such trust funds (Reinsberg, Michaelowa, and Knack 2017).

Whether it is through trust funds or traditional lending windows, wealthy governments have the biggest say in IFI activities. Revamping the governance arrangements of Western-based IFIs has been a long-term objective of developing countries, yet the various reform rounds have only moderately rebalanced voting shares while still protecting the power of countries in the Global North (Vestergaard and Wade 2013; Buira 2005). Reflecting such frustrations, as mentioned above, China and other powerful developing economies set up new IFIs, with the aim of presenting alternative options to the Northern-dominated

institutions. Despite the promise of these IFIs giving more voice to borrowers, their governance arrangements have thus far mirrored those of established IFIs. In the case of AIIB, voting shares are heavily skewed toward China (30.8 percent of shares) and its major developing-country partners, like India (8.6 percent) and Russia (6.8 percent), but also to high-income countries, like Germany (4.6 percent) and France (3.5 percent). The reason for the high voting shares of non-regional countries is to tap wealthy-country resources, as well as to bolster the AIIB image as a multilateral bank (Kaya and Woo 2021).

IFIs AND THEIR DISCONTENTS

Given the power that IFIs have to act as major lenders, shape development trajectories, legitimate economic ideas, and underpin multilateral economic coordination, it is no surprise that their activities have attracted persistent attention and debate. While covering the full range of these discussions is beyond the remit of this chapter, below we lay out two of the biggest academic and political controversies: the social and environmental impact of IFI activities; and IFIs' imposition on "policy space."

Social and Environmental Impact

For decades, it has been well known that IFI activities can harm the natural environment and adversely impact local communities. Perhaps the best-known culprits are large MDB project loans for development projects such as highways and dams—especially those sponsored by the highly visible World Bank (Shandra, Shircliff, and London 2011). Such projects have long been targeted by activists, including those in wealthy shareholder countries, decrying both their negative environmental impacts and their displacement of vulnerable populations. Under political pressure from wealthy-government lawmakers, the World Bank (along with some other MDBs) adopted environmental safeguards, and began to consult with civil society groups on the impact of their projects (Wade 1997; Rich 1994; Fox and Brown 1998; Nielson and Tierney 2003). However, because the MDBs have a powerful financial incentive to get loans approved, the real impact of such innovations can be circumscribed (Goldman 2005).

A distinct but equally important set of environmental and social concerns arise from *program* lending—that is to say, loans not intended to finance projects, but to incentivize particular kinds of policies, through conditionality. Compared to the highly visible social and environmental damage caused by MDB projects—homeless villagers, flooded valleys, denuded forests, and so on—the social and environmental hazards of program loans are less perceptible to the untrained eye, as they entail can longer causal pathways: macro-level policy change—like increased taxes, deregulated economic activities, or liberalized trade—is often less proximate to affected communities than the highly visible dislocation that investment projects can wield. Nonetheless, such policies have can have profound social impacts, as they alter the ways in which the state and businesses make economic decisions, approach individual and social rights, and treat the environment.

The IFI that has the most experience with conditionality is the IMF, which has been attaching policy conditions to its loans since the 1950s (Babb 2007). As noted earlier,

standard IMF conditions include fiscal consolidation (more commonly known as austerity), privatizations of natural resources or state-owned enterprises, and labor market deregulation. The impact of such measures on socioeconomic, environmental, health, and educational outcomes has been the focus of sustained debate with the focus being on two issues. First, to what degree is the impact of these reforms independent of the impact of parallel financial crises or past economic mismanagement? Indeed, the IMF often fashions itself as a doctor who treats a patient with bitter medicine that is nonetheless essential for a return to full health. But such a narrative only serves to minimize the agency of the organization: the scope of its economic advice may not be the most appropriate to help ailing economies to recover as fast as possible, and this advice might have a range of adverse side-effects of its own. The second debate follows on from the first in that it seeks a more fine-grained understanding of when, where, and how different conditions impact socioeconomic outcomes. That is, rather than referring to IMF conditionalities on the undifferentiated aggregate, scholars have tried to isolate the impact of individual reforms mandated in loan—for example, the links between labor market conditionalities on public sector wages (Rickard and Caraway 2018).

These debates—and their various methodological implications—have attracted persistent scholarly attention, reviewed recently elsewhere (see Dreher and Lang 2019; Forster et al. 2020; Kentikelenis 2017; Lang 2020; Stubbs et al. 2018; Vreeland 2003, 2006, 2007). But, zooming out, the main finding of these analyses is that conditionalities tend to exacerbate pre-existing economic problems due to reforms being mistimed, ill-conceived, or excessive, and this has a range of follow-on adverse implications for societies. Indeed, ongoing policy debate—even within IFIs themselves—is focused on how to revamp financial assistance in order to minimize its negative side-effects and aid countries in meeting inclusive and sustainable development targets (Kentikelenis and Stubbs 2021).

The Impact on Policy Space

Beyond the immediate social and environmental consequences of IFI lending, another, more structural, way of assessing the role of IFIs focuses on how they shape the “policy space” available to countries to pursue their chosen development strategies. This term refers to a government’s ability—especially over the medium and long term, given that crises and ensuing IFI lending can be associated with short-term constraints—to select their desired policy mix to underpin development efforts or overcome economic problems (Gallagher 2005; Chang 2006).

The constraints that IFIs place on policy space are most direct in the case of lending conditionalities—particularly since the onset of “structural adjustment” programs which sought to promote market liberalization in the developing world (Simmons, Dobbin, and Garrett 2008; Babb and Kentikelenis 2021). Such policies were initially rolled out in Latin America and Sub-Saharan Africa in the aftermath of the Third World debt crisis of the 1980s, and were subsequently vigorously promoted in Eastern European countries after the collapse of the Soviet Union (Hanley, King, and Tóth 2002). They entailed unprecedented levels of collaboration between the IMF, MDBs, and bilateral aid programs of wealthy countries. In addition to standard IMF macroeconomic reforms, loans were tied to such policies as extensive privatizations, rapid economic deregulation, the liberalization of trade flows, and

cuts to the number or pay of civil servants and to the availability of public services. In other words, the power to impose policy reform conditions as a precondition for loan tranches gave IFIs extensive power over reshaping the domestic political-economic environments of borrowers.

Of course, not all countries comply immediately with IFI policy prescriptions. In fact, they often do not: for example, more than half of IMF programs experience interruptions, whether temporary or permanent, due to the non-implementation of reforms (Reinsberg, Stubbs, and Kentikelenis 2021b). This can be highly costly for countries. Failing to uphold IMF conditions can prevent countries from accessing the resources of the World Bank, other MDBs, and bilateral aid donors (Dijkstra 2002). It may also lead to capital flight and foreign disinvestment: being in the IMF's good graces serves as a "stamp of approval," signaling to banks and foreign investors that a country is working to overcome economic difficulties. For these reasons, it is common for countries that lapse into noncompliance to ultimately return to the IMF for a fresh loan with new conditionalities (Reinsberg, Stubbs, and Kentikelenis 2021a).

Such policies have contributed to "hollowing out" state infrastructures; for example, by defunding certain areas of policy action or by adversely impacting levels or effectiveness of public sector staff (Reinsberg et al. 2019). These issues matter because bureaucratic quality and state capacity are essential for countries seeking to implement ambitious economic development plans (Evans and Rauch 1999). By undermining, bypassing, or defunding public institutions, IFIs limit the policy options of future policymakers who will have to govern states with stunted or declining administrative abilities, thereby making the return to IFIs for funding or technical assistance more likely.

In addition to lending, the epistemic authority of IFIs also serves to limit the scope of "legitimate" policy choices for developing countries. By corraling their extensive expertise and agenda-shaping capacity, IFIs seek to diffuse policy norms and scripts about what is the most effective and appropriate policy action for a range of policy problems—from the management of cross-border capital flows to the financing of social protection systems, and from domestic tax policy to the operation of central banks (Polillo and Guillén 2005; Henisz, Zelner, and Guillén 2005; Kentikelenis and Seabrooke 2017). In bolstering the legitimacy of such policies, it helps that these organizations are staffed by highly trained officials—often holding graduate degrees from elite universities—who write influential reports on development strategies (Wade 1996), and who develop a host of indicators to measure development performance (Broome, Homolar, and Kranke 2017; Broome and Quirk 2015; Doshi, Kelley, and Simmons 2019) and generate economic forecasts (Kentikelenis and Stubbs 2021). In turn, these reports, policy papers, forecasts, and indicators shape the range of policy options that countries—especially those with more limited resources—consider legitimate, and that global capital markets come to expect as appropriate (Simmons, Dobbin, and Garrett 2008).

DIRECTIONS FOR FUTURE RESEARCH

This chapter has sought to provide a broad overview of the state of research on IFIs, and is intended as a point of departure for further academic exploration. In lieu of summarizing the

points made in our analysis, we spell out some promising directions for future research. First, the overwhelming focus of academic scholarship is on the IMF and the World Bank. For example, our search on JSTOR yielded over 77,500 hits for the World Bank, but only 1,301 for the African Development Bank and 1,025 for the European Investment Bank. The attention on the Bretton Woods twins is well justified, given their key role as focal organizations of global economic governance: they provide financial and technical assistance to countries in the Global South, and are loci for the development of rules and norms on international and national economic policy. However, its side-effect has been the relative scholarly neglect of the wide array of IFIs that exist and cater to governments and businesses around the world, and—by extension—affect developmental outcomes. This omission makes this group of international organizations prime targets for increased academic scrutiny. Already a recent strand of work tries to elaborate on these less-studied IFIs—from the smaller MDBs to the regional liquidity providers (Kring and Grimes 2019; Fritz and Mühlich 2019; Humphrey 2019). But there is a long way still to go to develop a fine-grained understanding of the policies and politics of these institutions, and their potential—or lack thereof—to become challengers to status quo IFIs.

Second, the persistent attention of academic work on IFI lending and conditionalities—both on their determinants and their consequences—has meant that other aspects of IFI practices remain insufficiently understood. Technical assistance provides a case in point: many IFIs provide such support to countries and—in the case of the IMF and World Bank—this is a major component of their operations, even though it remains at the discretion of the countries to implement this technical advice. The fact that such assistance is a “soft” output of IFIs, compared to the coercive pressures of lending and conditionalities, should not diminish its ability to still be a powerful vehicle for the diffusion of policies and “best practices” around the world (Simmons, Dobbin, and Garrett 2008).

Third, unpacking the inner workings of IFI governance structures also holds much promise for future research. While academic work has persistently drawn attention to the informal pathways through which shareholders try to steer IFIs, what goes on within their governing bodies remains a black box: Are boardroom deliberations just kabuki theater or consequential in their own right? How do individual board members or coalitions try to shape organizational output? Under what conditions are less powerful shareholders able to influence IFI activities? And what is the relationship between governance structures and staff? Knowledge on these issues is limited and haphazard, even though centrally important for understanding the politics of IFIs and possibilities for their reform.

Finally, knowledge on the independent role of IFI staff is also severely limited—are they just line bureaucrats strictly following organizational policies and guidelines or do they have independent agency? Past research has documented high degrees of uniformity among staff—for example, in the types of training they have received (Nelson 2014; Chwioroth 2014). However, as the skillset relevant for these organizations is changing—whether due to the emergence of new policy issues that are taken on by IFIs, like climate change mitigation and adaptation, or due to the rise of managerialism and consultancies (Seabrooke and Sending 2020; Seabrooke and Nilsson 2015)—future research can examine whether, when and how staff composition matters for organizational policies and their effectiveness.

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CHAPTER 23

FROM SILOS TO BARNS? REGIONAL INSTITUTIONS IN INTERNATIONAL POLITICAL ECONOMY

ETEL SOLINGEN

INTRODUCTION

THREE main substantive puzzles animate International Political Economy (IPE) research on regional economic institutions (REI): how do we explain or interpret the emergence of such institutions, their specific designs, and their effects?¹ These three themes furnish the core intellectual agenda of the field and, to that extent, provide a centripetal focus to otherwise disparate work. Beyond that, however, theoretical, methodological, and epistemological (TME) differences pull the study of REI apart, as in the overall IPE enterprise, minimizing dialogue and cross-fertilization.² As this volume's editors note, IPE scholars rely on their respective TME or comparative advantages to provide insights on favored topics. Positivist studies leaning on qualitative or quantitative data seek improved analysis of causal effects. The explosion of data on trade, investment, and other exchanges enables cumulative statistical knowledge about regional patterns of economic integration. Studies leaning on constructed cultural and normative considerations enable insights into deeper foundations of REI, not necessarily concerned with causal claims or predictive theory.

This chapter proceeds in five sections. The first section examines different strands in the literature on the emergence of REI.³ The second section reviews work geared to understand their design. The third section surveys research on the effects of REI. The fourth section distills some examples where the potential for dialogue across TME approaches might be promising. These include growing attention to the connections between globalization, domestic politics, and REI, and to diffusionary dimensions of REI. The domestic politics of inward-looking economic nationalism and populism across regions have undermined the European Union (EU), North American Free Trade Agreement (NAFTA), and mega-regional arrangements such as the Trans-Pacific Partnership (TPP) and the Transatlantic Trade and Investment Partnership (T-TIP), making this agenda all the more relevant today. The fifth section offers some conclusions and distills further thoughts for future research.

THE EMERGENCE OF REI

Different TME approaches are bound to vary on the definition of REI and indeed the very definition of regions (Katzenstein 2005). The EU provided a core empirical focus of early work on regionalism across the TME spectrum. As an anomaly rather than the norm, it provided a distorted lens through which to examine other experiences (Solingen 1998, 2014). Classical IPE largely neglected the study of REI beyond Europe and transatlantic relations but the empirical repertoire across the TME spectrum has expanded significantly over the last two decades (Hettne and Söderbaum 2000; Tussie and Ruggirozzi 2015). Given their focus on free trade agreements (FTAs) and the latter's dramatic expansion worldwide, positivist studies implicitly adopted a more expansive definition of regions, now paying particular attention to mega-regional and trans-regional FTAs, such as TPP/CPTPP, the Regional Comprehensive Economic Partnership (RCEP), and TTIP. Constructivist studies exhibited as much interest in the EU but also ventured into empirical pastures well beyond it. Whereas positivist work typically focused on legally codified REI, constructivist studies broadened the field to include informal arrangements that emerge through practice.

In the neorealist tradition, hegemony is a leading variable in the creation of REI, conceived as convenient instruments of statecraft, pliable superstructures coating the deeper foundations of power (Waltz 2000). REI can help regional powers expand their hegemony, as Brazil's creation of UNASUL (Tussie and Ruggirozzi 2015) or China's proposed Free Trade Area of the Asia Pacific (FTAAP), arguably to derail the US-led TPP. Alliances may help create freer trade and expand trade flows among allies through REI (Gowa 1994; Long and Leeds 2006; Mansfield and Bronson 1997). REI may also emerge out of efforts to protect against systemic threats such as hegemonic decline or global recession, or to gain advantage in General Agreement on Tariffs and Trade (GATT)/World Trade Organization (WTO) negotiations (Mansfield 1998). Great powers are not always the drivers of REI, however. Stubbs (2014), for instance, explains the Association of Southeast Asian Nations (ASEAN)'s leading role in the emergence of East Asian REIs. TPP itself emerged from the P4 (Singapore, Brunei, Chile, New Zealand). RCEP's genesis is in the Japanese-proposed Comprehensive Economic Partnership for East Asia (CEPEA) and included members of the East Asia Summit (EAS) that had already concluded an FTA with ASEAN.

Neoliberal institutionalist approaches emphasize states' incentives to create REI to manage growing interdependence, overcome collective action, reduce uncertainty, enhance information about preferences and behavior, lower transaction costs responsible for market failure, monitor compliance, detect defections, increase opportunities for cooperation, reduce the costs of retaliation, facilitate issue-linkages, and offer focal points or salient solutions (Keohane 1984; Williamson 1985). Moravcsik's (1998) landmark study found economic interdependence to have been the primary force compelling European democracies to create the European Community. Other studies examined the role of democracy in other regional contexts (Pevehouse 2005, 2016). Mansfield and Milner (2012) found that democracies have greater incentives to conclude FTAs than non-democracies. However, as the number of veto players increases, the likelihood of entering preferential trade agreements (PTAs) decreases. Haggard (1997) found "little evidence that higher levels of interdependence generate demand for deeper integration" or that trade generates prisoners' dilemmas that only

institutions (or hegemons) can resolve. Measuring reductions in transaction-costs *a priori* (or even *a posteriori*) is difficult, especially because one must weigh presumed reductions relative to hypothetical environments without institutions (Kahler 1995). Furthermore, neo-liberal institutionalism has proven less apt for explaining why certain points become “focal” but not others (Johnston 2001) or why some solutions along the Pareto frontier—that leave everybody better off—are adopted over others (Krasner 1991).

Whereas rationalistic studies emphasized exogenous constraints in the emergence of REI, constructivist studies took the role of identity and legitimacy seriously (Hobson and Seabrooke 2007). Risse-Kappen (1996) drew attention to the role of ideas, networks, and communicative action in EU integration. Following Deutsch (1969), Adler and Barnett (2009) emphasized the “sense of community” while Risse (2010) surveyed debates on “common identity” in the EU context. Acharya (1997) emphasized the role of “Asian values” in the creation of ASEAN, a notion that many contested.⁴ Checkel et al. (2016) doubted that collective identity was a precondition for, or driver of regional integration anywhere. Other brands of constructivism drew attention to economic interests as deeper identity sources, arguing that material factors may partially motivate states and domestic actors but historical, cultural, and ideational concerns add directionality to those preferences. National identity, for instance, helps understand the states against which relative success is measured, and from which states seek autonomy (Abdelal 2001). The debacle over Ukraine, after all, had its origins in debates over precisely these categories (Hopf 2016).

Finally, other studies brought into relief the importance of integrating the global, regional, and domestic levels more explicitly in the study of REI (Solingen 1998, 2008, 2014; Solingen and Malnight 2016). Relying on second-image-reversed effects (Gourevitch 1986), one strand traced domestic competition between two ideal-typical models of political survival to economic globalization. Internationalizing and inward-looking models, respectively, span state and private actors and are arguably the deeper drivers of a region’s IPE. A region’s proclivity to advance REI with a commitment to open versus closed economies depends on whether domestic coalitions with internationalizing or inward-looking strategies prevail in that region. For example, internationalizing coalitions seeking to grow their economies via the global economy drove the creation of the ASEAN, the Asia-Pacific Economic Cooperation (APEC), and the ASEAN Regional Forum (ARF). That strategy sought to nurture a stable and peaceful regional environment, minimize military expenditures that burden economic reform, and avoid policies inimical to macroeconomic instability. Internationalizing models invented ASEAN: the incentives to dampen conflict were logically prior to the institution itself. Yet Ravenhill (2008) found liberalization under ASEAN’s auspices to have not been sufficiently significant to encourage business groups to invest substantial resources in lobbying for deeper integration. The logic of internationalizing coalitions is global, not necessarily regional.

THE DESIGN OF REI

REI vary in their degree of institutionalization and their formality, architecture, and decision rules. For neorealism, design is derivative. REI are merely flimsy artifacts, arenas for exercising power, or pliable superstructures subject to changes in relative power.

Nonetheless, power distributional considerations can explain variation in institutional design. Hancock (2009), for instance, explains how smaller states may choose a plutocratic governance structure based on delegating authority to the wealthiest state. Conversely, a regional hegemon may choose to bear the burden of creating institutions that provide side-payments to weaker states acquiescing to hegemony (Grieco 1997). Neoliberal institutionalist studies, by contrast, took issues of design far more seriously, arguing that institutions take different forms and degrees of legalization contingent on the type of collective action problem to be solved (Goldstein et al. 2000; Koremenos, Lipson, and Snidal 2001). Growing economic interdependence, for instance, may account for “thick governance” or designs that are more “binding” (Haftel 2013), although East Asia, and especially a highly interdependent Northeast Asia, remain notable anomalies. According to Lenz and Marks (2016), however, most REI combine low levels of pooling (transfer of authority to the REI) with medium or high levels of delegation (states transfer a conditional grant of authority but retain control over REI).

What makes highly legalized institutions less compelling? The list is long: members’ long time horizons; repetitive gains from cooperation; rampant uncertainty about future benefits; widespread imperfect information and incentives to defect; peer pressure; preference for less public scrutiny; competing bureaucratic pressures foiling formalization; and a preference for flexibility to cope with changing conditions (Lipson 1991; Harris 2000). Likewise, ample information (pivotal to functional accounts) and robust trust (pivotal to norm-based accounts) can obviate the need for formal institutions. REI beyond the EU are thus no empirical anomalies but average practice. Indeed, as EU difficulties remind us, formalization can undermine cooperation, investments are not always crucially about material resources, and talk is not always cheap (Lipson 1991).

Constructivist studies, typically more attuned to informal institutions, trace variation in institutional design to domestic norms and institutional legacies. Differences between informal network states in East Asia, and formal Weberian states in Europe, for instance, yielded different conceptions of REI (Katzenstein 2005). National legal cultures rooted in civil law are arguably more likely to adopt formal rules of dispute settlement (Duina 2006). Domestic constitutional tilts toward the executive enabled informal presidential diplomacy in Mercosur (Malamud 2005). Democratic norms and power-sharing arguably facilitate delegation (Acharya and Johnston 2007). Yet, despite a common Arab identity and norms, Middle East autocrats suppressed calls for deep integration (Barnett and Solingen 2007). Jetschke and Rüländ (2009) found ASEAN’s cultural features antithetical to formalization despite ASEAN’s putative efforts to emulate the EU. Jetschke and Katada (2016) noted an evolving receptivity to greater formalization in ASEAN. Clearly, various factors may explain how changes in the normative landscape may lead to different design features over time, as the EU clearly suggests. For all of the pre and post-Brexit skepticism, however, the view of Europe as a single force in world politics remains very much alive (Moravcsik 2017).

Finally, the nature of dominant political-economy coalitions may explain variations in REI design. East Asian REI are not rigid and legalistic but do reflect compromises and a shared commitment to “open regionalism.” In Goldilocks fashion, their design was “just right,” able to accommodate states that were at diverse stages and exhibited different forms of economic liberalization, export-led growth, and regime-type. The penchant for consensus, informal means to advance confidence building; the plethora of routine meetings; bilateral, plurilateral, and multilateral FTAs; and unilateral liberalization providing self-binding

commitments that facilitate diffuse reciprocity, all were synergistic with efforts to enhance regional stability (Solingen 2008). Conversely, a wide range of shared identities and interests would surely have placed Arab states high on most predictors of regional institutionalization. Yet its dominant coalitions established an Arab League firmly rooted in sovereignty and informality, “just right” for the nature of inward-looking domestic political economies (Barnett and Solingen 2007). The Gulf Cooperation Council (GCC) has intermittently tried to transcend this model in their own sub-region.

THE EFFECTS OF REI

An important strand of work explored the extent to which REI enhance or undermine globalization (Krugman 1993; Bhagwati 1993; Mansfield and Milner 1999). Baldwin (1997) argued that regional PTAs often created a “domino effect” that promoted their enlargement. Mansfield and Reinhardt (2003) saw internationalization as accelerating the pace of commercial regionalism, which, in turn, led to further internationalization. There is hardly a strong consensus on whether PTAs have led to greater trade creation than trade diversion (Kim, Mansfield, and Milner 2016). Statistical studies initially concentrated on FTAs considered to have had concrete effects, largely in Europe, North America, and Asia, a choice that smuggled in selection effects and arguably neglected causal conditions that might have led to ineffective FTAs in other regions. That has changed over the last decade, with a burgeoning research program on FTAs (Kim 2015).

Other work, across TME divides, hypothesized that REI have effects that go well beyond the economic realm. Powers and Goertz (2011) argued that REI lead to the construction of security institutions. Congruent with normative and instrumental convergence around war avoidance and common security, ASEAN took the lead in the creation of the ARF (Johnston 2012). The synergies between economic and security considerations stemming from REI are evident across regions (Solingen 1998, 2008). Others emphasized the effects of REI on democracy, human rights, and environmental policy (Pevehouse 2005; Hafner-Burton 2009; Mansfield and Pevehouse 2000). Constructivist studies paid special attention to the ways in which REI socialize member states and provide resources in order to persuade them to adopt their policy preferences (Broome and Seabrooke 2011). Lenz and Marks (2016) reviewed the consequences of institutional design for peace and security, economic welfare, domestic institutions, and international “actorness.”

An early precursor to the study of REI, Ernst Haas (1958) did not consider formal structures to be good guides for predicting outcomes, acknowledging instead the role of domestic interest group competition, perceived interests, capitalist social democracy, and pluralism as drivers (see also Ruggie et al. 2005). The EU debacle, Brexit, and the election of Trump deepened interest in the *cui bono* (who benefits) from REI. Some find regional arrangements in internationalizing regions to have arguably reinforced the domestic logic of internationalizing coalitions (Solingen 2008). Similarly, regional arrangements in inward-looking regions often reinforce the domestic logic of their inward-looking coalitional referents. Inward-looking populism fueled by America First Trumpism challenged the “fairness” of NAFTA and TPP for the US. Where President Obama saw TPP as an opportunity to shape the rules of the global economy in the twenty-first century collaboratively,

and others saw TPP as a tool to contain China, Trump briskly extricated the US from the TPP process altogether. In the European context, challenges to the EU stemmed from a plethora of concerns, including legitimacy. Different constituencies, however, upheld different normative allegiances. Some constructivist work, and especially critical theory, has emphasized inequalities in power and resources and benign opposition to EU intrusions in national sovereignty. The latter arguably energized more extreme nationalist, xenophobic, and anti-democratic forces. Studies along the TME spectrum highlighted the introduction of the euro as the remote source of EU upheaval in recent years. McNamara (2015) highlighted how the EU legitimated its authority partly by transforming symbols and practices of everyday life, from money to architecture and beyond.

To the extent that East Asian REI can claim substantive effects (a contested proposition), they appear to have privileged dominant coalitional preferences for internationalization, shared by a large and rapidly growing consumer middle class. This was not much different from the effects of more formal NAFTA, EU, and other REI (Moravcsik 1998; Börzel 2015). Given the greater informality of East Asian institutions, and the dramatic expansion of underlying regional economic exchange, one might question whether those effects would have been distinctively different in the absence of those institutions. However, constructivist work notes that APEC arguably helped diffuse normative consensus around “open regionalism” and market-driven liberalization, and that other REI in that region developed consequential “habits of cooperation,” in China for instance (Johnston and Acharya 2007; Johnston 2014). East Asian REI may have constructed an identity pivoted on global markets and institutions that has arguably made it especially resilient, over the last decade, against populist backlash and crisis, although no region is immune to inward-looking turns.

BARNS, NOT SILOS?

Like in most IPE research arenas, more work on REI has taken place in silos than in barns. Silos store only grain. Barns house grain, animals, equipment, and more. The specialized nature of IPE journals has partially driven the entrenchment of ever more towering silos, each of which builds cumulatively on cognate TME foundations. Barns are fewer and far between, yet two research programs, among others, illustrate their potential advantages.

First, there is growing appreciation for the fact that REI are embedded in a broader process of globalization, which affects REI both directly and through domestic politics. As noted earlier, domestic responses to globalization lead to the logrolling of competing coalitions that, in turn, influence various aspects of REI. Coalitional analysis is compatible with a wide range of TME exploring both material and identity-based foundations of coalition formation, proclivities vis-à-vis the global economy, economic nationalism, and REI. Yet, on the one hand, much positivist work has often undertheorized the social construction of nationalism, and the role of cognition, social psychology, emotions, and other sociotropic factors affecting coalitional dynamics in IPE (Solingen and Gourevitch 2016).⁵ On the other hand, constructivist work has largely neglected domestic politics as the arena where the political construction (and destruction) of REI takes place (Checkel 1997, 2016).⁶ Standard constructivist studies of East Asian REI, for instance, have emphasized highly contested “unique” “Asian values,” sovereignty and non-intervention. Ignoring the domestic

foundations of internationalizing identities oriented to the global economy, those studies overlook important influences on the emergence, design, and effects of REI. Perhaps ironically, the concept of “Asianness” may well be an unanticipated regional by-product of internationalizing domestic models prevalent in East Asia. Coalitional clusters, rather than abstract conceptions of states, can create regional identities.

Constructivist work can marshal useful analytical tools for identifying and interpreting domestic coalitional alignments, and for finessing the contextual meanings assigned to REI in different geographic and cultural contexts. In other words, it can help understand the shadow of the past in ways that positivist studies often neglect. A regional dialogue between adversaries coded equally for all regions overlooks important differences. Such dialogues are taken for granted in some contexts (Southern Cone) but symbolize gargantuan cooperative strides in other contexts (North and South Korea, Middle East). Taking the issue of legitimacy seriously, constructivist and critical theory can also help identify the imagined boundaries of regions, and the mechanisms by which domestic coalitions mobilize support for or against reliance on the global economy, for or against economic nationalism, for or against regional economic cooperation. Dominant coalitions vary in their constitution. Even those that are elite-based must persuade broader constituencies to endorse or oppose REI (Seabrooke 2007). Other coalitions emerge more organically bottom-up, from everyday actions by subordinate agents that may resist or affirm the legitimacy of an REI (Hopf 2013). REI use persuasion, socialization, shaming, and other mechanisms to enhance or diminish the appeal of competing coalitions, as Johnston and Acharya (2007) noted.

Tighter collaboration in this research program can also help elucidate scope conditions under which different conceptualizations of REI may offer better insights. One may surmise, for instance, that in some cases it may be difficult to establish clearly, *a priori*, the consequences of creating or designing REI for the distribution of power among regional states, or the potential of REI for reducing transaction costs. If normative implications are more transparent, they are likely to offer greater analytical potential. Conversely, in other cases it may be harder to capture the normative implications than those related to power or economic efficiency considerations. Moreover, the weight of different causal conditions affecting the creation, design, and outcomes of REI may vary for different regions. Such variation might render the effects of different causal conditions negligible or sizable, depending on the context. The point is that universal models sometimes provide good answers to core conceptual issues in the study of REI, while at other times intersubjective constructs illuminate fundamental dynamics at work. The potential gains from collaboration should not be underestimated.

A second example of an emerging “barn” is the application of diffusion analysis to understand the extent to which REI models and blueprints travel within and beyond regions. This is part of a broader disciplinary interest in diffusion across TME and their respective efforts to understand the anatomy of diffusionary processes.⁷ TME camps tend to focus on different causal mechanisms, with rationalist studies emphasizing competition and constructivist work dwelling often on emulation. Several studies addressed the extent to which the EU has served as an inspiration for other regional schemes through learning, emulation, or other mechanisms (Börzel and Risse 2012; Jetschke and Lenz 2013; Risse 2016). Other studies examined how the EU and NAFTA triggered the emergence of other PTAs in the Americas and East Asia (Baldwin 1995; Solís, Stallings, and Katada 2009) or how US-led PTAs diffused throughout sub-regions of Latin America, especially among Pacific Alliance members

(Quiliconi 2014). As inward-looking models (Nasserism, Ba'athism) diffused throughout the Middle East in the 1960s, primarily through coercion and emulation, they also entrenched REI forms antithetical to regional or global economic integration (Solingen 2008). In East Asia, by contrast, the progressive diffusion of successful export-oriented models, famously captured by the “flying geese” metaphor, led to REI attuned to “open regionalism” (Pempel 1997). APEC, for instance, helped diffused normative consensus around “open regionalism” and market-driven liberalization.

Other studies focused on barriers to diffusion, or firewalls. Culture can explain why international policies or norms do not diffuse automatically or consistently across regions (Gurowitz 1999). Domestic firewalls—dominant coalitions, autocratic institutions—can also help explain the extent to which REI may diffuse to some regions but not others (Solingen 1998, 2012). Studies of diffusion have only recently begun integrating domestic, regional, and global levels of analysis under a common theoretical framework. The study of REI may benefit from improved understandings of when a particular level dominates; when different levels are mutually reinforcing or mutually exclusive; when they are antecedent or catalytic conditions for the diffusion of REI forms; and how we can avoid the pitfalls of overstating one level or the other. Ideational studies sometimes dwell solely on the uniqueness of a region where positivist studies sometimes overstate their commonalities. The field can make further progress in identifying aspects of REI that diffuse more (or less) commonly from one region to another, spatial diffusion patterns, and mechanisms and firewalls preventing diffusion across regions. “Mimetic isomorphism” is rarely random. Some regions may be frequent senders (or receivers) of scripts on what REI should be about.

One of the most crucial developments in contemporary IPE is the diffusion of inward-looking nationalist models into the domestic politics of states in various regions. As noted, those models denounce globalization and have crucial implications for the emergence, design, and decay of REI. Anti-EU left or right populism has captured power at different points from Greece to Hungary, Poland, and the Czech Republic, and has grown stronger nearly everywhere else in Europe. In the Southern Cone, the diffusion of those models led to the decline of Mercosur in all but name (Malnight and Solingen 2014). In North America, Trump’s attacks on NAFTA, Mexico, and Mexicans has influenced the outcome of Mexico’s 2018 elections.⁸ In a typical instance of contagion, Andrés Manuel López Obrador organized a nationalist populist coalition of right and left. By contrast, despite some inroads into Asian countries (e.g., Philippines), leaders of the remaining 11 partners to the former TPP have thus far erected an important firewall against Trumpism, refashioning TPP into the Comprehensive and Progressive Agreement for TPP (CPTPP), which entered into force in December 2018.

CONCLUSIONS

The explosion of research on REI over the last two decades has led to a richer understanding of why they emerge, what form they take, and what effects they have. As this chapter suggests, research on REI is not a monopoly of any particular TME approach to IPE. Even when dialogue is scarce, the menu is richer and the insights more expansive, leaning not merely on standard political science and economics but on sociology, psychology, and critical theory

as well. Yet, as in IPE as a whole, REI studies cluster in silos more often than barns, although this chapter has highlighted some research programs with potential for fostering barns. Overall, exclusive attention to power, economic efficiency, transaction costs, and normative diffusion—the common analytical currency in standard accounts of REI—may conceal deeper domestic drivers underlying REI dynamics. Domestic frameworks attentive to broader IPE and CPE themes—including responses to globalization, the rise of nationalism and populism, and related coalitional shifts—provide important insights on why REI emerge, in whose interest, and with what effects.

Finally, it is important to note that the study of *the IPE of regions* is much broader than the study of REI. One should not exaggerate the causal effect (or interpretive potential) of REI for regional economic orders. REI may explain far less of a region's economic exchange and cooperation than do underlying socioeconomic forces and everyday politics. Regional economic cooperation may occur without formal (and even informal) institutions, and conflict can arise despite their presence. Notwithstanding common language and shared notions of Arab nationality, history, and culture, inward-looking models turned the oldest regional institution (the Arab League) into a centrifugal force. By contrast, internationalizing models facilitated dramatic expansion of regional economic exchange, as well exchange with the rest of the world, even in the absence of legalized REI.⁹ The underlying logic of internationalizing regional orders is global. Economic and political cooperation does not necessarily hinge on institutionalized interdependence—via REI—but on stability, foreign investment, and global economic access. More legalized institutions may—but need not—emerge as side-products of prior economic regionalization. Indeed, some have questioned the extent to which REI add much to what a region's state of affairs might have been in their absence (Kahler 1995; Solingen and Malnight 2016). An improved understanding of REI is not equivalent to an improved understanding of the broader, logically prior, nature of regional orders. Institutions are endogenous to regional orders and one can hardly understand REI except against that wider background. This is not to say that REI have no reciprocal effects on the IPE of regions, but those effects remain contested, especially beyond Europe and North America. REI may not be entirely epiphenomenal to underlying regional economic orders, but neither do they encode a region's complete DNA. Future research must restore this broader focus in the study of REI.

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NOTES

1. For an overview of that literature prior to 2008, see Solingen (2008). The concise nature of this chapter precludes the inclusion of a vast literature published since, reviewed in Mansfield and Solingen (2010), Solingen (2014), and Börzel and Risse (2016).

2. For a broader discussion of intellectual divides in IPE, see Cohen (2007) and Phillips and Weaver (2010).
3. Some studies refer to “regionalism” as the study of REI driven by political forces and “regionalization” as increased regional economic exchange driven by market forces. However, this obscures the ways in which politics underlies regionalization and markets can create conditions for the emergence and design of institutions.
4. “Asian values” and “ASEAN way” could not explain both earlier periods of militarized conflict and the subsequent absence of war. Furthermore, East Asia is not at all culturally homogeneous.
5. Some exceptions include Hooghe and Marks (2005) and Mansfield and Mutz (2009).
6. Although not addressing REI, Herrera (2005) highlighted how material conditions can underdetermine understandings of the economy, forcing attention to the role of imagination and intersubjectivity.
7. For a summary of this vast literature across TME, see Solingen (2012).
8. As the architect of the most extreme inward-looking administration since the Second World War, Trump declared: “We hear a lot of talk about how we are becoming a ‘globalized world.’ But the relationships that people value in this country are local.” http://www.realclearpolitics.com/video/2016/12/01/trump_there_is_no_global_flag_no_global_currency_no_global_citizenship_we_are_united_as_americans.html
9. Inter-Arab trade accounted for 7–10 percent of total trade since the 1950s (United Nations Development Programme 2002, 126). East Asian economies exported over 52 percent of their total exports to each other on average (2000–2008); Middle East economies less than 16 percent (International Monetary Fund, Direction of Trade Statistics, <http://www.imf.org>).

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CHAPTER 24

THE ECONOMICS AND POLITICS OF PREFERENTIAL TRADE AGREEMENTS

LEONARDO BACCINI

THE proliferation of preferential trade agreements (PTAs) is a key feature of this current wave of globalization.¹ To date there are 609 PTAs currently in force, whereas their number was 129 before 1990.² Both developed and developing countries are deeply involved in preferential trade liberalization and they are currently negotiating a large number of PTAs. While tariff reductions on a preferential basis are central to the formation of trade agreements, the inclusion of trade-related issues—provisions protecting foreign investment (FDI), for instance—have become increasingly common in PTA treaties. In fact, many PTAs regulate trade-related issues more extensively and more stringently than does the World Trade Organization (WTO). Simply put, preferential liberalization is the main instrument of trade policy and trade governance nowadays.

Given the importance of the phenomenon, it is no surprise that scholars have devoted increasing attention to the causes and consequences of preferential liberalization. According to the WordCat database, the yearly average number of peer review articles on trade agreements has been 2,000 over the past 10 years.³ Importantly, while they have long been considered a rather technical issue exclusively relevant for a small circle of academics and policy wonks, PTAs are now central to the political debate in both the US and European countries. The heated discussion over the negotiations of two mega-regions, that is, the Transpacific Partnership (TPP) and the Transatlantic Trade and Investment Partnership (TTIP), has recently testified to the political salience of this issue.

In this chapter, I survey the main and most recent contributions to the literature on trade agreements. This body of research is so extensive that covering all the relevant studies would be a daunting and unfeasible task. To limit the scope of my literature review, I will mostly focus on contributions made by International Political Economy (IPE) scholars and, to a lesser extent, by economists.

There are four take-home messages from this large collection of scholarly work. First, studies on PTAs tend to overwhelmingly fall into the positivist/rationalist tradition. Second, empirical contributions on the causes and the effects of PTAs tend to dominate over

theoretical contributions. Third, we see frequent intellectual interactions between political scientists and economists in this area of research, given that they both employ similar methodologies. Fourth, the two main journals in which the most important contributions on PTAs are usually published are *International Organization* (IO) and the *Journal of International Economics* (JIE), according to my review of the literature. The *Review of International Organization* (ROIO) has also emerged as a crucial outlet for studies on PTAs, attesting to the vibrant scholarly overlapping on this topic between political science and economics.⁴

The chapter is divided into four sections. In the first section I review the literature, explaining why countries form PTAs. In the second section I survey studies exploring the consequences of preferential trade liberalization. The third section covers the literature on the design of PTAs. A final section concludes with some consideration on the fate of preferential trade liberalization in the wake of growing nationalism and protectionism.

WHY DO STATES FORM PREFERENTIAL TRADE AGREEMENTS?

Due to the salience of the topic there is no shortage of explanations on why countries form PTAs. To get a handle on this vast number of studies, I divide this literature into three bodies based on the nature of the main explanatory variable(s): economic determinants, domestic politics, and systemic variables. I examine each of these bodies in turn.

The Economic Determinants of Trade Agreements

The economic literature has initially focused on the gravity model variables, which are proxies of trade costs and trade benefits, to explain the formation of PTAs. Using the natural trading bloc argument, Krugman (1991) argues that preferential trade agreements are welfare-enhancing when they match countries that should “naturally” be economically intertwined with each other because of proximity. Using a Krugman-type general equilibrium monopolistic competition model of international trade, Baier and Bergstrand (2004) provide the first systematic analysis of the economic determinants of the formation of PTAs. They find that pairs of countries are more likely to form a trade agreement: (1) the closer the two countries are geographically, (2) the more remote a pair of natural trading partners is from the rest of the world, and (3) the larger and more similar in economic size two trading partners are. Their model correctly predicts a large number of PTAs, but several country-pairs show evidence of excessive or insufficient bilateralism, unveiling the importance of other explanations in addition to geographical and economic variables.

While Baier and Bergstrand (2004) focus on the macro-economic determinants of PTAs, several studies look at their distributional effects, which inform theories and analyses of the political economy of trade agreements. The unifying theme of this body of research is that sectoral cleavages supporting or opposing PTAs arise between winners and losers from

trade agreements (Grossman and Helpman 1995; Hathaway 1998; McGillivray 2004). Much of this literature exploits variation *across industries* to explain support for or opposition to preferential trade liberalization and focuses either on intra-industry trade (IIT) or on global value chains (GVCs) as main explanatory variables.

Seminal papers in IPE make the argument that IIT is associated with lower adjustment costs and so greater support for trade liberalization (Lipson 1982; Milner 1997, 1999). According to these studies, IIT may reduce direct competition between products due to product differentiation, hence lowering the number of domestic companies that perceive foreign imports as a threat. While the argument is straightforward, the empirical evidence remains mixed (Gilligan 1997; Kono 2009). For instance, Manger (2012, 2015) finds that vertical IIT increases the probability of North-South PTAs and that tariff reductions are larger in industries with high levels of IIT, though this second analysis relies only on trade agreements signed by the US.⁵ On the contrary, using a newly-collected dataset on preferential tariffs of 61 PTAs, Baccini, Dür, and Elsig (2017) find little evidence that intra-industry trade speeds up preferential trade liberalization.

The effect of GVCs on trade agreements is related to companies' increasing reliance on imports of intermediate goods, that is, goods that are sourced for the purpose of serving as inputs for the production of other goods. Preferential tariffs allow firms to cut production costs when re-importing these products back to the home market. Empirical evidence showing that GVCs are a key driver of PTAs abounds. Chase's pioneering work (2003, 2005) finds that support for the North America Free Trade Agreement (NAFTA) varies across industries, depending on economies of scale and firms' ability to move production across borders. Baccini, Dür, and Elsig (2017) find that tariffs on intermediates are cut quicker than tariffs on finished goods, looking at all trade agreements formed by major trade entities after 1995.

Firm-level analyses confirm the micro-foundation of the relationship between PTAs and GVCs. Manger (2009) argues that the growth of PTAs reflects the interests of firms with production operations spread around the world and finds qualitative evidence in support of his argument. Blanchard and Matschke (2015) provide extensive and compelling evidence that offshoring activities lead to greater preferential concessions, using firm-level data of US subsidiaries operating in host markets. While GVCs create the demand for PTAs, sourcing generates strong political conflicts not only among firms, but also among workers. For instance, Osgood (2017) finds that vertical integration is a key driver of industrial fragmentation over preferential trade liberalization. Regarding labor market effects, Owen (2017) shows that US legislators are significantly less likely to vote in favor of trade agreements in counties that are heavily affected by jobs being moved offshore.

As it is evident from this section, the political economy of trade agreements has increasingly relied on new micro-level data to identify the key supporters of the formation of PTAs. The take-home message from this literature is that the actors pushing for preferential liberalization are the largest exporters and multinationals (MNCs), as empirically demonstrated by Osgood et al. (2017) in a case study on the support for the US-Central America Free Trade Agreement among Costa Rican firms. While perhaps not surprising, these results provide a powerful tool to understand the growing opposition to preferential trade liberalization. What is left to know is whether gains from trade really materialize for large exporters and MNCs after forming PTAs, a point to which I will return below.

Domestic Politics and the Formation of Preferential Trade Agreements

The body of literature focusing on the effect of domestic politics on the formation of PTAs builds on the credible commitment argument. In a nutshell, governments have incentives to sign trade agreements by their desire to commit vis-à-vis domestic industrial lobbies (Maggi and Rodriguez-Clare 1998, 2007). Incentives to do so vary depending on the institutional environment in which governments and leaders compete.

The seminal study linking domestic institutions to the formation of preferential trade agreements is by Mansfield, Milner, and Rosendorff (2002). Their first and main contribution concerns the relationship between democracies and PTAs. Their theory argues that democratic governments, which face competitive elections, may use trade agreements as signaling devices to domestic constituents. Voters with little information about government policies may blame democratic leaders for their welfare loss—due to the high price of consumer goods, for instance—even if this welfare loss is a result of exogenous shocks, for example a systemic crisis, and not of rent-seeking policies implemented by the government. Forming a preferential trade agreement, which constrains a government's behavior through international commitments observable by voters, may signal that democratic leaders are truly committed to implementing welfare-enhancing policies rather than rent-seeking policies. In turn, this signaling device may increase a democratic leader's probability of keeping the office, since she would be less likely to take the blame in the case of welfare losses faced by voters due to economic downturns.

The empirical evidence showing that pairs of democracies are significantly more likely to form PTAs than pairs of autocracies is very robust. Mansfield and Milner's (2012) book confirms the results of the original paper in *International Organization* with a larger sample of trade agreements and tests a new set of auxiliary hypotheses. In particular, Mansfield and Milner show that the positive relationship between democratic regimes and probability of forming PTAs is stronger for left-wing governments and for democracies that depend heavily on trade. Furthermore, they find that democracies tend to form PTAs with a dispute settlement mechanism (DSM) to strengthen their enforcement. In a recent paper, Mansfield and Milner (2017) find also that democracies are particularly likely to form PTAs in periods of economic crisis, confirming the argument that PTAs help democratic leaders to stay in power in hard times.

The second important contribution to the literature of the formation of trade agreements by Mansfield and Milner concerns the role of veto players. In an article published in *International Organization* together with Mansfield, Milner, and Pevehouse (in 2007), and in their book (2012), Mansfield and Milner argue that as the number of veto players increases, so does the probability that at least one such player loses from the PTA and, in turn, vetoes its ratification. This increases the cost of ratifying PTAs for leaders facing many veto players. The greater these costs, the less likely leaders are to enter into trade agreements and the more difficult it will be to secure ratification. Mansfield, Milner, and Pevehouse find robust evidence of a negative relationship between the probability of forming PTAs and the presence of a large number of veto players. Moreover, Mansfield and Milner show that veto players increase the length of time it takes to ratify PTAs and that a large number of veto players reduces the probability of including a DSM in the design of trade agreements.

While these studies have convincingly showed the importance of domestic politics on the formation of PTAs, they do not say much about the timing in which agreements are signed. Indeed, both type of regime and presence of veto players are low time-varying variables, whereas the likelihood of countries forming PTAs has spiked in the past two decades. Recent political events have also highlighted that not all voters favor preferential trade liberalization.

Baccini and Urpelainen (2014b, 2014c) put forward an argument that captures the dynamics of the PTA formation. In particular, they argue that new leaders in democratizing countries are under pressure to implement reform, which allows them to deliver fast economic growth and to stay in power. However, the same new leaders may not have the consolidated power to overcome domestic opposition to reform, compared to leaders with longer tenure. Signing into PTAs with major power allows new leaders in democratizing countries to credibly commit to liberal policies through legally binding provisions included in PTAs. Moreover, PTAs create domestic political support for reform by providing economic benefits to export-oriented constituencies through preferential market access and side payments to losers—higher inflows of foreign aid from the European Union (EU) and the US, for instance (Baccini and Urpelainen 2012). Using a mixed-method approach, Baccini and Urpelainen (2014c) show that the interaction between new leadership and democratization has a substantive positive impact on the probability of developing countries to form deep PTAs with the EU and the US.

In line with much of the literature in International Relations (IR), the debate on the domestic political determinants of PTAs focuses mostly on the distinction between democracies and autocracies. However, the literature in comparative politics has recently highlighted important variation of domestic institutions among autocratic regimes. Surprisingly, only a few studies have taken up this challenge and explored the variation across autocratic regimes in the probability of forming PTAs. Take China for instance, the country has been signing a large number of trade agreements over the past two decades, trade liberalization being one of the trademarks of its autocratic regime.

In line with their argument on democracies, Mansfield and Milner (2012) show that autocracies with high domestic political competition, like China, are more likely to sign trade agreements than autocracies with low and null domestic political competition. In a recent paper, Baccini and Chow (2017) show that new leader vulnerability matters not only in democracies but also in autocracies. In particular, new autocratic leaders who enter into power illegally, for example through a coup, are more vulnerable to elite-led coups than leaders who enter into power legally, for example through succession. In turn, extralegal leaders have incentives to use PTAs both to build support from exporters and pressure disloyal importers. Empirical evidence confirms that extralegal leaders, especially those facing high risk of coups, are more likely to form PTAs than legal leaders in autocratic regimes.

The Role of Systemic Variables in the Formation of Trade Agreements

Systemic variables have been always central to theories explaining the proliferation of international agreements in general, and international economic agreements in particular (Mansfield 1993). This body of research can be divided into two groups: (1) studies focusing

on the link between security and trade cooperation, and (2) studies focusing on the interdependence of PTA formation.

First, the spread of preferential trade agreements may result from the need for counterbalancing the trade-policy choices of other countries. Neorealist IR theory argues that the anarchic structure of the international system makes states apprehensive of increases in the power of other states, as these states may use their new capabilities to attack and defeat them (Waltz 1979). Gowa (1995) applies this neorealist view to the realm of PTAs. She contends that whenever preferential trade agreements stimulate trade flows between two countries, they lead to a more efficient allocation of resources and thus free up some resources for military use. Countries are well aware of the security externalities coming from commerce and are therefore more likely to form PTAs with political allies. In doing so, countries are able to bolster their military capability, to achieve common security goals, and to minimize the risk of empowering political enemies.

Empirical evidence supporting the argument that allies are particular likely to form PTAs abounds. Moreover, a casual look at some agreements unveils the importance of security in explaining the formation of some PTAs. The first trade agreement signed by the US was with Israel in 1985 and was heavily motivated by an effort to provide military support to a key ally. Since then the US has signed PTAs with Jordan and Morocco, which are hardly important trading partners, but obviously important political allies.

Second, several studies point out that trade agreements are interdependent. In other words, in deciding whether to form PTAs countries take into consideration the trade policies implemented by competitors and peers. This stream of research builds on a larger literature in political science arguing that policies diffuse from one country to another through mechanisms of learning, emulation, and competition (Dobbin, Simmons, and Garrett 2007). Learning is linked to the perceived success of the trade policies of one or several countries, which may thus lead others to adopt similar policies (Krueger 1997). The economic success of the member countries of the European Economic Community, for example, may have motivated economic integration among countries in Latin America and Africa in the 1960s (Pomfret 2001).

Competition is by far the mechanism that received the most thorough scrutiny in this body of research. On this view, preferential trade agreements impose costs on excluded countries, in the form of trade and investment diversion, making the latter eager to join or to set up a rival agreement. Following this line of reasoning, Kenneth Oye argues that discriminatory trade policies in the 1930s and the 1980s had the unintended consequence of promoting further openness (Oye 1993). There is also strong evidence to suggest that NAFTA was a consequence of Mexico's reaction to the creation of the Canada-US free trade agreement in 1988 (Gruber 2000). Similarly, NAFTA and the trade and investment diversion that it may have caused was a major stimulus for both the EU and Japan to conclude a trade agreement with Mexico (Manger 2005; Dür 2007).

Quantitative evidence confirms the importance of the competition mechanism. Using a spatial econometric model to operationalize the size of trade diversion faced by countries excluded by PTAs, Baccini and Dür (2012) find strong evidence that competition among exporters is a key driver of the proliferation of trade agreements over the past two decades. Studies in economics find similar results relying on analogous econometric techniques (Baldwin and Jaimovich 2012).

While both domestic political and systemic explanations help improve our understanding of the proliferation of PTAs, a striking difference between these two bodies of research and the first one on the economic determinants of PTAs is the almost exclusive focus on macro-relations between dependent and independent variables. The unit of analysis of these two last sets of studies remains largely at the country- or dyad-level, making difficult to test the micro-foundation of the mechanisms at play. This is at odds with much of the IPE, and more generally IR literature, which has become increasingly micro over the past 10 years in an effort to improve the identification strategy of the empirical analysis. This is a challenge that future studies should take up.

WHAT ARE THE EFFECTS OF PREFERENTIAL TRADE AGREEMENTS?

The large majority of literature explaining the effects of trade agreements explores their impact on trade and FDI. A smaller body of research investigates the impact of PTAs on other outcomes such as reform, conflict, democratization, and human rights. I will review both streams of literature in the next two sections.

The Effect of Trade Agreements on Trade and Investment

Economic studies usually treat trade blocs as an exogenous variable and evaluate the static impact thereof on national and world welfare. Viner (1950) was the first to point out that the welfare effects of customs unions could be ambiguous. To explain this idea, he used the terminology “trade creating” and “trade diverting.” A customs union could result in both trade creation among its members, as trade barriers within the group are reduced, and trade diversion, when the increased trade between countries forming the preferential trading agreement comes at the expense of trade formerly taking place with third countries. Thus, the welfare effect of a customs union depends on the relative magnitude of these opposite results.

Just as Viner introduced the concepts of trade creation and trade diversion, Bhagwati (1991) introduces the concepts of “stumbling blocs” and “building blocs.” The former concept states that the spread of preferential trade agreements is likely to damage the multilateral trading system and is often associated with the pursuit of beggar-thy-neighbor policies. At the extreme, those who share the stumbling blocs view foresee the possibility of a world of trading blocs with relatively high barriers between them, in which trade diversion becomes the norm and an outright trade war is always a possibility. The latter concept states that, due to the dominating effects of trade creation, regionalism leads to further trade liberalization and improves multilateralism.

The theoretical debate of whether trade agreements are “stumbling blocs” or “building blocs” has not been settled yet (Aghion, Antràs, and Helpman 2007).⁶ For instance, Limão (2006) claims that major powers, which also have non-economic goals in forming PTAs, have incentives to maintain high most favored nation (MFN) tariffs to extract political

concessions from developing countries. On the contrary, Ornelas (2005) contends that trade agreements create a “rent destruction effect,” which reduces incentives to raise external tariffs with countries excluded from PTAs and, in turn, leads to a building bloc effect. The empirical evidence is also mixed, though recent studies find that trade creation effect is larger than trade diversion effect (Magee 2008) and that preferential liberalization tends to precede external tariff liberalization (Esteveadeordal, Freund, and Ornelas 2008).

Related to the debate on the welfare effect of trade agreements, several studies explore the effect of PTAs on trade and investment flows. Given that PTAs are, in their most basic form, a policy tool to reduce tariffs, it is natural to assess whether trade and investment among member countries actually increase after preferential trade liberalization. With few exceptions (Rose 2004), there is overwhelming evidence showing that PTAs have substantively increased trade flows in the post-Second World War period (Goldstein, Rivers, and Tomz 2007; Baier and Bergstrand 2007).

In addition to increased trade flows, Mansfield and Reinhardt (2008) find that PTAs reduce trade volatility among member countries. Furthermore, recent studies show that PTAs affect both the intensive margins of trade—member countries export a larger quantity of a given good after preferential liberalization—and the extensive margins of trade—member countries export a wider set of goods after preferential liberalization (Baier, Bergstrand, and Feng 2014). Moreover, in a widely-cited paper, Bütte and Milner (2008) find that PTAs promote FDI by enabling governments to make credible commitments to policies desirable to multinationals.

These results help explain why governments have been rushing to form a plethora of PTAs over the past 20 years, and the reason is simple: PTAs work. However, these studies overlook the distribution consequences of PTAs. While we know that trade liberalization is welfare-enhancing at the aggregate level, we also know that it always produces winners and losers. To test the distributional effects of trade liberalization, it is necessary to move the unit of analysis from country to firms, and to MNCs in particular, which account for over 80 percent of the value of the goods and services traded across national borders.

Using fine-grained firm-level data of the activities of the near universe of US-based multinationals, Baccini, Pinto, and Weymouth (2017) find strong evidence that tariff reductions offered by the US to its preferential trading partners has led to a strong expansion of the global supply chain activities of the largest and most productive firms. Smaller and less productive firms, on the other hand, see no benefits from preferential trade liberalization. The net result of this reallocation of sales from the smallest to the largest MNCs is higher market concentration in sectors with higher tariff cuts. The policy implications of these findings are timely and important: The uneven gains from preferential liberalization help explain the growing unpopularity of this trade policy in both the EU and the US. Again, the reason for voters’ dissatisfaction is simple: PTAs work, but not for everyone.

The Effect of Trade Agreements on Other Issue Areas

While tariff reductions are an important element of PTAs, a large percentage of goods face zero MFN tariffs. Moreover, preferential cuts are usually quite small and tariff transition—the number of years before preferential tariffs go to zero—can be quite long. It is therefore no surprise that several scholars have explored other gains from preferential liberalization.

In a book and in a series of articles, Baccini and Urpelainen (2012, 2014a, 2014b, 2014c) performed the most extensive analysis on the relationship between reform and PTAs to date. In particular, they show that EU and US PTAs have promoted ambitious reforms in intellectual property rights (IPRs), privatization, and service liberalization. Using various statistical methods such as difference-in-differences, they investigate PTA-negotiating countries and compare their reform patterns with a matched sample of non-negotiating countries that are comparable along various political and economic dimensions. Moreover, they complement the quantitative analyses with case studies on Chile, Colombia, Croatia, and South Africa, which confirm the link between the formation of trade agreements with the EU and the US and important records of economic reform.

Economic reforms are not the only non-traditional gains from preferential trade liberalization. For instance, Gray (2009) finds that Eastern European countries that cooperated with the EU and obtained its seal of approval on reform implemented in the post-communist period faced substantially lower spreads on foreign debt. Moreover, some studies argue that trade agreements may affect cooperation in other issue areas through issue linkage and membership conditionality. For instance, Pevehouse (2002) finds that regional trade agreements foster the process of democratic consolidation, whereas Limão (2005) shows that PTAs contribute to cooperation on environmental issues. Furthermore, Hafner-Burton (2013) shows that PTAs help improve human rights in developing countries, though Spilker and Böhmelt (2013) find weak evidence that trade agreements affect human rights compliance when selection bias is accounted for.

Finally, Mansfield and Pevehouse (2000) explore a core topic in IR: the relationship between conflict and trade. In particular, they find that trade agreements may reduce the probability of interstate conflict between member countries, especially among pairs of countries that trade heavily with one another. Surprisingly, there is a lack of literature linking PTAs to intra-state conflicts, which are more frequent than interstate conflicts. Given the availability of newly-collected micro-level data on violence and on economic variables, this would be a fascinating and important area for future research.

THE DESIGN OF PREFERENTIAL TRADE AGREEMENTS

The large majority of the literature that has been surveyed so far treats PTAs as a dichotomous variable capturing merely the presence or absence of trade cooperation. However, several recent studies point out that there is a great deal of variation in the design of PTAs. For instance, since the formation of NAFTA many trade agreements have featured the inclusion of provisions to protect investment and IPRs, to regulate government procurement and competition policies, to liberalize services, and to improve labor and environmental rights. This process of deep integration through PTAs has happened at the same time as the Doha Round deadlock in the WTO. Thus, preferential liberalization has emerged as the main instrument of trade governance to lower not only traditional tariff barriers but also “behind-the-border barriers” over the past two decades.

In this section I will first survey the main datasets that measure the design of PTAs. Then I will review the literature that (1) aims to explain the variation in the design of PTAs, and (2) explores how the design of trade agreements affects trade flows.

Measuring the Design of Trade Agreements

The most extensive dataset on the design of PTAs is Desta (Dür, Baccini, and Elsig 2014).⁷ The dataset codes the design of more than 620 PTAs signed after the Second World War, that is, the entire universe of PTAs except partial trade agreements. The initial coding involves more than 100 variables capturing fine-grained features of the design of PTAs. The two main dimensions measured by Desta are *depth* (i.e., the strength of commitments) and *flexibility* (i.e., the degree to which countries can escape their commitments). For both variables, there is an ordinal index, which captures the presence of treaty chapters regulating trade-related issues, and a continuous index, which captures the overall latent depth and flexibility of PTAs using principal component analysis.

There are other datasets implementing a similar data collection on a more limited sample of PTAs. For instance, Estevadeordal, Suominen, and Teh (2009) collect data on the design of 74 PTAs. Horn, Mavroidis, and Sapir (2010) code WTO-Plus and WTO-X provisions included in all the PTAs signed by the EU and the US.⁸ The WTO's annual World Trade Report (2011) extends Horn, Mavroidis, and Sapir's initial coding to 97 more PTAs. Kim (2012) has developed an index of deep integration commitments for all the PTAs formed by Asian countries. Orefice and Rocha (2014) collect data on the content of 100 PTAs signed between 1958 and 2011.⁹

Finally, there are other datasets coding specific dimensions or issue areas in the design of trade agreements. For instance, Allee and Elsig (2016) code provisions related to the enforcement of PTAs, whereas Lechner (2016) codes non-trade issues, such as human rights, labor standards, and environmental standards. In addition, Roy, Marchetti, and Lim (2009) focus on services commitments in PTAs, whereas Piermartini and Budetta (2009) collect data on technical barriers to trade (TBT) included in PTAs.

Explaining the Design of Trade Agreements and Its Effect

Much of the literature on the design of trade agreements has focused on arguments explaining why countries commit to deep integration. One body of research focuses on the importance of global value chains and on the preferences of multinationals. For instance, several studies have found robust correlations between a high level of trade in parts and components and deep trade agreements with permissive rule of origins (Chase 2008; Manger 2009; Kim 2012). In particular, Kim (2012) finds that strong commitments in TBT provisions are strongly associated with deep integration, whereas the correlation between strong commitments on competition policy and deep PTAs is weak. A key problem of these studies is to understand the arrow of causality, since deep PTAs are also likely to affect global production networks (Orefice and Rocha 2014; Baccini, Pinto, and Weymouth 2017).

Another body of the literature has explored the diffusion of the design of PTAs. Manger (2009) provides the most compelling argument on the diffusion of trade-related issues. MNCs push for the inclusion of provisions protecting investment and IPRs to gain an edge over competing multinationals from other countries. In turn, those MNCs from countries without deep PTAs lobby their governments to form PTAs with the same coverage to level the playing field with competitors. In short, Manger's (2009) theory explains both the proliferation of PTAs and the proliferation of deep integration. Several case studies support the main thesis of the book. Subsequent papers find quantitative evidence that provisions diffuse from one PTA to another (Baccini, Dür, and Haftel 2014; Baccini and Dür 2015; Kim and Manger 2017).

While the majority of the studies focus on the depth of PTAs, a few papers explore the variation of flexibility provisions included in PTAs. Kucik (2012) shows that interest groups' preferences are a key determinant of the degree of flexibility included in PTAs. In particular, import-competing industries push for the inclusion of escape clauses, which allow them to protect their share of the markets. On the contrary, exporters bear the costs of flexibility, which reduces the predictability of trade relations.

Furthermore, recent studies have investigated the interdependence of different dimensions of PTAs, and the nexus between depth and flexibility in particular. Johns (2014) develops a game theoretic model to show that deep agreements, which require large tariff cuts, are more likely to include flexibility provisions than PTAs including shallow commitments. Using a sample of 587 PTAs, Baccini, Dür, and Elsig (2015) find that flexibility and depth are positively correlated, but that this correlation is weaker for democracies than autocracies. Surprisingly, the diffusion literature and studies positing the interdependence of different dimensions of PTAs have progressed on separate avenues to date. Future papers should implement full-fledged models that incorporate both mechanisms, that is, diffusion and interdependence, to explain how the network of design of PTAs has developed over time.

Finally, several studies have tested the effect of the design of PTAs on trade flows and FDI. Results indicate that deep PTAs increase trade flows between member countries significantly more than shallow PTAs (Hicks and Kim 2012; Dür, Baccini, and Elsig 2014). Similarly, Büthe and Milner (2014) find that PTAs that include investment provisions and a DSM are also more likely to increase FDI than PTAs without investment provisions and a DSM. Interestingly, Bearce, Eldredge, and Jolliff (2016) show that PTAs including greater restrictions on the use of escape clauses tend to be more trade effective, a result confirming the trade-off between politically viable PTAs and economically efficient PTAs.

CONCLUSION

PTAs are a trademark of the contemporary international economic system. Thus, it is no surprise that preferential trade liberalization has been affected by the growing backlash against globalization. Indeed, some new PTAs are at risk and some old ones seem in deep trouble. In particular, the future of the two aforementioned mega-regions looks rather grim. On the one hand, the TPP has been the first victim of Trump's presidency, with the US withdrawing from the deal, and it seems unlikely that the other TPP members will implement the treaty

without the US. On the other hand, the fate of TTIP is also quite uncertain, facing strong opposition from both the US administration and citizens in many important EU countries.

The EU, which is the most integrated trade agreement, is also experiencing unprecedented economic and political turmoil. The UK triggered article 50 to exit from the EU at the end of March 2017, making Brexit an inevitable reality. Moreover, there are ongoing tensions between continental European countries and Southern countries over austerity measures and monetary policy. Disturbingly, the EU approval rate is at its lowest level in virtually every European country and Eurosceptic parties are gaining popular support everywhere. While the collapse of the EU seems unlikely, major adjustments may be necessary to grant its stability in the long term.

Scholars working on PTAs should take up this challenge and try to explain the causes of voters' dissatisfaction over preferential liberalization. While the previous literature has prioritized analyses of the net welfare effect of PTAs, the dramatic redistribution of resources among firms and individuals triggered by trade liberalization has been generally overlooked by both IPE and economic studies.¹⁰ Building on these gaps, future research should explore which policies countries should implement to mitigate the uneven gains from trade, for example compensation policies alleviating the strain placed on displaced workers. In exploring the link between trade policy and welfare policy future studies can rely on novel rich micro-level data of firms, consumers, and workers, which would allow them to go to the heart of the IPE debate on winners and losers from globalization.

To conclude, I would like to address the issue of the "Atlantic divide" in IPE, which is one of the themes of this handbook. From my reading, this divide does not appear to exist in the PTA literature. While the share of pieces relying on qualitative methods or on constructivist approaches is probably higher in Europe than in the US, the research questions addressed by scholars on both sides of the Atlantic tend to be quite similar. Moreover, there is a great deal of academic consensus on some important findings related to PTAs at this stage, since this field is quite mature. In cases in which disagreements persist and debates remain unsettled, I see little evidence of an "Atlantic divide," which seems to be a thing of the past if indeed it ever existed.

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NOTES

1. Broadly speaking, preferential trade agreements are arrangements that liberalize trade among member countries in a discriminatory way. PTA is a general term covering different types of agreements: (1) Partial Preferential Agreements grant member states preferential access to a selected segment of their markets; (2) Free Trade Areas eliminate internal trade barriers; (3) Customs Unions eliminate internal trade barriers and set a common external tariff; (4) Common Markets allow the free movement of factors of production and finished products across national borders; and (5) Economic and Monetary Unions are common markets with a common currency.

2. Data are from Desta, available at <https://www.designoftradeagreements.org/downloads> (accessed on March 23, 2017).
3. Accessed through the McGill University Library on March 23, 2017.
4. The importance of JIE and ROIO may be explained by the fact that they have a strong empirical focus, which tends to dominate this line of research. Being the leading IR journal, the importance of IO is not surprising and it is common to other research areas in IR.
5. Vertical IIT can be defined as trade in similar goods produced by the same industry, but differentiated by the unit value of goods.
6. A review of this vast literature is beyond the scope of this chapter. For an extensive and thorough survey, see Freund and Ornelas (2010).
7. The dataset is available at <https://www.designoftradeagreements.org/>.
8. WTO-Plus and WTO-X provisions either broaden or deepen commitments made in the WTO.
9. The dataset is available at https://www.wto.org/english/res_e/publications_e/wtr11_dataset_e.htm.
10. For an important exception, see Autor, Dorn, and Hanson 2016.

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CHAPTER 25

CORPORATE NETWORKS

MILAN BABIC, JAN FICHTNER, AND
EELKE HEEMSKERK

INTRODUCTION

IN 2012, the *New York Times* proclaimed the age of “big data”—the harnessing and analysis of large swaths of previously unavailable information about the social world (Lohr 2012). This proclamation was not only limited to the usual suspects running their business model on large amounts of user data. A “new breed of political scientists” (Lohr 2012) was expected to thrive in this new age of data abundance. The major field of application of this abundance was quickly identified as network science (Przulj and Malod-Dognin 2016). This was an exciting promise for the relatively young discipline of International Political Economy (IPE) for the 2010s. Previous theoretical and qualitative insights postulated that corporate network fora like elite communities were driving political economy phenomena from global trade governance to European integration (see, e.g., van Apeldoorn 2002). With the availability of large-scale, fine-grained corporate network data and the application of cutting-edge methods, these insights could be empirically bolstered and leveraged on a global, cross-temporal scale. With its strong rooting in grand theories of international (economic) relations, IPE as a discipline presented a fruitful “testing ground” for the application of data-driven network analysis. Reflecting this hope in 2009, Robert Keohane named network analysis a “much more sophisticated” way of analyzing complex interdependence in IPE, and expressed hope that network analysis (among others) would retain the “I” in IPE for the future of the discipline (Keohane 2009, 39).

In this chapter, we take stock of the role of corporate network analysis after a decade of the proclamation of the age of big data (see also the chapter by Young and Winecoff). Specifically, we ask to what extent the introduction of data abundance and new methods was employed to answer older, paradigmatic theoretical questions related to corporate networks as fora for political change. To do so, we survey the corporate network literature in six major IPE journals over the last decade. Our contention is that while corporate networks have increasingly captured a major position as fora of political contestation in IPE discussions, the field still does not comprehensively exploit the abundance of “big data” and the respective methods to answer essential questions related to the issue. Despite some landmark studies outside of

core IPE journals (see, e.g., Vitali et al. 2011), big data and respective methods are often still not widely used in corporate network studies, or they are being employed to answer rather specific questions in subfields of the discipline. However, we also find a number of studies that lately deliberately fuse big questions with big data and network analytics (see, e.g., Ajdacic et al. 2021; Babic et al. 2020; Heemskerk and Takes 2016; Winecoff 2020). We argue that such fusing of both aspects can help IPE to re-appreciate corporate networks as central fora for transformation and change in the global political economy. Finally, we describe recent work outside of the mainstream core of IPE which engages in a fruitful fusion of big questions and big data and which presents promising avenues where large-scale corporate network analysis can make a real empirical contribution pushing the field forward into the 2020s.

In the remainder of this chapter, we first survey the mainstream IPE literature on corporate networks of the last decade and analyze in how far the emergence and availability of big data has had an identifiable impact on the field. We then review work outside of this mainstream core that illustrates ways of bringing big data approaches to corporate network studies and create important impulses for a further development of the field. Here we focus on how rapidly reconfiguring corporate ownership networks act as underappreciated fora for politico-economic change. We conclude by embedding our findings in the broader context of a changing IPE in the twenty-first century.

CORPORATE NETWORKS RESEARCH AFTER THE FIRST DECADE INTO THE AGE OF BIG DATA

In order to make qualified statements about the state of the field of IPE and network science, we surveyed the contributions of the following academic journals since 2010: *Competition & Change*, *European Journal of International Relations*, *International Organization*, *International Studies Quarterly*, *New Political Economy*, and *Review of International Political Economy*. While this is not an exhaustive list of what can be considered IPE (and especially Global Political Economy (GPE)¹), we deem this selection to represent the core of the mainstream discussion within the broad field of IPE. We searched for papers with “network” in their title and/or abstract. From an initial sample of 74 papers we had to cut those that did not relate specifically to corporate networks. We checked whether the abstracts of these papers are dealing with corporate networks in the broadest sense. Our final sample then includes 53 papers that match our search strategy.

In our search strategy, we define “big data” quite broadly, namely that a publication uses large-scale, often fine-grained corporate (network) data for analysis. What is “large-scale” depends to some degree on the subject of study, but the used data should be global in the sense that it strives to cover the whole extent of a phenomenon, not only a fraction or sample of it. In most cases of corporate network data, this means that we speak of thousands to millions of financial, ownership, investment, etc. relations. Consequently, the methods used to analyze big (corporate) network data in this type of research often differ (Heemskerk 2018). Instead of “variable-based” research designs that seek to establish (causal) inference, big data approaches often resort to so-called “pattern-based” designs that embrace causal complexity instead (Oatley 2017). Naturally such a focus lends itself to network analytics that

embrace complexity and large-scale patterns of corporate and human behavior as their unit of analysis (Glattfelder 2010; Wasserman and Faust 1994). Our understanding of “big data” hence entails a strong connection between this data and the usage of network science or at least network metrics to analyze a given amount of information.

In our analysis, we specifically look for work that tackles “big questions” of IPE and corporate network analysis in particular. The reason for this is that we regard the yardstick of the success of the big data revolution in whether such approaches can actually help to push the field meaningfully forward. We hence specifically look for contributions blending big data approaches with asking historically relevant, cross-cutting questions about corporate and state power, global hegemony, economic development, the rise and fall of global actors, and the demise and transformation of global economic structures. Put simply, we want to stress the *asking of big questions* such as for instance: who controls today’s major global corporations? Who holds power in the international system? What are the drivers of global capitalist transformation? How can we understand the evolution of global corporate control? Are states or corporations more powerful in the twenty-first century? Is a global corporate elite emerging or are elites still fragmented along national lines? And many others. What distinguishes such fundamental theoretical questions from “everyday” social science questions is that tackling the former requires researchers to innovate either theoretically, methodologically, or empirically to provide a meaningful answer. They are, to paraphrase Robert Cox (1981), not merely problems to be solved but issues that require innovative out-of-the-box thinking. Such thinking is often found in cross-disciplinary research that embraces causal complexity rather than strictly unidirectional problem-solving research designs.

Within the sample of publications we gathered from the last decade, we find that out of 54 papers, seven papers fit our definition of big data publications, while 22 tackle fundamental theory questions in the broadest sense. A majority of network-related papers are thus not concerned with big theoretical questions, while only around 15 percent use a big data framework. In total, five papers, or around 9 percent of all publications, actually combine fundamental theoretical and big (corporate network) data questions. While this is a rather modest amount, we have to take into account that we survey here the core of the mainstream literature on the topic; and that we employ a network-centered focus. The mere existence of such publications in top IPE journals is hence already a basis to work with.

Among the 22 papers tackling fundamental theoretical questions, we can find contributions from various theoretical strands, from critical IPE to more mainstream accounts. In one way or another, most of these studies analyze how corporate and/or state networks are “fora” for politico-economic change whose importance is not yet appreciated. The topics covered range from questions about corporate elite networks (De Graaff and Van Apeldoorn 2014), global value chains (Neilson et al. 2014), and state transformations (Ougaard 2018), to issues of development and global inequality (Mahutga 2014; Selwyn 2013). Many of those studies actually employ a network analytical approach, for example van der Pijl et al. (2011) on transnational capital fractions, De Graaff and Van Apeldoorn (2014) on foreign policy-making networks, Gray and Potter (2012) on trade networks, or de Graaff (2020) on the integration of Chinese corporate elites into global networks. The focus of the lion’s share of these studies certainly fits within a “pattern-based” research approach. Most studies interested in big questions relating to corporate power or elite transnationalization employ a descriptive, but theoretically very firm approach to tackling these questions. They also aim to cover as much ground as possible in their studies and choose data collection strategies aiming at presenting a *global*

picture of their respective topic. As an example, van der Pijl et al. (2011) collect various board membership information on the top 100 transnational corporations in 1992 or the top 150 corporations in 2000 and 2005 to present a global picture of the phenomenon at hand. Others like de Graaff (2020) collect detailed biographical information on Chinese elite network members to create their globalized ego networks. Mahutga (2014) analyzes global garment and transportation equipment trade data through network analytical techniques.

Such studies have in common that they aim to cover global phenomena in accordance with their “big questions asked” approach, and therefore have to find a methodological way of dealing with data limitations: van der Pijl et al. (2011) for instance combine different data sources and acknowledge the limited nature of some these data (395). De Graaff (2020) furthermore explains that her approach covers a more limited sample of firms and organizations, but therefore allows her to actually conduct a complete biographical mapping of her elite data (212). Mahutga (2014) argues that a “lack of cross-nationally comparable firm-level data” (166) means he has to use nationally aggregated trade data instead. These examples show how scholars aim to reconcile big questions of the discipline of IPE with methodological aspects that would optimally require large-scale, often fine-grained, firm-level data. Heemskerk (2018) describes the relation between theoretical ambition and empirical implementation for some of these studies (e.g., van der Pijl et al. 2011) as “unfulfilled promise.” He asks whether and why “theory trumps empirical analysis?” (van der Pijl et al. 2011, 235) and calls for a more rigorous application of empirically oriented work that complements the big theoretical questions raised by such IPE corporate (network) analyses.

Such a rigorous employment of big data and the respective methods is being conducted in another, smaller subset of our sample. Studies like Pažitka et al. (2021) use fine-grained firm-level data on IPOs (initial public offerings; i.e., firms that list on the stock market for the first time) over a longer time frame sourced from the Dealogic ECM database. They employ this data to create the explanatory variables for their study, which also includes prominently network measures (Pažitka et al. 2021, 9). The usage of such data allows them to analyze which investment banks are extracting how much profit by underwriting IPO deals over a certain time period. Another such example is the collection and analysis of a large amount of textual data by James et al. (2021). This study uses responses to EU financial regulatory consultations, which are employed to map patterns of (networked) financial industry coordination within Europe. Drawing on over 8,000 written responses, the authors create a unique and large dataset which allows them to carve out the emerging cross-border networks of financial industry coordination (James et al. 2021, 909).

Those studies not only have in common that they aim to deliver new, potentially “big” datasets that are able to tackle the issues described in these papers. Moreover, they employ these data and respective innovative methods to fill specific gaps in the respective literature, or extend and correct our knowledge of specific themes. Pažitka et al. (2021) aim to describe rent extraction practices of a specific group of financial actors, while James et al. (2021) seek to advance the study of the variety of financial lobbying networks through their methodological contribution (James et al. 2021, 899). Such problem statements are crucial to advance IPE as a problem-driven discipline (see the Introduction to this handbook). The methodological and data-related contributions of these papers are an important extension of the first identified group of papers that put a stronger focus on “big” theoretical questions. Taken together, both sets of contributions advance our understanding of corporate networks as fora for politico-economic change.

A third and smaller subgroup of papers, which attempts a combination of big theoretical questions and big data and which lays the groundwork for further research, is discussed in the next section. Taking a closer look at those publications that combine both aspects shows some of the already realized potential and the thematic breadth of this fusion. In his study, Dasandi (2014) asks whether and how international inequality affects poverty in the global system. The paper tackles a long-standing problem of the political economy of global development: what factors cause some states to remain “poor” while others remain relatively rich? (Gunder Frank 1966; Rostow 1959). Different from many mainstream approaches, Dasandi probes a structural and global explanation: he argues that it is the unequal relations emerging within the global trading system that produce and sustain poverty and relative inequality. The gist of this argument is clearly a “grand theory” question in the best tradition of World Systems and Dependency theory (Cardoso 1982; Wallerstein 2005). However, the methodological approach Dasandi uses is not at all traditional but is, in fact, rather innovative. He employs network analytics to a large dataset on global trade relations between 1980 and 2007 in order to determine the relative position of different countries within the global trade network. The study utilizes different network concepts like regular equivalence to measure these positions (Dasandi 2014, 208). Dasandi argues that such a measure better reflects the complex nature of international trade today, as it incorporates *indirect* relations, and hence interdependence, and not only direct ties to other states. In a second step, the study employs another network analytical technique, namely hierarchical clustering, to determine country clusters based on their similarity (Dasandi 2014, 209). The results show how countries can be located in four different clusters depending on their relative power position in the global trade network.

A second study in this group takes on another important question of political economy and corporate networks research, namely the evolution of the global trade system in the twentieth century. Abdollahian and Yang (2014) argue that classical economic theories explaining ever rising trade volumes such as the Heckscher-Ohlin model need to be complemented by empirical work fusing trade, growth, and globalization in the twentieth century. In trying to think these three themes together, they seek to “derive patterns describing the structure and evolution of global trade in ways that traditional econometric measures cannot” (Abdollahian and Yang 2014, 605). This means first and foremost the employment of network analytic methods to a large dataset on global trade of 180 countries between 1960 and 2009. They employ different network measures such as degree, betweenness, and eigenvector centrality to carve out these patterns on a global scale. Afterwards, they take a sample of seven large trading countries and explore the found patterns further for a longer time period, specifically, from 1920 to 2010. The paper finds that over the course of the twentieth century trade convergence has taken place and trade inequities are declining, at least among major powers. Abdollahian and Yang take this as an indicator that globalization might not after all increase trade inequities over the long run, but they also caution that their paper is only a first step into exploring these patterns over a longer time frame. The focus on a pattern-based, rather than variable-based, research design enables them to cover more ground, but it also implies that these findings need to be scrutinized against studies that indicate the persistence of inequity in the world system, like the paper discussed by Dasandi (2014). The potential controversy arising out of such different findings of different approaches fusing big data and important theoretical questions is a fruitful avenue for further research and scrutiny from this perspective.

Staying with the debate on the merits and consequences of globalization, Heemskerk and Takes (2016) take up another key debate in corporate network research: how far has transnationalization created a global corporate elite that forms a distinct social community? The authors aim to contribute to this question by exploring the structural qualities and network characteristics of this global corporate elite. They are not the first scholars to do so, as corporate elite network research has a long history in the field (see Carroll and Fennema 2002; Mizruchi 1996; Useem 1986). However, Heemskerk and Takes bring a big data corporate network approach to the table that does not work with (limited) samples, but utilizes the wealth of the whole Orbis database to extract millions of board interlock positions on a global scale. In order to understand how corporate control is distributed in the twenty-first century—a long-debated theoretical question—the paper draws on state-of-the-art network techniques such as community detection. Heemskerk and Takes source in total the largest 968,409 firms from 208 countries, which entail over 3 million individuals in senior management and on corporate boards (Heemskerk and Takes 2016, 98, 99). Within this sample, they find around 1.7 million board interlocks, which form the basis for their large-scale study. By iterating over this sample with a community detection algorithm, the authors refine the emerging structure of the global corporate elite and find in total eight distinct communities (for an overview see Heemskerk and Takes 2016, 107). The resulting network of global corporate elites is not a “flat” one, but has its power locus in the North-Atlantic and Commonwealth cluster, which also shows dense relations to other European communities. As a general result, the fusion of grand theory and big data enable Heemskerk and Takes to show that the global political economy is becoming more multipolar, while at the same time retaining some stability in the “core” centered around North America and Europe.

A recent study by Winecoff (2020), which also belongs to our sample, revisits a long-standing theory question not only of corporate network research, but of the field of IPE itself. Winecoff asks how Susan Strange’s notion of structural power in the world system could be analyzed empirically by the means of modern network science tools and access to large-scale and fine-grained data sources. Strange (1987) argued that the (then-and-now) prevailing notion that US power is declining in the international system can hardly be sustained “when it is subjected to close and searching scrutiny” (552). She argued that the structural position of the US in the realms of finance, trade, security, and knowledge remains pervasive—a thought which stands against the “frequently relational, linear, and monotonic” (Winecoff 2020, 212) standard model of an (alleged) US decline (see also Oatley et al. 2013; Starrs 2013; Fichtner 2017). Winecoff then aims to put Strange’s intuition on a solid empirical basis. With the help of insights from network science, he contends that the global political economy is a complex system in the truest sense of the word: it consists of different units (nodes) that interact in order to produce a system which is more than the sum of its underlying parts (Winecoff 2020, 214). Based on this insight, Winecoff argues that

[i]f we develop a strong understanding of the ways in which global networks emerge, evolve, and are shaped and reshaped over time then we can not only better understand the importance of interdependence, but also uncover the organization of structural power.

(Winecoff 2020, 216)

The key tool the paper uses to do so is the fitness plus preferential attachment (FPA) model derived from network science, which makes it possible to include “node-level” (here: state-level) attributes as well as more systemic characteristics (Winecoff 2020, 220). With

this model, Winecoff analyzes the four structures of world politics suggested by Strange and concludes that US power is far from being replaced nor is it likely to diminish over time. On the contrary, in many global structures the US could even be said to have expanded its structural power position since 1990 and 2008, respectively. In this sense, Winecoff's paper fuses concrete network science tools and data analysis to push the frontiers of our knowledge about enduring issues in corporate network and IPE research.

Our survey of several IPE journals revealed some important patterns across the mainstream core of the field. We can see that "grand theory" questions are far from being marginalized within the mainstream journals. Furthermore, we observed that approaches drawing on large-scale (network) analysis represent a minority in our sample, but that they at least took place in the last decade. Within this limited number of publications, we also found some papers that fused both aspects and thus attempted to break new ground within and beyond the research on corporate networks. The fact that such papers still play a minor role in the grand scheme of things is in our estimation due to missing "big data" approaches rather than to the lack of big questions being asked. And this is actually good news: we do not believe that a lack of ambition or the diminishing importance of grand theory is actually something that is likely to happen to the field anytime soon. If this ambition is present as ever, all it takes is to bring in new methods and perspectives on how to tackle those questions. Contributions like Winecoff (2020) exemplify that this is possible and desirable; and that mainstream IPE has the means to bring large-scale data analysis to important questions of corporate networks as fora for politico-economic change.

As a final step, we want to draw the attention to innovative IPE work that speaks to this issue and that is taking place *outside* of the mainstream core of the field. It is not controversial to say that the field of IPE as an intellectual project spans much more than a handful of peer-reviewed, high-impact journals. For newly emerging approaches like those surveyed in this chapter, it is not unusual for them to "come in from the side" as they often break with long-established disciplinary boundaries and rules of how to do "proper" research. As already discussed in this chapter, the pattern-based and hence often descriptive nature of much of big corporate network data-based research is certainly disruptive in the sense that it is clearly of a quantitative nature, but does not adhere to the usual "rules" of variables-based quantitative research. It is hence perhaps not so surprising that some of the most path-breaking work is located outside of the core IPE journals surveyed here, and is only slowly finding its way into these outlets. We discuss some of these publications in the following section with a focus on how corporate networks constitute a "forum" for broader political and economic change that is yet unappreciated in the IPE mainstream.

CLOSING THE GAP: IPE CONTRIBUTIONS ON CORPORATE NETWORKS AS FORA FOR POLITICO-ECONOMIC CHANGE

Among the literature outside the mainstream that fuses big questions with big data in studying corporate networks, Vitali et al. (2011) represents a landmark study. The authors analyzed for the first time the entire global network of corporate ownership. Before this

analysis, only small national samples were studied and—according to those authors—there was no appropriate methodology to analyze corporate ownership and control from a truly global perspective. Using the Orbis database they generated a dataset covering the entire network of transnational corporations in 2007, which included over 600,000 nodes and 1 million ownership ties. Vitali and colleagues then applied a novel algorithm to compute network control. They found that in 2007 the top three global “control-holders” were the British bank and asset manager Barclays, followed by the two US asset managers Capital Group and Fidelity. The authors summarize their novel contribution as follows:

We present the first investigation of the architecture of the international ownership network, along with the computation of the control held by each global player. We find that transnational corporations form a giant bow-tie structure and that a large portion of control flows to a small tightly-knit core of financial institutions. This core can be seen as an economic “super-entity” that raises new important issues both for researchers and policy makers.

(Vitali et al. 2011)

On the one hand, this study was truly path-breaking for corporate network research in IPE, because it provided the first empirical analysis that covered virtually all transnational corporations and developed novel methods to compute which firms hold the greatest degree of control in this global corporate ownership network. On the other hand, the paper did not pursue theory-building. The topic lends itself to tackling fundamental questions of IPE research, such as corporate and state power and globalization. However, Vitali et al. (2011) neither investigated why this described global corporate ownership network had developed the way it did nor what some of the major politico-economic consequences of this “small tightly-knit core of financial institutions” might be. This is certainly partly because the authors would not necessarily consider themselves to be part of the broader field of IPE. Nevertheless, the study paved the way for fusing fundamental questions of corporate network research with large-scale data analysis and inspired much subsequent work in the field (see, e.g., Babic 2021; Garcia-Bernardo et al. 2017).

However, other work outside the IPE mainstream core did engage more closely with what we understand as the significant development of grand theory that is interlinked with large-scale corporate network data analysis. Using data from the Orbis database by Bureau van Dijk, Fichtner et al. (2017) have analyzed the complete ownership network of all 3,882 publicly listed companies in the US. Drawing on insights from Vitali et al. (2011), as well as crucial work by Davis (2008) and Braun (2016), Fichtner and colleagues identified that in 2016 the network of US listed corporations was dominated by just three American asset managers: BlackRock (which bought the asset management division of Barclays in 2009), Vanguard, and State Street, which they consequently dubbed the “Big Three.” These three giant US asset managers, seen together, constituted the largest shareholder in almost 90 percent of the 500 largest American corporations. These findings can be seen as transformational for the field of IPE, since the traditional view held by many economists and corporate governance scholars was that ownership of large listed US corporations would be dispersed and fragmented (see, e.g., Windolf 2002). Importantly, Fichtner et al. (2017) also provided a first explanation for why the “Big Three” were growing much more rapidly than almost all other asset managers: they dominate the burgeoning segment of “passive” funds that simply track existing stock (or bond) indices. What is crucial is that in passive index funds there are large economies of scale and significant first-mover advantages—once there are a few large

and liquid passive funds that track any given important index (e.g., S&P 500 in the US, FTSE 100 in the UK, and EuroStoxx 50 in the Eurozone) there is no reasonable chance for other funds to attract major asset inflows. This new situation is radically different from the previous era of actively managed funds in which no giant asset management firms could emerge due to an absence of large economies of scale.

The combination of big questions—such as, who holds power in US capitalism?—and big data analytics advanced the study of corporate networks in IPE in important ways. This new passive mode of investing via index funds has direct consequences for the formation and reconfiguration of ownership networks of publicly listed corporations. In the past, active fund managers (e.g., Capital Group or Fidelity) bought shares of (domestic or international) firms they deemed would generate high returns and they sold shares of firms they believed would underperform. This resulted in dispersed and transient, that is, constantly changing corporate ownership, networks. In this new age of passive asset management, investors in index funds are effectively outsourcing or delegating their investment decisions to the three major index providers MSCI, S&P Dow Jones, and FTSE Russell, as they determine which firms and countries are included in key indexes (Petry et al. 2021). Moreover, the index funds industry is dominated by the “Big Three.” As a result, corporate ownership networks in many countries are becoming much less transient, while at the same time BlackRock, Vanguard, and to some degree State Street are growing into the largest holders within these ownership networks. In the words of Fichtner and Heemskerk (2020), these asset managers are becoming the “new permanent universal owners” of this incipient era of index investing.

Corporate ownership determines corporate control; and the behavior of publicly listed corporations plays a pivotal role for politico-economic change in most countries. Simply due to its sheer size as the largest node in many corporate ownership networks, BlackRock has become somewhat of a “focal institution” that is increasingly *de facto* setting standards for listed firms concerning topics such as diversity and climate change mitigation. In the US this new index investing paradigm has led to the “Big Three,” taken together, on average now holding more than 20 percent of each of the 500 largest listed corporations. A recent incident has shown that this central position in the ownership network can be used to force change in large corporations. The “Big Three” supported the small hedge fund Engine No.1, which elected sustainability-oriented board members against the will of ExxonMobil, the largest US oil firm. It remains to be seen whether this incident was just a one-time (public relations) event or whether the “Big Three” will increasingly use their pivotal network position to act as agents of (lasting) politico-economic change.

A last paper we want to highlight in this respect is recent work on corporate elites by Valeeva (2021). In her contribution, Valeeva observes that much of the work on corporate elites in the past has established that transnational corporate networks play a major forum where power and control is exercised. At the same time, we know relatively little about the geography of these elites: where they are located, how these locations are connected, and where power and control is concentrated within this network. Valeeva calls this the “backbone” of the transnational corporate elite. She applies a set of community detection and network centrality analysis methods in order to filter, reduce, and analyze the vast network of transnational elite connections. Her (cleaned) dataset consists of the board members of over 10,500 of the largest global firms with over 2,600 city locations (Valeeva 2021, 15). Building on this unique dataset, she creates a city-by-city network using geographical information from the Orbis database and the Google Maps API. She then assigns this geographical

information to the corporate board interlock network into a final dataset consisting of over 1,600 cities which are connected via over 7,500 corporate ties. Valeeva applies a set of different methods to this network to weight the different nodes and to filter for significant ties. The results show that the cities in the sample share on average 61 individuals sitting on corporate boards, which illustrates the connectivity of the global corporate elite. It also becomes clear that half of the detected communities of transnational corporate elite members are located across different countries and regions, whereas the other half is “only” connected in the same region. This is a new and relevant finding and extends our knowledge of how global corporate elites re-organize in the twenty-first century. Among the first group of cross-regional communities there are interesting new patterns to be observed such as newly emerging Asian-European communities that are growing alongside well-established and well-researched communities like those in the Transatlantic and Anglophone world (Valeeva 2021, 20).

Valeeva’s research not only pushes forward the integration of new methods and large datasets into the study of corporate networks, but it also speaks to a long and established tradition of trying to understand the composition and structure of (transnational) corporate elites (Carroll et al. 2010; Fennema 1982; Mizruchi 1996). She contributes to long-standing debates within the research on corporate networks by employing new large-scale data and network methods to find out how the global corporate community is structured. The question of how global corporate elites are connected necessitates such approaches that build on data covering the whole globe. The study by Valeeva is hence a prime example of how to fuse big questions of corporate (elite) networks research with big data and network methods to generate new knowledge that pushes the field forward. Importantly, it also illustrates how corporate networks can be *fora* for global dynamics and change: the emerging Asian-European corporate communities Valeeva finds imply a slow shift from the “old” twentieth-century locus of power in the Transatlantic and Anglophone world toward Asia and the Pacific Rim as a new power center of global capitalism.

The research discussed in this section illustrates how a fusion of big questions of corporate network research with big data approaches can yield results that advance our knowledge of corporate networks as *fora* for politico-economic change. All the papers provided important insights into how global capitalism functions and changes on the basis of large-scale datasets that were analyzed not only for themselves, but with an eye to answering major questions of the field. We mentioned before that these studies are in some ways “outside” what is considered the IPE mainstream because they were not published in the main journals we identified above. The papers we surveyed also take inspiration from other disciplines such as sociology, finance, or computer science, and hence for some might not fit neatly into a narrow “IPE” definition. At the same time, however, the work conducted outside of mainstream journals is already part and parcel of the global discussion on the direction and future of IPE and corporate network studies in particular. From our experiences of visiting and organizing global conferences, workshops, and other *fora* for professional exchange, of submitting papers and book manuscripts for peer review, and engaging in other forms of scholarly activity, we do have the impression that the type of research described in this section has a firm standing among the variety of approaches within the field of IPE. This being said, producing and establishing cutting-edge research that gets published in top journals is also a matter of timing. We are optimistic that the promising approaches we identified here for the future of corporate network research will

increasingly find their way into top publications and the so-called “mainstream” of IPE in the coming years.

CONCLUSION

In this chapter, we aimed to sketch the current landscape of corporate networks research and we specifically paid attention to how these networks act as underappreciated fora for transformation and change in global capitalism. We argued that the age of big data that began in the early 2010s presented major opportunities to the field, as it enables researchers to draw on vast swaths of information to tackle long-standing contested or open questions of the field. These “big questions” concern mainly the globality of many phenomena we are interested in when it comes to corporate networks: Who holds power in global capitalism? How are corporate governance and corporate control changing? Is there a transnational capitalist class or elite and where is it located? Questions like these are not new, and for a long time scholars had to resort to limited data or reductionist approaches in order to tackle them. Building on this existing body of (limited) knowledge, new approaches fusing old questions with new datasets and cutting-edge methods break new ground and pave the way for a better understanding of how corporate networks can be fora for politico-economic change—today maybe even more so than in the twentieth century.

However, we also saw that such approaches and studies still constitute a minority among the core mainstream journals of IPE. This is not entirely surprising, as much of the work that only started to thrive in the second part of the last decade still needs to establish itself in sometimes long-winded paper and book development processes. For the future of the field of IPE this means that we should expect a proliferation of the innovative work we reviewed in this chapter; and that our review is a snapshot within this process. Such proliferation will surely also be fueled by the increase in BA and MA programs focusing on computational social science and related interdisciplinary programs we can observe, at least in Europe, in recent years. These and further developments will strengthen the interdisciplinary character of corporate network research and IPE as a whole, and hopefully also lead to an increase in major publications dealing with big questions using large-scale datasets and techniques down the road. The 2010s were an era of bringing together new approaches and insights and long-standing theoretical questions in the field. Our expectation and hope is that recent developments will give IPE a boost through work that embraces a reciprocal and alternating progression of empirical analysis and theory building in the 2020s. As a scholarly community, we should be very interested in keeping these conversations going and making IPE as an intellectual project as attractive, inclusive, and open as possible. If we fail to do so, cutting-edge research on corporate networks might move into other fora outside of the core of IPE—for example in general flagship journals like *Scientific Reports* (Garcia-Bernardo et al. 2017) or *Nature* (Battiston et al. 2016). In this sense, there is a possibility that work which is not taking place in the IPE mainstream might also just “move on” to greener pastures elsewhere. From our analysis above, we would count this as a loss for our field, as the merging of big data and big questions is especially interesting and fruitful for IPE as an intellectual project.

How exactly this project will look going into the 2020s also depends on the relevant topics and interests of academics working in the field in the coming years. We believe

that especially recent IPE work concerning finance/security infrastructure networks (de Goede 2021), the new interdependence approach (Farrell and Newman 2019), and a political economy of complex interdependence as such (Oatley 2019) will be fertile ground for further strengthening the nexus between corporate network research and big data approaches. All three themes embrace the complexity of the global political economy of the twenty-first century without reducing this complexity to “being complicated.” They acknowledge that the ways in which (digital) infrastructures, global (corporate and security) ties, and cross-border networks shape global capitalism are fundamentally changing. From the weaponization of cross-border networks to the exploitation of such ties by corporate, state, and other actors, understanding the complexity of the global political economy will need both fundamental theoretical and conceptual work as well as the empirical application of new methods and large, previously unavailable datasets. We are positive that despite epochal shifts in corporate networks and their organization across the globe, IPE and its neighboring fields have the theoretical and empirical means to make sense of a transforming global order. The research reviewed in this chapter shows how bringing together big questions and big data is possible and desirable in order to understand a global political economy in flux.

NOTE

1. At the time of writing, a new journal called *Global Political Economy* is being launched by Bristol University Press. This branching out into global themes is motivated by “an explicit intention of cross-disciplinarity,” as the editors state on the journal website, see: <https://bristoluniversitypress.co.uk/journals/global-political-economy>.

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PART V

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FLAWS

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CHAPTER 26

MONEY

MARK COPELOVITCH AND JAMES ANDERSON

INTRODUCTION

ONE need only be a casual consumer of the news of the last decade to be aware of the pivotal role of money and exchange rates in the global economy and in international and domestic politics. In Europe, the Eurozone crisis and its painful economic aftermath have dominated events in the European Union (EU) since 2010, reshaping domestic politics in Greece and Italy and triggering sharp clashes between the “North” and “South” in the monetary union. In the US and China, “currency manipulation” and exchange rates have been central to rising economic tensions between the two powers in both the Obama and Trump administrations.¹ In Poland, India, Argentina, Turkey, and elsewhere, currency crashes and exchange rate shocks have had an enormous impact on domestic politics (Ahlquist et al. 2020; Bhavnani and Copelovitch 2016; Steinberg 2015). And in both the global financial crises and the COVID-19 crisis, central bank cooperation and the leadership of the US Federal Reserve have played a central role in managing economic upheaval and mitigating instability (McDowell 2017; Sahasrabuddhe 2019).

In short, it is not an exaggeration to say that monetary issues sit at the heart of every major political debate and policy issue in the world economy. In our view, this makes the study of the politics of money perhaps the single most important topic in international political economy (IPE). As Jeff Frieden notes in his 2015 book, *Currency Politics*:

The exchange rate is the most important price in any economy, for it affects all other prices. The exchange rate is itself set or strongly influenced by government policy. Currency policy therefore may be a government’s single most significant economic policy. This is especially the case in an open economy, in which the relationship between the national and international economies is crucial to virtually all other economic conditions.

Yet, despite its centrality in the world economy and world politics, money remains something of a niche topic of study in the field of IPE. This has long been a feature of the subfield. For decades, the study of money was almost exclusively the domain of economists (Broz and Frieden 2001), until a foundational group of IPE scholars began to explore the politics of

exchange rates and the international monetary system in the latter years of Bretton Woods and the aftermath of its collapse (Cooper 1968; Kindleberger 1970; Strange 1971; Cohen 1977; Odell 1982; Gowa 1988). From these beginnings, scholars in the 1990s and 2000s pushed the literature forward, sharpening our understanding of the domestic and international politics of exchange rates, monetary policy, European monetary integration, and central banking (see, e.g., Frieden 1991; McNamara 1998; Broz and Frieden 2001; Bernhard et al. 2002). Nonetheless, IPE—especially of the “American” or “*International Organization*” (IO) tradition—has long privileged the study of other topics (most notably international trade and foreign aid) over the politics of international money and finance. Even now—as one readily sees from journal tables of contents and conference programs—the number of scholars, papers, and publications on monetary policy, exchange rates, and financial crises in IPE remains quite small relative to those on international trade, foreign direct investment, and (more recently) migration.

The reasons for this ongoing disconnect between the centrality of monetary issues in the world economy and current public policy debates and its relative neglect within the field of IPE are unclear. In part, the answer is surely methodological and data-driven: the shift in political science toward micro-level experimental work in IPE—as elsewhere in political science—has been closely associated with a renewed focus on public opinion about trade and trade policy, while the explosion in data availability has seen a similar surge in studies of bilateral and multilateral foreign aid. As a result, scholarship in IPE on international money and finance remains limited by comparison. Nonetheless, the real-world shocks and events in the global economy since 2008 have inevitably and unavoidably brought monetary issues to the fore and sparked a new generation of outstanding scholarship on the political economy of central banking, exchange rates, and the international monetary system. Yet even within the relatively small group of IPE scholars studying money, the sharp divisions within the subfield described by Pevehouse and Seabrooke (this volume) remain clear, with scholars of different schools approaching the same empirical topics and puzzles from very different theoretical and methodological perspectives. The “constructive non-engagement” of scholars in these different camps is evident in the study of money, as in other issue areas.

In this article, we survey the IPE literature on money over the last two decades, highlighting this problem of “non-engagement.” In taking stock of the field, we seek to identify the many important contributions of each camp, to note outstanding work and scholars that have recently begun to bridge this gap, and to highlight key empirical and theoretical topics where further constructive engagement would enrich our understanding of the politics of money. Substantively, we focus on three core issues, which form the bulk of IPE’s problem-centered approach to studying money since the 1990s: the political economy of exchange rates, the political economy of monetary policy and institutions (primarily central banks), and the politics of the international monetary system. In each of these substantive areas, scholars in both the “*International Organization*” and “*Review of International Political Economy*” IPE traditions have made enormous progress in exploring how domestic and international politics shapes outcomes. And yet, as in other topic areas, the dialog and engagement between the two camps remains insufficient and limited. Nonetheless, as we describe here, this has begun to change and there is great scope for productive synthesis between the two camps.

NICHES WITHIN THE NICHE: IDEOLOGICAL DIVIDES IN THE IPE OF MONEY

Within international relations, IPE still remains something of a niche subfield, with the IPE of money a further specialty. As Seabrooke and Young document in their important 2017 *RIPE* article “The Networks and Niches of International Political Economy,” IPE as a subfield of international relations remains quite balkanized, with scholars from different parts of the world and different intellectual traditions within IPE clustering into separate networks of citations, conference attendance, and topical/methodological approaches. They note, as illustrated below, that this is most clear in graduate school syllabi, where (broadly) scholars have clustered into two camps, which we might label the “*International Organization*” (*IO*) and the “*Review of International Political Economy*” (*RIPE*) schools, based on what is seen as the premier academic journal for each variety of IPE scholarship (Figure 26.1).

This stark division into separate but parallel research communities has sparked much consternation in the last decade, particularly from scholars more closely associated with the *RIPE* camp. Benjamin Cohen (2017), in particular, has lamented the dominance of the Open Economy Politics (OEP) paradigm in “American” (or *IO*-style) IPE (Lake 2009), and he has expressed his disappointment in the evolution of the subfield from the 1990s onward:

The main message of this essay is—to be blunt—disappointment. Research has become increasingly insular and introspective, largely detached from what goes on in the real world. Two parallel developments are responsible, both reflective of wider trends in mainstream IPE. First is a steep decline of interest in broader systemic issues, as the pendulum has swung sharply toward the domestic level of analysis. And second is a marked loss of interest in practical policy solutions . . . The fault lies, first and foremost, with the excessive priority given to formal scientific method (1).

Cohen laments that modern IPE scholars, especially in the *IO* camp, no longer do systemic analysis of the sort done by pioneering early scholars such as Strange (1971), Kindleberger (1973), and Cohen (1977). Quite clearly, given the scholars he cites, the lack of attention to money and the international monetary system is central to this lament.

Cohen raises important and thoughtful critiques of the *IO* camp and the OEP approach to studying IPE. Nevertheless in our view, Cohen’s first lament about the decline of interest in systemic issues is an empirically inaccurate assessment of the subfield. Following the collapse of the Bretton Woods system in 1971–1973, the world was left without a default standard in the international monetary system. In the absence of a clear organizing standard or norm for exchange rate policy and base currency—as in the gold, gold-exchange, and gold-dollar eras of 1870–1914, the interwar era, and Bretton Woods—the world has moved toward an international monetary “non-system” (Ocampo 2017), comprised of the national exchange rate policies of individual countries. In this post-Bretton Woods world, IPE scholarship was inevitably going to shift toward analyzing national policy choices instead of “systemic” analysis of the international monetary system, and scholars were inevitably going to shift their analysis toward domestic political factors as key explanatory variables. To lament this as “insular” or “introspective” is to lament history, not the evolution of the field of IPE or the literature on money.

REVIEW OF INTERNATIONAL POLITICAL ECONOMY

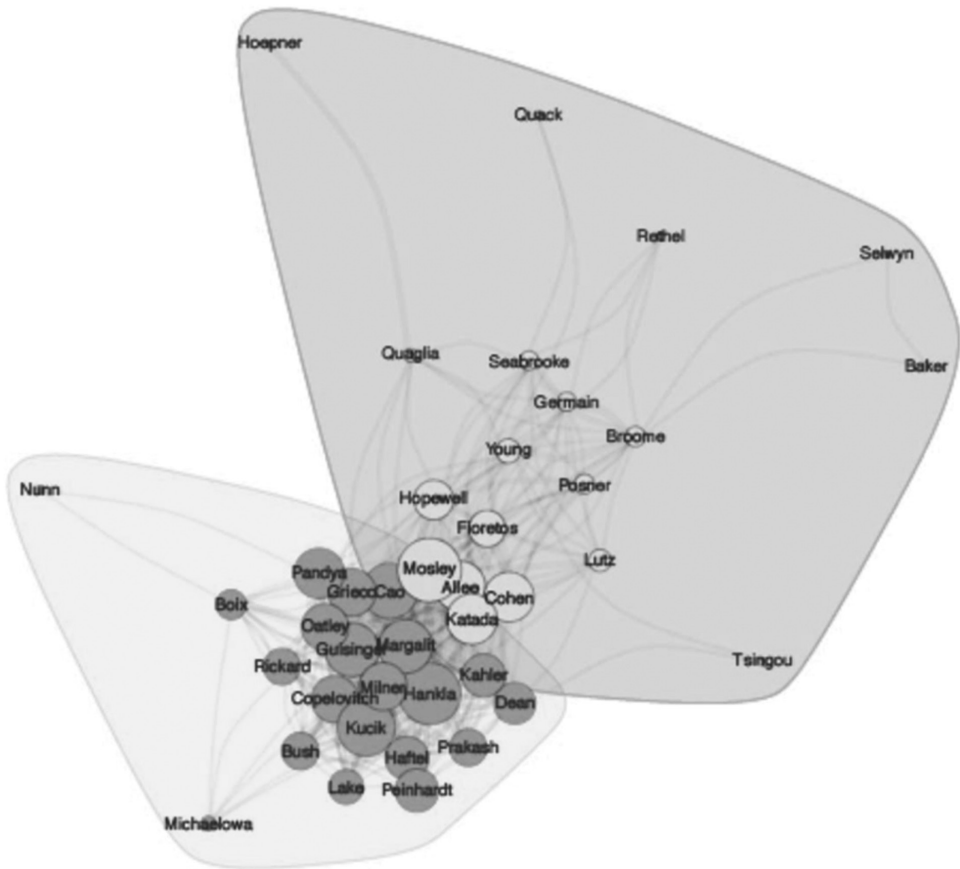


FIGURE 26.1. Network of IPE scholars and ties representing common journals assigned to graduate students.

Source: Reprinted from Seabrooke and Young (2017).

Moreover, many IPE scholars *have* quite clearly continued to focus on systemic analysis. This is readily seen in the rich and diverse literature on the political economy of European monetary integration and the Eurozone crisis from scholars in both the *IO* and *RIPE* camps (see Copelovitch et al. 2016; Matthijs and Blyth 2015). Even at the global systemic level, important new work in recent years has explored the political economy of reserve accumulation (Jäger 2016), foreign currency swaps (Sahasrabuddhe 2019), and the Federal Reserve’s role in global monetary and financial governance (McDowell 2017). Indeed, our understanding of how domestic and international politics shapes national policy choices and international monetary cooperation is deeper and richer than ever. That this work, and the specific topics on which it focuses, are not the same as in the 1970s and earlier eras is to its credit. The world has changed enormously in monetary and financial terms in the post-Bretton Woods era of globalization. IPE scholars have done an admirable job adapting their research to that ever-changing world.

Cohen's second lament is that the IPE of money as a field has given up on finding "practical policy solutions," focusing instead on "puzzles to be explained" through the application of the scientific method to the political world (3). Here, as in earlier related work (Cohen 2010), Cohen lays the blame at "American" IPE in the "Open Economy Politics" (OEP) tradition (Lake 2009), which he criticizes as having sacrificed interesting questions and important policy-oriented research on the altar of statistical methods and rationalist ontology. Other "British" IPE scholars have leveled related criticisms of "American" IPE, claiming that IPE scholars' failure to predict major events in the world economy (e.g., Brexit, the 2008 global financial crisis) is evidence of the crisis of the subfield (Blyth and Matthijs 2017).

In our view, this juxtaposition of "practical policy solutions" against "puzzles to be explained" is a false dichotomy. Indeed, the best IPE scholars and scholarship of recent years stands out for simultaneously explaining important research puzzles *and* for speaking to contemporary policy debates and current events, including the Eurozone crisis, the global financial crisis, monetary policy debates, and Brexit. The very best IPE scholars, whether of the *IO* or *RIPE* camp, all do this. But there is a crucial difference between conducting policy-oriented research and trying to predict the future. The latter is most frequently a fool's errand, and the failure to predict discrete events represents neither an existential crisis for "American" IPE (indeed, it is a failure fully and broadly shared by nearly all "British" IPE scholars and nearly all economists) nor the falsification of the probabilistic hypotheses of any specific IPE theories.

Instead, as Seabrooke and Young's analysis (2017) makes clear of the field as a whole, the main problem with today's IPE scholarship on money is not that any particular school is on the wrong track. Rather the problem is that scholars from the different networks and schools do not engage sufficiently in conversation with one another. This is unfortunate, because IPE scholarship on money is flourishing around the world, and both the *IO* and *RIPE* camps have much to learn and offer to each other. Moreover, where conversations between the two camps have occurred, new communities are emerging that promise to push the field forward, moving us from fragmented niches to productive synthesis.

"CONSTRUCTIVE NON-ENGAGEMENT" IN THE IPE OF MONEY

Nearly two decades ago, Broz and Frieden (2001), in a still-essential survey of the political economy of international monetary relations at the time, identified three core challenges for the field: (1) clarifying outstanding theoretical and empirical ambiguities; (2) integrating study of the domestic and international politics of money; and (3) exploring the connections between money and other issues in IPE, including trade and finance. It is striking—particularly as one of us began studying IPE at exactly this time—to look back now and see how much more we know about the politics of monetary policy, institutions, and exchange rates than we did at the turn of the century. Although the schools of IPE noted by Seabrooke and Young have mostly been "parallel processing" over the last two decades, the accumulation of knowledge in the field has been deep and broad.

Clarifying Theoretical and Empirical Ambiguities

Scholars of both the *IO* and *RIPE* camps have made enormous progress on the first challenge highlighted by Broz and Frieden: clarifying outstanding theoretical and empirical ambiguities in the field. Indeed, we know vastly more about the sources of individual and firm preferences over exchange rate regimes and levels than we did 30 years ago, when Frieden wrote his groundbreaking piece, “Invested Interests” (1991), which drew on theories of the distributional consequences of trade and capital mobility to develop a theory of actors’ preferences over both the level and degree of fixity of the exchange rate. Building on Frieden’s work, scholars in the *IO* tradition have spent much of the ensuing two decades exploring further, in richer theoretical and empirical detail, how the distributional consequences of monetary and exchange rate policy choices shape domestic political coalitions for and against particular policies (e.g., Simmons 1994; Henning 1994; Broz 1997; Frieden and Stein 2001; Frieden 2002; Bearce 2003; Steinberg and Shih 2012; Steinberg 2015; Quinn et al. 2020). Scholars have also made substantial progress in understanding how political institutions interact with interests to shape policy outcomes. One particularly rich literature has shown how the politics of money varies based on regime type. Traditionally, this work has focused on democracy and variation in democratic institutions (e.g., Leblang 1999; Broz 2002; Sattler and Walter 2010; Bearce and Hallerberg 2011), although important new work has extended this approach to authoritarian regimes (Steinberg and Malhotra 2014). Steinberg (2016) also shows that the key may be less about regime type than the government’s role in the economy. He shows state control of the financial system, as is common in “developmental states,” is associated closely with undervalued exchange rates in the developing world.

Stefanie Walter’s work perhaps best exemplifies this *IO*-style approach to examining the interplay between material interests and institutions at the domestic level. Focusing on East Asian countries during the 1997–1999 financial crisis, Walter demonstrates how domestic actors’ material vulnerability to exchange rate and interest rate movements shaped their preferences—and government policy choices—over currency devaluation (Walter 2008). Similarly, in her outstanding 2013 book, *Financial Crises and the Politics of Macroeconomic Adjustment*, Walter shows how voter and firm preferences over internal vs external adjustment policies also shaped policy choices in response to the global financial crisis. The strength of Walter’s work, as well as the rich literature cited above, is its ability to pinpoint which specific economic and political actors favor particular monetary policy choices for material economic reasons. Consequently, it has provided us with a strong theoretical foundation from which to identify the individuals, firms, governments, and policymakers that favor or oppose fixed exchange rates, currency appreciation or depreciation, and interest rate changes in response to macroeconomic shocks.

At the same time, scholars in the *RIPE* tradition have also produced outstanding work highlighting how norms, ideas, and non-material factors also shape actors’ preferences and policy outcomes in the realm of money. Kathleen McNamara’s groundbreaking book on the ideational roots of European monetary integration, *The Currency of Ideas* (1998), remains a foundational work in the IPE of money. In more recent work, Matthijs and McNamara (2015) focus on how narratives shape policy responses to financial crises in the European Monetary Union. They show how a dominant narrative about “Northern Saints” and “Southern Sinners” pervasively shaped EU policymakers’ response to the Eurozone crisis

and prevented deeper fiscal integration that might have more effectively resolved the debt problems of Greece and other southern Eurozone countries. James Morrison's study of the role of ideas in determining Britain's fateful 1931 suspension of the gold standard provides a fascinating historical analysis with echoes ringing through today (Morrison 2016). Furthermore, several recent scholars have produced excellent work showing the role of socialization and ideas in shaping the politics of the International Monetary Fund (IMF), capital account liberalization, and policy responses to financial crises, policies closely linked to money and exchange rates (e.g., Chwioroth 2009; Nelson 2014; Farrell and Quiggin 2017).

In addition to the literature on the political economy of exchange rates, the literature on central bank independence and monetary institutions is a second area where scholars in both camps have made substantial progress in clarifying the theoretical and empirical ambiguities noted by Broz and Frieden in 2001. The early, foundational literature on central bank independence (CBI) focused exclusively on its domestic political economy origins (Kydland and Prescott 1977; Barro and Gordon 1983; Rogoff 1985), setting out the logic of delegation as a solution to the time-inconsistency problem. Subsequent work in economics and political science has highlighted the key role of other institutions, partisanship, and other contextual factors in shaping the effectiveness of CBI as a commitment mechanism for achieving low inflation (e.g., Hall and Franzese 1998; Franzese 1998; Iversen 1999; Franzese 2001; Bernhard et al. 2002; Hallerberg 2002). Scholars in both the *RIPE* and *IO* traditions have also deepened our understanding of the adoption and spread of CBI and related monetary policy rules, such as inflation targeting, around the world (e.g., McNamara 2002; Johnson 2006; Mukherjee and Singer 2010).

More recently, new research has provided us with both better data and richer, more sophisticated theories of how the macroeconomic and institutional context in which independent central banks operate shapes policy outcomes (Copelovitch and Singer 2008; Broz and Plouffe 2010; Bodea and Hicks 2015; Garriga 2016). Cristina Bodea's excellent and prolific work exploring the complex interactions between CBI, exchange rate regimes (Bodea 2010), fiscal policy (Bodea 2013), and political institutions (Bodea and Hicks 2015) is emblematic of this important literature. A key takeaway from this work is that independent central banks cannot be understood merely as technocratic, global solutions to the time-inconsistency problem, but rather must be analyzed as agencies whose effectiveness is conditional and varies widely depending on the broader political and economic context in which they operate.

Indeed, IPE scholars in both camps have shown convincingly that central bankers are not simply technocrats, but rather deeply political actors, whose policy preferences and behavior are shaped by the domestic and international political context in which they operate. Adolph (2013) shows that central bankers' career incentives and trajectories matter. Specifically, he finds that central bankers concerned with future employment in private finance systematically adopt different monetary policy stances—favorable to the financial sector—than those from other backgrounds. Focusing on financial regulators (many of whom are central bankers), Singer (2004, 2007) demonstrates how regulators frequently pursue international regulatory harmonization as a way to circumvent or alleviate domestic political pressure. In related work, Clark and Arel-Bundock (2013) show that the US Federal Reserve is “independent but not indifferent,” exercising partisan bias in its monetary policymaking. They find that interest rates are more likely to fall when Republicans control the White House, but more likely to rise when Democrats do. In other words, the Fed is not merely a technocratic

agency, but rather a “conditional inflation hawk,” whose policy behavior is deeply responsive to domestic politics.

A separate but equally important body of research has shed light on the role of trust, speech, ideas, and communication in central banking. Braun (2016) focuses on public monetary trust, arguing that central bankers nourish the public’s folk belief that it directly controls the money supply to bolster their legitimacy and credibility. Braun (2015), Schmidt (2014), and others highlight how the management of expectations—both those of the public and those of the financial sector—poses challenges as well as opportunities for central bankers. Baerg (2020) also argues for the importance of central bankers’ speech, demonstrating that the content of central bankers’ statements has varying and substantial effects on both inflation and public opinion. Johnson (2016) highlights how the socialization of Eastern European central bankers in post-Communist countries into the transnational central banking community shaped their ideas about monetary policymaking. Collectively, this work makes clear that sociological perspectives have as much to offer in explaining the politics of central banking as rationalist materialist theories. A key challenge moving forward is for scholars of both the *IO* and *RIPE* camps to synthesize their work to understand the conditions and cases under which each set of theories and variables best explains variation in policies and outcomes.

Integrating International and Domestic Politics

IPE scholars of both camps have also made remarkable progress in meeting Broz and Frieden’s second challenge: integrating the domestic and international politics of money. The articles in the 2002 *IO* special issue on the political economy of monetary institutions remain foundational (Bernhard et al. 2002), highlighting the central role of interest groups and political institutions in shaping monetary policy preferences and exploring the degree to which fixed exchange rates and central bank independence are complements and substitutes under different political circumstances (Bernhard and Leblang 2002; Broz 2002; Clark 2002; Hallerberg 2002; Keefer and Stasavage 2002; Freeman 2002; Frieden 2002). Related work by these scholars and others further developed our understanding of the politics of speculative attacks and currency crises by modeling the strategic interaction between international investors and national governments (Leblang 2003; Leblang and Satyanath 2006).

More recently, scholars have explored how international factors influence countries’ vulnerability to currency and financial crises and shape governments’ decisions to devalue or resist exchange rate movements. For example, Moschella (2015) shows how changing ideas about central bank policymaking—specifically, the rising transnational embrace of “macroprudential” tools and awareness of the financial stability risks of monetary policy and exchange rate fluctuations—led the Swiss National Bank to intervene in foreign exchange markets to prevent the Swiss franc’s appreciation in the face of large capital inflows. Focusing on the case of the Chinese renminbi (RMB), Weiss and Wichowsky (2018) find that US pressure shaped the timing of Chinese exchange rate adjustments in 2010. Finally, Bauerle Danzman et al. (2017) show that global capital cycles—driven by the scale of US net borrowing in global financial markets—may be more important than domestic institutional and political factors in determining which countries experience banking and currency crises.

A related body of new research has shed light on the domestic political effects of monetary shocks and monetary policy. Focusing on Poland in the wake of the 2015 Swiss franc shock, Ahlquist et al. (2020) show that exchange rate shocks can affect voters' policy preferences, partisan policy responses, and voting decisions in foreign countries, when those voters are exposed to the shock through foreign currency borrowing. They find that Polish voters holding Swiss franc-denominated mortgages—and who were therefore directly exposed to the franc shock—favored generous bailout policies from the government and were more likely to switch their vote in the October 2015 Polish national election to the opposition party that offered it, the populist Law and Justice party (PiS). Gyöngyösi and Verner (2018) find similar evidence in Hungary, where foreign currency exposure increased support for Jobbik, the far-right party, in the 2010 election. This work demonstrates that exchange rate shocks can have substantial domestic distributional effects at the individual level, and that these effects can influence voters' political preferences and voting behavior when foreign currency exposure becomes a politically salient electoral issue. It also shows how integrated global financial markets serve as a transmission belt carrying national economic policy choices beyond borders, with foreign currency-denominated capital flows and exchange rates as key mechanisms. In a related vein, Arias (2018) shows that spillovers from US monetary policy and global financial cycles affect patronage politics and corruption in developing countries by influencing the budget constraints of local and national governments. A key contribution of this work has been to specify and test more fine-grained theories emphasizing specific channels linking international monetary factors to domestic politics. This work also contributes to broader debates in political science about the sources of the rise of populist and far-right parties in recent years, highlighting the key role that global economic factors have played in several key cases.

Another important area of research integrating domestic and international politics has been the study of European monetary integration and the politics of the Eurozone crisis (Matthijs and Blyth 2015; Copelovitch et al. 2016; Frieden and Walter 2017). Here, again, scholars in both the *IO* and *RIPE* camps have made enormous progress in recent years. Among the *IO* camp, a number of scholars have focused on the domestic politics of the Eurozone crisis, highlighting the distributional conflict surrounding crisis management in both deficit and surplus countries and exploring individual and firm-level preferences over Eurozone membership and crisis policies (Fernandez-Albertos and Kuo 2016; Bernhard and Leblang 2016; Walter et al. 2018; Walter 2016). Others have focused on the international politics of the crisis, exploring the bargaining between Eurozone countries over the size and terms of IMF/Troika bailout packages (Schneider and Slantchev 2018) and within the European Commission over EU fiscal surveillance during the crisis (Baerg and Hallerberg 2016).

Within the *RIPE* camp, several scholars have emphasized how ideas and narratives about appropriate macroeconomic policies shaped German and European views about austerity policies and fiscal integration (Matthijs and McNamara 2015; Matthijs 2016). This work has been crucially important in showing how social narratives about frugal northern Eurozone countries and profligate southern countries emerged and have come to dominate policy debates about monetary and fiscal policy in the Eurozone. Quaglia and Royo (2015) show how variation in the structure of national financial systems shaped how different Eurozone countries (specifically, Italy and Spain) experienced the crisis. Lütz et al. (2019) argue that the differing bureaucratic cultures of the IMF, European Commission, and European Central

Bank fueled clashes over how to resolve the Eurozone crisis and influenced decisions about conditionality and structural adjustment in Troika lending programs.

Finally, scholars in both camps have explored domestic and international linkages in promising new work on the politics of international reserve currencies. In two excellent papers, Liao and McDowell explore the rise of the Chinese RMB as a major reserve currency. In the first article (Liao and McDowell 2015), the authors explore the rise of bilateral Chinese currency swaps, showing that trade, investment, and related agreements make some countries more likely to enter swap agreements with China than others. In a second piece (Liao and McDowell 2016), the authors show that geopolitical distance from the US and affinity for China is a key factor explaining why some states diversify into RMB reserves faster than others. In related work, McDowell (2020) explores the opposite side of this issue: countries moving away from holding dollars as their primary international reserve currency. Focusing on Venezuela, Russia, and Turkey, he finds that US sanctions can increase “political risk” and weaken the attractiveness of holding dollars for targeted countries (McDowell 2020). A range of scholars in the *RIPE* camp have also explored the structure of the international monetary system and the sources of the dollar’s persistent hegemonic role (e.g., Kirshner 2008; Germain and Schwartz 2014; Cohen and Benney 2014; Norrlof 2014; Stokes 2014; Hardie and Maxfield 2016). Most recently, a cross-camp group of scholars has engaged on these topics in a symposium on “Global Monetary Order and the Liberal Order Debate” (Norloff et al. 2020). The pieces in this symposium highlight the continued “centrality of the US dollar and American financial power” in the last decade, despite the global financial crisis and broader concerns about US withdrawal from hegemony and isolationism under President Trump. Collectively, this work epitomizes the sort of “constructive engagement” that can occur when scholars from the *IO* and *RIPE* camps interact, and it provides a perspective markedly different from that presented by international security scholars. Across both camps, IPE scholars have demonstrated how monetary and financial power, the embeddedness of the dollar as the dominant global reserve currency, and the increasing centrality of the Federal Reserve as the de facto international lender of last resort continue to bolster American hegemony, despite the rise of China in world affairs and the Trump administration’s hostility to globalization and international cooperation. This is a key contribution of IPE scholarship on money and an achievement enhanced by the diversity of our subfield.

Links between Money and Related Issues

Finally, IPE scholars have made important, though less extensive, progress on meeting Broz and Frieden’s third challenge: exploring the linkage between money and other issues in the field. This is most evident in the burgeoning literature exploring the political economy of trade and exchange rates in tandem. Scholars in the *IO* camp have made particular progress in this regard, focusing on the interplay between governments’ trade and exchange rate policy choices. Copelovitch and Pevehouse (2013) show that countries are less likely to adopt fixed exchange rates and more likely to have undervalued currencies when they have a trade agreement with the “base” country to which they traditionally fix their currency or whom is their largest trading partner. Betz and Kerner (2016), in a similar vein, show that developing countries are more likely to initiate World Trade Organization (WTO) trade disputes when

overvalued real exchange rates put exporters at competitive disadvantage. Both papers highlight how exchange rate fixity and level affect trade through multiple channels. This work builds on important work in economic history (e.g., Eichengreen and Irwin 2010) showing that trade policy and exchange rate policy also acted as substitutes during the Great Depression, when countries that remained on the gold standard longer were more likely to impose tariffs than those that devalued earlier.

In other work, scholars have focused on the firm level and found similar evidence that exchange rate policy and movements shape trade and investment flows and policy preferences. For example, Jensen et al. (2015) find that persistent currency undervaluation is associated with increased vertical foreign direct investment and intrafirm trade by US multinationals, and that currency undervaluation is associated with more anti-dumping filings by US firms. Similarly, Broz and Werfel (2014) find that anti-dumping petitions by US firms are correlated with currency appreciation, but only in sectors where exchange rate pass-through is high. These studies highlight how carefully specifying government and firm-level preferences over both trade and exchange rate policies shed light on the interaction between the two, linking what have traditionally been separate silos of IPE literature.

These advances are crucial, but there is clearly much more work to be done here. Given the exchange rate's position as the "single most important price in any economy" (Frieden 2015), as well as the pervasive influence of monetary policy—especially the US Federal Reserve's interest rate decisions—on global capital flows, money and exchange rates are inextricably linked to literally every other topic in IPE: trade, finance, migration, and others. Yet scholars have only just begun to scratch the service of bridging the classic silos of the field. Promising recent work points the way forward, theorizing and testing hypotheses about the interactions between exchange rates and remittances (e.g., Singer 2010), wage bargaining institutions (Manger and Sattler 2019), and migration (Peters 2015). More work in this vein is crucial and would contribute productively to the ongoing and important discussion in the subfield about taking network and systemic effects more seriously (e.g., Oatley 2011; Chaudoin et al. 2015; Blyth and Matthijs 2017; Chaudoin and Milner 2017; Cohen 2017).

UNITING THE CLANS? TOWARD MORE CONSTRUCTIVE ENGAGEMENT

As this brief survey illustrates, IPE scholars of both the *IO* and *RIPE* camps have made enormous progress in meeting the challenges set forth by Broz and Frieden nearly two decades ago to further our knowledge of the politics of international monetary relations. Yet as this volume and article both make clear, much of that progress has occurred through a process of "parallel processing" or "constructive non-engagement." Too many of us (present company included) spend most of our teaching and research time within the clan that we were raised intellectually as scholars, from graduate school onward. This is unfortunate, as research from both perspectives has enriched our understanding of the politics of exchange rates, monetary policy, central banking, and currency crises. Moreover, any serious scholar of these topics will readily admit that the explanatory variables emphasized by the other camp—material interests and institutions on the one hand; ideas, norms, and socialization on the

other—matter for understanding the domestic and international politics of money. Likewise, any serious IPE scholar will also readily admit that we do not yet understand enough about how these variables interact to shape economic, policy, and political outcomes, or about the specific cases and circumstances under which one set of variables is more influential than the other.

Figure 26.2 conceptualizes what we see as the three key dimensions of inquiry and variation in IPE between the *IO* and *RIPE* traditions. Although scholars in both traditions fall at all places along each axis, *IO* scholars have historically, as a group, fallen broadly within the positive X-Y-Z space: emphasizing more positivist and more materialist theories and tending to privilege quantitative methods over qualitative research. In contrast, *RIPE* scholars have historically, as a group, fallen broadly within the negative X-Y-Z space: emphasizing more normative and more ideational theories and tending to privilege qualitative methods over quantitative research. As this article has sought to illustrate, however, these broad brush characterizations are increasingly obsolete stereotypes: scholars in both traditions in 2020 are scattered across all three dimensions, and productive interactions and conversations between scholars in the two camps have deepened substantially in the last decade. Where once one could discern two discrete vectors of IPE scholarship, there is now more of a cloud of multidimensional work. This is undoubtedly for the better.

And yet, the process of “constructive engagement” between scholars in the realm of money remains an ongoing and incomplete project. It is up to current and future generations of monetary scholars in IPE to “unite the clans”—or at least to bridge the gaps between them and ensure that the clans interact and collaborate with each other more frequently in the years ahead. One must be cautious about generalizing from personal experience, but our own sense is that this deeper engagement is beginning to happen, as cross-camp attendance at subfield conferences diversifies, as younger scholars become more accustomed than their predecessors to engage across subfield, geographic, and methodological divides earlier in their career, and as groups of scholars coalesce around specific substantive topics in spite of their different training and approaches. To date, these interactions still remain more of the exception than the rule. But one hopes that these steps toward “constructive engagement” grow into regular patterns of behavior in the years to come.

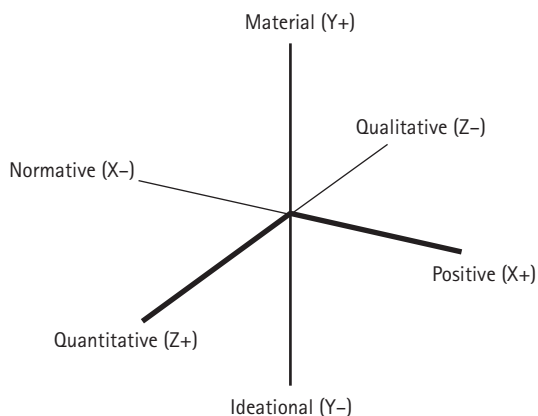


FIGURE 26.2. Dimensions of variation in IPE research on money.

CONCLUSION

The study of money remains at the heart of IPE, and combining the theoretical, empirical, and methodological diversity of the two camps is the best way to understand the pressing research questions and policy debates of the day. Our subfield must continue to explore variation in the monetary (and fiscal) policy responses to the COVID-19 crisis, both across the Atlantic, within the EU, and more broadly around the world. We must keep trying to explain the political factors behind the observed paradox in national policy responses to economic crises over the last decade—the combination of aggressive, unconventional, and countercyclical monetary policy coupled with extensive, procyclical fiscal austerity. And we must further engage with the future of dollar hegemony and the evolution of the international monetary system in a time of upheaval and uncertainty about the trajectory of trade policy, global capital flows, and the future of American leadership. Without a doubt, both camps have important things to say about these pressing topics, and the best and most policy-relevant scholarship going forward will take seriously both material and institutional factors and non-material and ideational variables, as well as the complex and inevitably important interactions between them.

Ultimately, more extensive interaction and collaboration between scholars of the *IO* and *RIPE* camps—and, dare we say it, increased cross-publication of authors in the relevant journals—will not only produce richer theoretical and empirical insights about the politics of international monetary relations, it will also represent a return to the founding years of the subfield. Indeed, as McNamara (2009) noted a decade ago in her discussion of the separate intellectual monocultures of IPE, “constructive non-engagement” is a “misguided departure from the pluralism that once defined the discipline” (74). In the study of money, as in other areas of IPE, it is long past time to return to first principles.

NOTE

1. <https://www.npr.org/2019/08/06/748775639/u-s-china-trade-war-spreads-from-tariffs-to-a-battle-over-currency>; <https://www.reuters.com/article/us-usa-china-obama-currency/obama-says-china-must-stop-manipulating-currency-idUSTRE49S7FQ20081029>; <https://thehill.com/homenews/campaign/509685-biden-under-trump-attack-casts-himself-as-firm-on-china>

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CHAPTER 27

TRADE

ELISABETH VAN LIESHOUT

INTRODUCTION

IN 2019, 19 trillion US\$ worth of goods were traded between countries, the same amount as the combined GDP of China, India, and Brazil or 22 percent of world GDP.¹ This is a ten-fold increase compared to 35 years earlier, and a 300-fold increase over another 35 years before that. Governments have put a great deal of effort into shaping and cajoling trade flows, both by imposing protectionist barriers and by cooperating to eliminate them. The effects of trade flows have been both credited for rapid economic growth across the world and blamed for increasing inequality and the global rise of populism. In short, trade flows are a central force on today's globalized economy.

This article lays out our current understanding of trade flows: where do they come from, what affects them, and what do they affect in turn? I focus on these questions as an empirical problem, looking at what the best available evidence can teach us, while also touching on the theoretical explanations we have for why these relationships exist.² Following recent developments in trade theory, I center firms in this discussion, as they are the main actor engaged in exporting. I draw equally upon work from within international political economy and from economics, where much of the empirical work on trade's causes and effects originates.

Identifying the drivers and effects of trade flows is a complex empirical problem, and there are three recurrent challenges that make credible estimates difficult. First, the proper econometric specification for so-called gravity models of trade, the workhorse approach to modeling trade flows, has been subject to continuous updating. Second, detailed, consistent measurements of both trade flows and the corresponding independent or dependent variable are often elusive, particularly when searching for data available over a large range of countries and time. Third, since the causes and consequences of trade frequently overlap (as discussed more below): reverse causation is almost inevitably a threat to inference. Fortunately, there have been tremendous improvements in recent years on all three of these fronts, which has greatly increased confidence in our estimates.

The rest of this article will proceed as follows. The first topic is the drivers of trade, the underlying economic factors that can make international exchange worthwhile as well as the

policies that shape them. Next, I turn to the wide range of factors that can moderate these trading relationships, examining the role of transaction costs, of geopolitical context, and of trade policy. Then follows a discussion of the effects of trade, focused on asking when and how trade flows can create positive feedback loops or undermine their own success. Lastly, I conclude by discussing the divides both within and outside the literature on international political economy.

DRIVERS

The underlying fundamentals that give rise to international trade flows have been explored extensively within the economics literature. However, this does not mean the drivers of trade are devoid of politics. A wide range of policy choices, far beyond what can be considered direct trade policy, contributes to the circumstances that drive the exchange of goods between states. Both the international and comparative political economy literature have pointed out some of these connections, although scope for further work on this intersection remains.

The longest-standing explanation for trade flows is the idea of “comparative advantage”; some countries are better suited to the production of some goods than others, and specialization and exchange allows for more efficient allocation of resources. These differences can come from countries’ factor endowment: their relative availability of capital, labor, and land (Dixit and Norman 1980; Schott 2004). This explanation implies that the exchange of these factors, that is, migration and capital flows, could be substitutes or creators of trade flows, and that any policies governing these flows have implications for trade too (Peters 2017; Quinn and Inclan 1997).³ Comparative advantage can also originate in differences in available technology (Costinot et al. 2012; Eaton and Kortum 2002): meaning that research and development policies are also a lever affecting trade (Haaland and Kind 2008). Lastly, policies and institutions can themselves be a source of comparative advantage; for example, stronger contract enforcement can make countries more suited to the production of contract-intensive goods (Levchenko 2007; Nunn 2007).

Under ‘new’ trade theory, the driver of exchange between countries is economies of scale: goods are often produced more efficiently in larger quantities, meaning exporting to a world market allows for a larger range of differentiated goods, which is particularly beneficial when consumers have a taste for variety (Evenett and Keller 2002; Krugman 1980). Most recently, “new new” trade theory has turned our attention to variation between firms as a major driver of trade, as exporting firms are consistently larger and more productive than those producing only for domestic sales (Bernard et al. 2007; Melitz 2003). Both these ways of thinking about trade points to the importance of the individual firm, which produces a specific variety with its own technology. Consequently, industrial policy which creates the circumstances for certain firms to grow into superstars is also a form of trade policy (Aghion et al. 2015; Harrison and Rodríguez-Clare 2010).

Trade flows are not determined in isolation: trade is frequently a substitute or complement for other types of international exchange. Flows of labor and capital were already mentioned above; particularly worth noting is the interaction between trade and foreign direct investment (FDI) (Helpman 2006; Kim et al. 2020). Multinational corporations (MNCs) are

crucial actors in trade; for example, around 90 percent of imports and exports in the US are conducted by MNCs (Antràs and Yeaple 2014). MNCs' investment decisions affect trade flows in two countervailing ways. In cases of "vertical" FDI, where parts of the production chain are moved abroad, this investment leads to an increase in trade flow of goods back to the home market. By contrast, "horizontal" FDI, where sales or production operations are duplicated in multiple locations, means goods can be produced locally for a foreign market and is thus a substitute for trade. Policies and other factors shaping incentives for investment are therefore indirectly highly influential in determining trade flows.⁴

MODERATORS

Even when the potential for international exchange exists, this does not mean trade flows will always follow. The decision on whether to export ultimately lies with firms, who are the crucial actors in making international trade happen (Helpman et al. 2008; Kim and Osgood 2019). In their exporting choice, firms consider the costs involved in trading as well as the uncertainty about those costs. Since exporting generally involves an upfront fixed cost, investing in ties with foreign markets may only be worthwhile if barriers to trade are not only low but also predictable (Handley 2014). Here I consider three bundles of potential moderators of firms' export choices: transaction costs, the geopolitical context, and trade policy.

Transportation and Transaction

International trade, by definition, requires shipping goods from one country to another, often over great distances. One of the earliest and most persistent empirical results in the analysis of trade flows is that distance matters (Disdier and Head 2008). The very name of the "gravity model" comes from the idea that units further away from one another will exert a weaker pull, meaning they will see smaller trade flows.⁵ A simple explanation for the role of distance is that it increases the costs of transportation. Even with the improvements in shipping technology, the costs and delays entailed in moving a good from one place on the globe to another are still non-trivial in firms' export and import decisions (Anderson and van Wincoop 2004; Hummels and Schaur 2013).

While the role of trade costs is persistent, they are neither constant nor immutable by policy (Hummels 2007). The construction of physical infrastructure such as ports (Clark et al. 2004); canals (Feyrer 2009); and railroads (Donaldson 2018) can greatly lower costs and thus enable trade (see also Francois and Manchin 2013; Limão and Venables 2001). With an eye toward the inhibiting role of trade costs, trade facilitation policies have become a common feature of both trade agreements and international development efforts, and measures such as increasing port efficiency and streamlining customs procedures can indeed be effective at increasing exchange (Portugal-Perez and Wilson 2012; Wilson et al. 2003).

International trade also involves transaction costs that are often not present within countries. Merely the presence of a national border, separate from any transportation costs, hampers trade flows (Anderson and Wincoop 2003; McCallum 1995). The presence of

different sets of regulations and the need to acquire country-specific information creates a fixed cost of operating in another market (Chen 2004). In response, both multilateral and preferential trade cooperation has placed a great deal of focus on regulatory harmonization as a way of enabling trade (Grossman and Staiger 2019; Sykes 1999). Harmonization succeeds at increasing trade, notably also for firms in third-party countries as these now face a single fixed cost to exporting into multiple markets (Czubala et al. 2009; de Frahan and Vancauteren 2006; Moenius 2004).

Financing can be another transaction cost. Firms often need access to liquidity in order to make exports feasible, and an insufficiently deep financial market can be a constraint on trade (Manova 2013). Correspondingly, financial crises often cause deeper and more persistent drops in international trade flows than would be expected purely based on their income effects (Amiti and Weinstein 2011; Xu 2020). When having to deal in multiple currencies, firms also face exchange rate risks that could deter trade or lead firms to time their shipments strategically (Bahmani-Oskooee and Hegerty 2007). The creation of a common currency may therefore be an effective trade-promoting measure (Santos Silva and Tenreyro 2010): although the effectiveness of customs unions varies and may have gone down over time (de Sousa 2012).

International transactions also involve interacting with people abroad, and these interactions may be made more difficult by cross-border differences. Language can pose a barrier to trade (Egger and Lassmann 2012): meaning the availability of both translators and migrants to form a bridge can play an important role in facilitating trade (Melitz and Toubal 2014). A sense of shared culture and trust is also associated with greater trade flows, perhaps because even in the presence of strong legal institutions, trade still requires relying on another (Felbermayr and Toubal 2010; Guiso et al. 2009). The interconnectedness created by previous colonization has also long been a strong predictor of trade flows, but these ties seem to erode as time passes since independence (Head et al. 2010).

Geopolitics

As the name indicates, international trade takes place between different states. It therefore stands to reason that the relationships between those states can shape the trade that flows between them. Studies on the effect of international politics on trade differ greatly in the type of international ties and tensions they focus on. Full-scale war is found, perhaps least surprisingly, to strongly reduce trade flows between adversary states (Glick and Taylor 2010). Lower-level but still violent interactions, known as militarized international disputes, also have a negative effect on trade (Hegre et al. 2010; Keshk et al. 2004). Even in the absence of active conflict, states who are adversaries generally trade less with one another while allies trade more (Mansfield and Pollins 2001).

The link between interstate relations and trade can be the result of deliberate policy. Gowa (1995) argues that it is in states' strategic interests to reduce trade with adversaries while encouraging trade with allies. Since trade creates wealth which can be invested in security resources, there are respectively positive and negative externalities to trade with allies and opponents. Whether governments indeed shape trade flows in response to geopolitical interests depends on the relationship between governments and trading firms. Kastner (2007) finds that the relationship between diplomatic relations and trade is moderated by

the strength of internationalist economic interests within domestic politics; if powerful domestic groups stand to lose from trade restrictions vis-à-vis adversaries, such policies become less likely. Conversely, Davis et al. (2019) find that trade by state-owned enterprises is far more affected by political relations, as the government has more direct influence over the import and export decisions of these companies. Davis and Meunier (2011) argue that the incentives for governments to link their economic and security interests have decreased over time, as the sunk costs entailed in trade relations in a globalized economy make such policies far less attractive.

Even in absence of government policy, international tensions can be a reason for firms not to export to adversaries. Violence can raise trade costs through destruction of infrastructure and loss of resources, often to prohibitive levels. The possibility of both violence and new government restrictions creates uncertainty for firms, which can make them hesitant about incurring the fixed costs of creating a new exporting relationship. As a result, the expectation of conflict, even in absence of conflict itself, can already depress trade (Long 2008). Correspondingly, it is particularly unexpected instances of violence that lead to an observable drop in trade (Li and Sacko 2002). Simmons (2005) shows that border conflicts in particular depress trade in the disputed territory because economic actors there face a greater level of uncertainty over future property rights.

Diplomatic interactions beyond alliances and adversaries can also end up shaping trade flows. States frequently engage in economic diplomacy to promote the sale of their firms' products abroad, which Moons and van Bergeijk (2017) find seems to be effective. Perhaps more disconcertingly, countries can also use their influence abroad to directly engineer trade flows. (Berger et al. 2011) show that CIA interventions during the Cold War were followed by increased imports from the US. Conversely, states can use trade as a tool to gain more influence as well; Fuchs and Klann (2013) show that China follows through on its warnings to other states not to host the Dalai Lama, with visits being followed by a decrease in exports to China. Citizens, too, can use trade for diplomatic means: consumer boycotts on products from specific states can be effective at substantially reducing trade flows (Heilmann 2016; Michaels and Zhi 2010).

Trade Policy

A major focus of the international political economy literature has been explaining what determines trade policy. These theories start from thinking about actors who differ in their trade policy preferences as well as their ability to organize politically (Alt and Gilligan 1994; Mayer 1984. Individuals' attitudes toward trade may depend on how trade openness affects them and those around them (e.g., Mansfield and Mutz 2009; Mayda and Rodrik 2005; Scheve and Slaughter 2001. Import-competing industries can seek protectionism, while exporting industries stand to gain from reciprocal liberalization (Gilligan 1997; Grossman and Helpman 1994). Within a given industry, particularly large firms in concentrated sectors are active in trying to see their preferences implemented (Bombardini 2008; Kim 2017.⁶

The interests of these different actors are aggregated through domestic institutions which give more weight to some voices than others. Thus levels of protectionism may depend for example on a country's regime type (Milner and Kubota 2005); its electoral system (Rogowski 1987); the number of veto players (Mansfield and Milner 2012); and the

branch of government tasked with trade policy authority (Bailey et al. 1997). International institutions serve to aggregate the preferences of different states, allowing them to internalize externalities imposed by protectionism (Bagwell and Staiger 1999; Grossman and Helpman 1995); solve domestic commitment problems (Maggi and Rodriguez-Clare 1998); and overcome hold-up problems (Carnegie 2014). The combination of these actors and institutions adds up to a complex set of tariff and non-tariff trade barriers that varies across countries and products.

Identifying the direct effects of these trade barriers on trade flows is not straightforward. One line of work examines the price elasticity of trade, seeing how imports vary in response to a change in product prices (e.g., Imbs and Mejean 2015; Kee et al. 2008; Simonovska and Waugh 2014). But changes in trade barriers do not map one-to-one onto price shifts; another line of work examines the extent of tariff pass-through, seeing to what extent producers or consumers bear the costs of tariffs (e.g., Cavallo and Neiman 2019; De Loecker et al. 2016; Ludema and Yu 2016). But less work combines these two dynamics to map directly from a change in trade policy to a subsequent trade flow effect. One exception is a recent series of papers examining the effects of the 2018 US tariff increases (Amiti et al. 2019; Fajgelbaum et al. 2020; Flaaen et al. 2020).⁷

There is a number of reasons why precisely estimating the impact of a given trade barrier on flows can be challenging. For one, the instatement and removal of barriers may be endogenous to the elasticity of demand for a particular country and product (Bagwell and Staiger 1999; Broda et al. 2008). Moreover, many trade barriers do not come in the form of easily measurable tariffs, requiring the construction of measures of how trade-restrictive a given non-tariff barrier is (Kee et al. 2009). The relationship between barriers and trade also need not be linear. Yi (2003) argues that falling tariffs worldwide did little to encourage increased trade until they were sufficiently low that global value chain production became feasible, which caused an explosion of intermediate product trade.

While the effects of specific forms of protectionism are hard to measure, there is an extensive literature on the effect and effectiveness of various forms of international trade cooperation. Although subject to extensive methodological debates, researchers have found robust evidence that trade flows are indeed increased by multilateral liberalization in the General Agreement on Tariffs and Trade (GATT) and World Trade Organization (WTO) (Goldstein et al. 2007; Subramanian and Wei 2007); by preferential trade agreements (PTAs) (Baier and Bergstrand 2007; Cipollina and Salvatici 2010); and by non-reciprocal arrangements such as the Generalized System of Preferences (GSP) (Gil-Pareja et al. 2014; Tobin and Busch 2019).⁸ Other work has unpacked the when, where, and who of such effects, which can be grouped into several themes.

One way to disaggregate overall effects of trade cooperation is to look at the margins and timings of change. Liu (2009) finds that the GATT is effective at both increasing the amount of trade between pre-existing partners and creating trade flows between previously unconnected dyads. (Dutt et al. 2013) look at the effect of the GATT within country dyads and find that the effects are concentrated in trade in previously untraded products, with no increases in the quantity of already traded goods. Looking at PTAs, (Baier et al. 2014) find that the intensive margin (amount of trade in existing goods) is encouraged in the short run, whereas in the long run there is change on the extensive margin (adding new products). Lastly Herz and Wagner (2011) find that the effectiveness of the GSP is primarily in the short run, with effects disappearing over time.

There are heterogeneities in the effectiveness of cooperation based on the country dyad's pre-existing relationship. Since preferential agreements are not entered into at random, accounting for the selection into such treaties when their potential impact is largest is important in properly estimating their effects (Baier and Bergstrand 2007; Egger et al. 2011). For both the GATT and PTAs, the effects appear to be strongest when dyads are geographically close, experience higher ex ante trade frictions, and have stronger incentives to manipulate their terms of trade (Baier et al. 2019; Eicher and Henn 2011). The effectiveness of GSPs depends in part on whether the developing country is in a position to make use of the granted market access, for example if they have received trade facilitation assistance as a WTO member (Ornelas and Ritel 2020) or if they are already exporting a similar product to another OECD country (Garred et al. 2020).

Reciprocal trade liberalization can also affect different types of firms in different ways. (Baccini et al. 2016) find that PTAs by the US primarily increased the imports from the largest and most competitive subsidiaries of American MNCs in the partner countries. (Spilker et al. 2018) show that the effects of the CAFTA-DR trade agreement depended on whether a firm sought to export differentiated or homogenous products to the US. Firms producing differentiated goods saw an increase only along the extensive margin, meaning they were able to sell more varieties of their goods, while firms selling homogenous goods were the ones to increase their exports along the intensive margin.

Perhaps the most politically interesting heterogeneity to examine is institutional design. Allee and Scalera (2012) find that countries facing stricter accession requirements to join the WTO see greater trade effects as a consequence. This lines up well with Pelc's (2011) finding that countries with more valuable market access face more demanding accession terms. Using different measures of the content of PTAs, (Dür et al. 2014) and (Kohl et al. 2016) find that "deeper" agreements, which span a larger range of issues or entail more substantive commitments on those issues, have larger trade-encouraging effects. However, in a close examination of the tariff schedule of NAFTA, (Besedes et al. 2020) find that the timing of tariff removals is not reflected in the timing of subsequent trade increases, suggesting a need for further work on the mapping between precise agreement commitments and subsequent trade outcomes.

In addition to lowering trade barriers, another major function of trade cooperation is to provide greater certainty over future barriers. Consequently, trade agreements increase not only the level but also the stability of trade flows (Mansfield and Reinhardt 2008). Even when the effective tariffs undergo little or no change, the guarantee that these will not be unexpectedly hiked up in the future can increase firms' willingness to trade. Handley and Limão show this effect in relation to both Portugal's entry into the European Union (EU) (2015) and China's entry into the WTO (2017). It is therefore perhaps unsurprising that access through the GSP, which is perpetually uncertain and at the discretion of the access-granting country, can lead countries to seek reciprocal and binding PTAs instead (Manger and Shadlen 2014).

EFFECTS

Given the enormous scale of trade flows in the current globalized economy, their effects permeate every aspect of society. Here, I focus on two sets of consequences. On the one hand,

trade can be a self-reinforcing phenomenon. The ways in which trade flows affect a country's economic and political make-up can make further trade and trade liberalization more likely. On the other hand, trade is not without downsides: it can cause substantial inequalities. These distributive effects have given rise to doubts about the value of open borders.

Virtuous Cycles

Most broadly, trade leads to gains across the economy: it is associated with economic growth (Frankel and Romer 1999; Wacziarg and Welch 2008): rising real income (Bernhofen and Brown 2005; Feyrer 2019): and falling poverty (Dollar and Kraay 2004). Insofar as citizens base their support for their government on economic success, trade can therefore be a politically attractive proposition. Trade also benefits consumers in the form of lower prices (Bai and Stumpner 2019): lower mark-ups (Edmond et al. 2015; Feenstra and Weinstein 2017): and the availability of more variety (Broda and Weinstein 2006). Baker (2005) shows that these consumption effects can also be meaningful in shaping support for trade liberalization.

Trade also leads to increases in productivity and innovation. This happens both because of a shift in production toward the more efficient firms and because firms themselves invest more into their technological upgrading (Bloom et al. 2016; Pavcnik 2002). In particular exporting firms see an increase in their productivity through scale economies and R&D investment (Aw et al. 2011; De Loecker 2007; Van Biesebroeck 2005). Insofar as trade is driven by differences in technology and productivity, the incentives toward further innovation in the face of a larger market creates a self-reinforcing effect. Moreover, large, productive firms are more likely to engage in the type of foreign direct investment that generates trade flows.

While trade flows more easily when there is no conflict between states, trade itself can also create more peaceful conditions. Trading entails accepting fixed costs and engaging in specialization, which creates an interdependence between states that makes conflict more costly. Even accounting carefully for the possibility of causality running in two directions, it appears to be the case that trade and peace are mutually encouraging (Russett and Oneal 2001). For an in-depth discussion of this literature, see the article by Edward D. Mansfield in this volume.

Increased trade flows also shift the interest group politics of protectionism in favor of further liberalization. As goods flow freer, import-competing industries and firms will shrink or disappear entirely due to increased competition. Exporting sectors and companies, meanwhile, see a growth due to profits from selling abroad. These changes in size translate into changed political influence, with support for liberalization increasing and calls for protection diminishing, locking in open border policies (Bailey et al. 1997; Gilligan 1997). Integration into global production chains also lowers firms' support for protectionism, as trade barriers would also be applied on inputs to their production process or the outputs of their foreign affiliates (Blanchard et al. 2016; Osgood 2017, 2018).

Perilous Inequality

While trade's beneficial effects are substantial, it is not without downsides. While some gain, others are hurt by the process of liberalization, and these differences can create

inequality. Beyond being a normative concern in its own right, this increased inequality also undermines public support for open borders, especially in absence of policies that sufficiently compensate and assist those who lose from liberalization. Such inequality can arise between sectors, between workers, and between regions.

The fact that some sectors would be harmed by trade liberalization has long been acknowledged by the literature on trade, but recent research suggests that the impact might be larger than previously thought. Work on the American manufacturing industry, for example, finds large and persistent effects as a result of China's increased integration with world markets (Autor et al. 2013; Pierce and Schott 2016). Workers in hard-hit sectors see their wages reduced (Hakobyan and McLaren 2016) and for those who lose their job it is hard to move into another industry (Artuç et al. 2010; Dix-Carneiro 2014). The effects of such a negative shock on workers can be both substantial and long-lasting, and particularly affect those with lower initial wages (Autor et al. 2014).

Trade can also cause a divergence in wages between individual workers. Productive firms generally pay more, and trade shifts more economic activity toward them, further increasing wage dispersion within an industry (Verhoogen 2008). Trade also raises the return to human capital, thus leading to a greater "skill premium" or a larger difference in wages between high- and low-educated workers (Burstein and Vogel 2017). As a result, trade translates into an increase in wage inequality both within and across sectors and occupations (Helpman and Redding 2017): an effect that takes place in both developed and developing countries (Goldberg and Pavcnik 2007).

Trade also interacts with economic geography to create divergences between regions. Specific industries and highly productive firms often naturally cluster together into particular areas (Baldwin and Okubo 2006; Krugman 1991). This means that when uncompetitive sectors and firms are hit by a negative trade shock, certain areas are particularly harmed (Caliendo et al. 2019). While in theory people who have lost their jobs could relocate to flourishing regions, in practice geographic mobility is generally low (Diamond 2016). Moreover, floundering regions experience negative agglomeration economies, whereby the decline of some industries leads other firms, high-skilled workers, and investment to leave the area (Glaeser and Gottlieb 2009; Rosenthal and Strange 2004). As a consequence, regions become stuck in an upward or downward spiral depending on how an opening to trade affects them, and different parts of a country diverge (Moretti 2012).

The economic damage done by trade liberalization has translated into increased opposition to globalization as well as support for populist politics (Autor et al. 2020; Colantone and Stanig 2018; Rodrik 2018). However, the negative consequences of trade were not inevitable; compensation and assistance can be given to the workers and regions hurt. Negative shocks from globalization increase support for welfare policies among both voters and parties (Burgoon 2012; Walter 2010): and compensation is effective at cushioning negative attitudes toward trade (Hays et al. 2005; Margalit 2011). But while exporters and their political representatives have some incentive to provide compensation in order to build a pro-trade coalition (Rickard 2015): the political institutions that enabled them to implement trade liberalization also led to the silencing of those who would be hurt by trade (Goldstein and Gulotty 2021). As a result, compensation has been insufficient in most countries to prevent disillusion with globalization (Bisbee et al. 2020).

CONCLUSION

This volume asks whether a disconnect exists between American and European approaches to international political economy. In the literature on trade flows, such a divide is not apparent. Scholars on both continents concern themselves with the same questions and problems: where does trade policy come from, how does it shape trade flows, and what are the political consequences. While the types of arguments and variables emphasized may at times differ, this need not be a problem as these explanations are rarely mutually exclusive. However, two other divides do appear to be present and unproductive.

First, there could be more integration of the work on what explains trade policy, housed mostly in political science, and research on the effects of such policy, mostly done in economics. While treating these as separate questions is often analytically useful, bringing them into closer conversation could lead to valuable insights. In estimating a policy's effect, the fact that its implementation was endogenous to its anticipated effects could be an interesting part of the story rather than a methodological nuance. When asking what leads to a policy decision, it is worth keeping in mind that many policies do not have their intended or anticipated effects. One example of an area of research where the back-and-forth between these questions has been given careful consideration is in the selection into and effect of creating preferential trade agreements, which has translated into a rich understanding of the dynamics of PTAs.

A second divide lies between the literature on trade policy in particular and the broader international and comparative politics literatures on various non-trade economic policies. A much wider range of policy choices than formal trade barriers can have an impact on trade, especially as larger shares of countries' GDP become entangled in international trade. There are at least two productive ways that trade scholars could interact with the extensive work on the drivers and effects of other economic policies. First, it would be valuable to identify when and how the impact on trade might be different from domestic production, for example due to trade's concentration into a small set of large firms or due to the heightened sensitivity of international trade to uncertainty. Second, we can conceive of governments as having a wide range of policy levers in their portfolio of choices when it comes to influencing trade flows, which raises the question of when and why they choose to use different instruments.

Overall, this is a promising time for research on trade flows. Recent work has made great strides in unpacking the heterogeneities that underlie overall effects identified in earlier research. Detailed questions into how trade flows and their determinants have changed over time, how these forces differ by industry and by firm type, and the implications of specific forms of institutional design have greatly enriched our understanding. Improved data on disaggregated trade flows as well as detailed measures of trade policy mean that a lot of previously inaccessible questions are now open to be answered, which promises another wave of innovative research in the near future.

NOTES

1. Source: World Bank.
2. This article focuses solely on the trade in goods, leaving trade in services beyond its scope.

3. For further discussion of the drivers and effects of labor flows, migration, and capital flows, see articles by John S. Ahlquist, Georg Menz, and Mark Copelovitch and James Anderson respectively in this volume.
4. For an overview of the literature on investment flows, see the article by Rachel L. Wellhausen in this volume.
5. For an in-depth review of the theory and estimation of gravity models, see Head and Mayer 2014.
6. For further discussion on the role of households, interest groups, and firms in IPE, refer to the articles in this volume by Genieve LeBaron and Liam Stanley, Iain Osgood, and Glenn Morgan, respectively.
7. Other research has examined the impact of tariff changes on welfare (Caliendo and Parro 2015; Spearot 2016): firm entry and exit (Caliendo et al. 2015; Fitzgerald and Haller 2018): wages (Amiti and Davis 2011; Attanasio et al. 2004): productivity (Amiti and Konings 2007; Bernard et al. 2006; Lileeva and Trefler 2010; Topalova and Khandelwal 2011): mark-ups (Brandt et al. 2017): technology (Fan et al. 2015): and quality (Bustos 2011).
8. For more on the drivers and effects of preferential trade agreements see the article by Leonardo Baccini in this volume.

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CHAPTER 28

FOREIGN DIRECT INVESTMENT

RACHEL L. WELLHAUSEN

INTRODUCTION: FDI AS (VICTIM OF ITS) SUCCESS

THE concept known as foreign direct investment (FDI) is intended to capture economic activity in a given political jurisdiction, over which a foreign actor has meaningful managerial control.¹ International Political Economy (IPE) scholarship builds research agendas on the back of this concept. Statistical offices collect data on this concept. Sovereign states set policies targeting this concept. The United Nations (UN) publishes an annual World Investment Report on this concept, which the Secretary-General calls “an important tool for the international development community.”² In this piece, I probe why we have found it useful to separate this concept from all other forms of international economic activity. In so doing, I highlight how the perhaps remarkable consensus over the parameters of this concept has underpinned major advances in IPE scholarship. I also select on scholarship published in just the last few years to provide evidence.³

I then argue that FDI as a concept is in danger of becoming a victim of its own success. In my view, the major scholarly shortcoming in the field is that we too readily accept the usefulness of cutting international economic activity along the lines of “F,” “D,” and “I.” Scholars that question rather than accept the concept of FDI are unearthing compelling new answers to old questions. Moreover, scholars that question the concept of FDI are spurring creative, high-priority research agendas on new questions. These efforts are picking up in both “scientific” and “critical” research traditions. As examples, I will highlight gains being made by probing the “F” in FDI. First, probing “F” has enabled a body of research that takes seriously the starting point that, say, American investors might be different from other investors in ways that shape the political economy of FDI. Second, much progress has been made from a common conceptualization of “F” as short-handing for large, productive, profit-maximizing multinational corporations (MNCs), but “F” also includes state-owned enterprise (SOE) activities abroad that fit uncomfortably within theories based on non-state actors. Third, selecting on “F” means that the impacts of “F” on domestic firms, and the political consequences, are included in studies of international economic relations only on the

margins. I conclude with encouragement that, beyond the huge progress made due to de facto agreement on the concept of FDI, scholars would do well to continue prioritizing research questions that critically engage with the concept.

CONTRIBUTIONS THANKS TO AGREEMENT ON THE CONCEPT OF FDI

A host state that allows foreign actors to own, operate, and profit from businesses in its geographical territory necessarily constrains the sovereign freedom of action otherwise reserved to it. Susan Strange's turn of phrase "the retreat of the state" is born of the truism that, in order to accept FDI at all, a state retreats (Strange 1996). This need not be the way the world works; the existence of FDI is a manifestation of political choices. It follows that political scientists have a particular comparative advantage over recognizing, and answering, questions arising from this starting point.

Literature that accepts FDI as a key, separate concept in international political economy reinforces how this starting point is different from that of trade, sovereign lending, portfolio investment, remittances, economic migration, foreign aid, and the myriad of other ways in which capital moves across borders. Especially in recent years, important contributions have highlighted the ways in which FDI impacts these kinds of other substantive areas of interest. For example, scholars have explored connections between FDI and central bank independence (Bodea and Hicks 2015); exchange rates (Egan 2017); sovereign debt (Wellhausen 2015a); portfolio investment and financial market development (Alzer and Dadasov 2013; Graham, Johnston, and Kingsley 2018; Pepinsky 2014, 2013; Pond and Zafeiridou 2019; Tomashevskiy 2015); aid and multilateral financial institutions (Biglaiser, Lee, and Staats 2016; Donaubaue, Meyer, and Nunnenkamp 2016; Dreher, Nunnenkamp, and Schmaljohann 2015; Ana C. Garriga and Phillips 2014; Malik and Stone 2018; Williams 2017); and illicit and informal economic activity (Beladi, Dutta, and Kar 2016; Blanton, Early, and Peksen 2018; Jafri 2019; Mandal, Marjit, and Beladi 2018). A growing literature highlights how FDI and trade have intertwined political effects (Bowen and Elsig 2018), with respect to immigration (Kapur 2014; Mosley and Singer 2015; Peters 2014, 2017); treaties (Hicks, Milner, and Tingley 2014; Lechner 2018; Manger and Shadlen 2014; Osgood 2016, 2018; Osgood and Feng 2018; Pelc and Urpelainen 2015); and global value chains (Baccini, Pinto, and Weymouth 2017; Dallas 2015; Gereffi 2014; I. S. Kim et al. 2019; I. S. Kim, Liao, and Imai 2019; Pinto and Zhu 2016; Ravenhill 2014). Other work starting from the concept of FDI reveals further sectoral-level variation in the relationship between this kind of international economic activity and politics (Bogliaccini and Egan 2017; Bunte et al. 2018; Cheon 2019; Christensen 2019; Hughes 2013). For example, scholars have examined the political consequences of the foreign firm's mode of entry (Broome, Homolar, and Kranke 2018; Lee, Biglaiser, and Staats 2014; Owen 2019; Pandya and Leblang 2017). Increasing attention has been paid to how often-mischaracterized foreign-owned business activity in services (Lakatos and Fukui 2014; Weymouth 2017), including the banking sector (Copelovitch and Singer 2017; Epstein 2014; Goyer and del Real 2014; Grittersová 2014; Iwanicz-Drozowska, Bongini, and Witkowski 2018), challenges the theories built on the back of FDI characterized by fixed-asset investments (Kerner 2014; Kerner and Lawrence 2014).

These bodies of scholarship establish how FDI fits in the heterogeneous international political economy. They also set the stage for scholarship focused on the relationship between FDI and political institutions, processes, and outcomes. Work on the relationship between FDI and regime type continues apace (Arias, Hollyer, and Rosendorff 2018; Bak and Moon 2016; Bastiaens 2016; Frantz 2018; Jensen 2013; Li, Owen, and Mitchell 2018; Moon 2015; Tomashevskiy 2017; Wright and Zhu 2018). Importantly, the relationship of FDI and other aspects of domestic politics are receiving increasing attention, in host states as well as home states. Important scholarship has challenged our priors on, for example: FDI and legislatures, labor power, and subnational politics (Jensen, Malesky, and Weymouth 2014; Owen 2013, 2015; Pandya 2014; Payton and Woo 2014; Pinto 2013; Samford and Gómez 2014); lobbying not only by industry associations but also individual firms (Brooks 2013; S. E. Kim and Urpelainen 2014; Töpfer 2017; Weil 2018); FDI's effects on domestic public opinion (Mansfield and Mutz 2013; Shin and Lee 2019) and the implications of FDI for the fight against corruption (Cieřlik and Goczek 2018; Malesky, Gueorguiev, and Jensen 2015; Pinto and Zhu 2016; Webster and Piesse 2018; Zhu 2017; Zhu and Shi 2019). Other work improves our understanding of how FDI as a specific part of the international political economy influences security, in host and home states as well as at the interstate level (Barry, Chad Clay, and Flynn 2013; Barry 2018; Carter, Wellhausen, and Huth 2019; Ana Carolina Garriga 2016; Mihalache-O'Keef 2018; Vadlamannati, Janz, and Berntsen 2018). Of particular importance is work looking broadly at the impact of FDI on coordination at the interstate level (Crasnic, Kalyanpur, and Newman 2017; S. E. Kim and Urpelainen 2014; Moehlecke 2019; Oatley 2019). Private commercial actors are driving international regulatory agendas and outcomes (Fariás and de Almeida 2014; Farrell and Newman 2015; Kalyanpur and Newman 2019), at times substituting for classic government functions and creating alternative private governance institutions (Baron 2014; Doh et al. 2017; Fort 2015; Malesky and Mosley 2018). In the context of declining developed-country interest in international cooperation, these trends could fundamentally reconfigure international economic relations, in ways that IPE scholars have a comparative advantage in elucidating (Johns, Pelc, and Wellhausen 2019).

This brief review of key themes in research on the political economy of FDI demonstrates that, by isolating FDI among all possible international economic activity, scholars engage with the consequences of a particular political reality in which sovereign states allow foreigners to own and operate businesses in their geographic jurisdictions. While it need not be so, the political choice to accept FDI has been wildly popular in recent decades; scholars examine the ways in which politicians fall over themselves to attract FDI (Jensen et al. 2014; Jensen and Malesky 2018). Taken together, the wellsprings of FDI scholarship in both the "scientific" and "critical" research traditions accord with real-world interest as well as the pressing theoretical puzzles that FDI exemplifies.

THE SHORTCOMINGS OF AGREEMENT ON THE CONCEPT OF FDI

I now beg the reader's pardon in articulating two lurking stereotypes of FDI research in the "scientific" and "critical" traditions. Scholars coming from a "scientific" tradition might default to data-driven analyses of cross-national, time-series datasets of aggregate measures of

FDI, dutifully correlated with political and economic covariates. On the other side, scholars from a “critical” tradition might default to *dependencia*-inspired analyses of the adverse impact of MNCs on welfare outcomes in developing countries. My intention is not to debate the usefulness or accuracy of these stereotypes.⁴ Rather, notice how both stereotypical approaches (perhaps surprisingly) flesh out the original, barebones concept of FDI described above in very similar ways. These two stereotypes agree on the unit of analysis underlying FDI: big, successful, profit-maximizing MNCs, with subsidiaries in a variety of host states, accountable to private shareholders in traditional capital-sending home states. Now, this particular unit of analysis is clearly consequential, to publics, policymakers, and scholars. In the “scientific” tradition, cutting-edge research connecting heterogeneous trade theory (“New New” Trade Theory) to FDI establishes just how important such “superstar” MNCs are in the global economy (I. S. Kim et al. 2019; I. S. Kim, Liao, and Imai 2019). Research in the “critical” tradition has long examined why “superstar” MNCs pose particularly consequential challenges to state power (Abdelal 2015; Sharman 2020; Skovgaard Poulsen 2015).

Nonetheless, new research agendas probe the extent to which the assumptions behind the “superstar MNC” wash away relevant variation in the actors engaging in FDI. With heterogeneity as a starting point, scholars are providing new answers to old puzzles, as well as identifying puzzles otherwise obfuscated by conceptual agreement on FDI—overlooked puzzles that, in my view, should be the target of more of our scarce research resources. By way of illustration, I focus here on scholarship that problematizes the “F” in FDI.

The Home Country of “F”

Is identifying an investor as “F” sufficient to understand outcomes in the international political economy? A long research tradition, in international business and elsewhere, has elaborated on the “liability of foreignness”—the ways in which it is exactly the point that, yes, outcomes differ because the firm is foreign (Alimadadi and Pahlberg 2014; Haftel and Thompson 2018; Newenham-Kahindi and Stevens 2018). A booming, interdisciplinary literature lays out the suite of political risk management strategies with which MNCs can address this shared liability (Arel-Bundock, Peinhardt, and Pond 2019; Betz and Pond 2019; Jensen et al. 2019; Pond 2018).

Nonetheless, scholars are increasingly focusing on how the single category of “F” falls short in explaining many outcomes of practical and theoretical interest. For example, take the phenomenon of members of a diaspora abroad disproportionately investing in their home state. Scholars have grounded such real-world observations in theories about the advantages possible when “F” more closely approximates a domestic actor in the host state (Andrews, Leblang, and Pandya 2018; Dreher, Nunnenkamp, and Vadlamannati 2013; Fossati 2019; Graham 2014, 2019; M. Kim et al. 2015). Are there other ways that MNCs can limit or overcome liabilities by more closely approximating domestic actors? One promising area of IPE scholarship focuses on particular advantages for MNCs when the institutions of their home state match those in the host state, for example, when both home and host share similar levels of corruption (Beazer and Blake 2018; Godinez and Garita 2016).

Where exactly “F” is from can also be consequential—consider for example the political calculations made by Japanese MNCs versus others in China (Vekasi 2017). MNCs with different home states must manage different kinds of bilateral home-host state tensions,

and they have access to different kinds of home-state-tied resources to draw on in case of conflicts, especially diplomatic support (Duanmu 2014; Gertz 2018; Gertz, Jandhyala, and Poulsen 2018; Wellhausen 2014). Yet home country diplomatic support for MNCs varies when they have, let's say, complicated national origins (perhaps a strategically located headquarters in the Cayman Islands despite a different popularly-understood national identity), or when they engage in "round-tripped" FDI, in which the investor incorporates abroad and then invests back in the home state (Fichtner 2016; Moehlecke 2019; Wellhausen 2014). From the host state's point of view, a diversity of MNC national identities shapes the tensions between FDI and sovereignty: if other "F" MNCs do not see the host state's nationalization of an American-origin MNC as a reason to abandon ship, the state can both exercise its sovereignty and play host to (some) FDI (Wellhausen 2014, 2015b).

Research agendas that critically engage with "F" are a particular priority, given the increasing importance of MNCs coming from the Global South (Collins 2013; Feldmann 2019). Scholars are rightly questioning whether the preferences we have attributed to MNCs are in fact specific to the Global North (Feldmann 2019; Huang and Zhu 2016). To push this research agenda, scholars might prioritize descriptive and critical work that documents how and when Global South-origin MNCs appear to be theoretical "outliers." Such work can in turn spur needed systematic reevaluations of our expectations about the preferences of "F" and under what conditions (if ever) they are uniform.

The Owner of "F"

A compact characterization of the MNCs that comprise the standard unit of analysis in FDI research is that they are firms that "organize the production of goods and services in more than one country, involving the transfer of assets or intermediate products within the investing enterprise and without any change in ownership" (Jensen et al. 2012: 1). This definition does not precisely require MNCs to be profit-maximizers, but that is the standard ancillary assumption. This assumption serves us well in understanding, for example, the conditions under which (profit-maximizing) MNCs generate the kinds of economic growth, technological spillovers, employment, diversification, and other benefits that make host states so eager to attract them in the first place (Pandya 2016). Of particular importance in recent years has been the effort by IPE scholars to employ firm-level data when examining FDI, which is the most proximate point at which to link firm decision-making over profit-maximization and political economy outcomes. Scholars are collecting and wrangling this firm-level data in creative ways (Dorsch, McCann, and McGuirk 2014; Earle and Gehlbach 2015; Johns and Wellhausen 2016; S. E. Kim 2018; Malesky, Gueorguiev, and Jensen 2015; Markus 2012; Vadlamannati, Janz, and Berntsen 2018; Weymouth and Broz 2013). Novel data collection efforts are all the more important given the extensive shortcomings of standard time-series cross-sectional data sources (Kerner 2014; Linsi and Muggge 2019).

But what if MNCs are not profit-maximizers? The prominence of state-owned enterprises (SOEs) as actors in the international political economy begs this question. Today, there are many SOEs operating in domestic markets alongside privately owned firms, in developing and also developed countries, in industries like oil and gas but also in a variety of others. This reality contrasts starkly with the context of society-wide state ownership in which Cold War-era scholars considered SOEs. Particularly relevant here is the reality that SOEs also compete

abroad; their investment flows are part of the FDI statistics that we use to motivate and test our hypotheses. How do our expectations born of profit-maximization translate when we engage with the kinds of outcomes state owners seek to maximize? Broadly, what does foreign state-ownership of an MNC mean for the push-and-pull between FDI and host state sovereignty?

As evidenced by a bevy of publications in just the last few years, interest in the political economy of today's SOEs is high. Clearly, the rise of China and Chinese SOEs acting as foreign direct investors has inspired research that bucks the assumption that "F" means private (Cuervo-Cazurra 2017). Scholars examining natural resource politics and the phenomenon of Sovereign Wealth Funds (SWFs) also explicitly theorize around the effects of state ownership (Chwieroth 2014; Wang and Li 2016). Lurking behind this work is the potential trap that a finding interpreted as a result of state-ownership is in fact a finding on Chinese-nationality MNCs, or the oil and gas industry, or something else. Scholarship engaging widely with state ownership is thus an important complement to these research agendas. For example, scholars have linked SOEs participating in the global economy to trends in inequality (Avsar, Karayalcin, and Ulubasoglu 2013); conceptualized SOEs as a means of reinforcing domestic authority (Carney 2015); and considered what changes in state ownership (i.e., privatization or nationalization) mean for outcomes of interest (Bieler and Jordan 2018; Wellhausen 2014). Others start from the firm level and explore the conditions under which state ownership generates different firm-level preferences and strategies (Cannizzaro and Weiner 2018; Cazurra et al. 2014). Impressive new data collection efforts improve our ability to characterize trends among MNC-SOEs as well as evaluate their successes and failures along metrics other than those of privately owned "F" (Babic, Garcia-Bernardo, and Heemskerk 2019).

"F" in the Context of Its Domestic Counterparts

As a last example, selecting on "F" means that IPE scholars working on FDI have prioritized research agendas studying "F" in isolation from its counterpart, domestic firms in the host state. To be fair, there is a compelling theoretical reason that often justifies setting domestic firms' experiences aside: with "F" comes the benefit of a credible threat of exit that is inaccessible to a domestic firm (Blake and Moschieri 2017; Farrell and Newman 2015; Wellhausen 2019). Nonetheless, given the practical importance of domestic firms to policymakers, their political importance to domestic publics, and the plethora of research questions orthogonal to variation in the threat of exit, *ex ante* excluding domestic firms from consideration limits progress (Bauerle Danzman 2020). Host state interest in FDI typically centers on what it can mean for the domestic economy, including existing and potential domestic firms (Bauerle Danzman 2019; S. E. Kim 2018; Klein 2019; Pandya 2016; Skovgaard Poulsen 2015). Consistent scholarly effort goes into measuring the benefits FDI brings (or not) to domestic economic actors (Du, Harrison, and Jefferson 2014; Elliott and Zhou 2015; Merlevede, Schoors, and Spatareanu 2014). Expectations born of such analyses are a key input into host states' decisions as to how and how much to open to foreign capital (Pandya 2013, 2014).

Some recent scholarship takes up the challenge of considering "F" in the context of domestic firms (Albertus and Gay 2019; Callaghan 2015; Hale 2015; Oladi, Beladi, and Gilbert 2014). For example, there is evidence that domestic firms enjoy relative advantages over foreign firms when they can leverage flexible organizational structures, or when the structure

of their industry leaves more opportunities for small competitors alongside MNCs (Johns and Wellhausen 2020; Post 2014). Most prominently, scholars have embraced the domestic alongside “F” in one substantive area, where the relationship is unavoidable: the political economy of international investment law.

As it stands today, the property rights of those engaged in FDI are protected by what has become a *de facto*, spaghetti-bowl international regime codified in thousands of Bilateral Investment Treaties (BITs) and international investment agreements (IIAs) (Bonnitcha, Poulsen, and Waibel 2017; Cho, Kim, and Lee 2016). A considerable amount of work by IPE scholars has gone into figuring out how much this *de facto* international investment law regime increases FDI, a question that in recent years has effectively morphed into whether there are any conditions under which international investment law actually has a demonstrable positive effect on FDI (Bütthe and Milner 2014; Kerner and Lawrence 2014). The most controversial piece of this *de facto* regime is Investor-State Dispute Settlement (ISDS), the somewhat opaque setting in which foreign investors can sue host states for compensation over treaty or contractual violations of their property rights (Hafner-Burton, Steinert-Threlkeld, and Victor 2016; St John 2018; Wellhausen 2016). In fact, the quest that began with identifying the positive impacts of international investment law has further morphed into identifying whether ISDS carries any benefits for host states, particularly if and when they get sued (Blake and Moschieri 2017; Johns, Thrall, and Wellhausen 2019; Manger and Peinhardt 2017; Poulsen and Aisbett 2013; Wellhausen 2019). Normatively disappointing answers to these questions are multiplied by other scholarly findings, such as the high costs developing country host states face as respondents in ISDS arbitration, and the difficulty of throwing off treaty commitments (Franck 2019; Haftel and Thompson 2018; Peinhardt and Wellhausen 2016; Thompson, Broude, and Haftel 2019).

One other characteristic of the *de facto* international investment regime is particularly relevant here: only foreign investors have access to these international legal institutions via which they can win compensation, directly from the host state, outside the domestic court system. Domestic firms do not have access to these protections. The implications of this imbalance, both normatively and in terms of the sustainability of ISDS as it is currently structured, have not gone unnoticed (Meunier and Morin 2017; Nichols 2018; Simmons 2014). In fact, there is evidence that domestic firms prioritize financing from foreign investors that bring the firm not only capital but also access to international investment law (Betz and Pond 2019). I would offer that domestic firms are particularly uninterested in competing in a world in which they face a “liability of domestic-ness.” In choosing research priorities, IPE scholars would do well to remember again that “F” relies at the end of the day on a political decision to allow “F,” and that domestic firms are much more direct players in domestic politics than foreign MNCs.

CONCLUSION

I ask the reader to keep two takeaways in mind. First, we understand the politics of economic integration more thanks to longstanding agreement on FDI as a unique source of constraints on host state sovereignty. It has been important and useful that scholars have overwhelmingly agreed to operationalize the unit of analysis underlying FDI as big, successful,

profit-maximizing MNCs, with subsidiaries in a variety of host states, accountable to private shareholders in traditional capital-sending home states. The recent advances made by the huge body of scholarship born of this unit of analysis, from both “scientific” and “critical” research traditions, should not be overlooked.

Second, reconsidering the concept of FDI and its boundaries allows us an opportunity to uncover new answers to old questions in addition to reevaluating our priorities when it comes to our research agendas. In a world of scarce resources, I would prioritize scholarship probing “F” over scholarship that relies on the default conceptualization of the actors undertaking FDI. The same exercise can and should be extended to “D” and “I” as well. In a world of shareholders focused on quarterly earnings, the longer-term, managerial control conferred by “D” may not be the most politically relevant (Kerner 2014; Logvinenko 2019; Pandya and Leblang 2017). On “I”: it is not obvious that investment is so different from trade in a world of intra-industry and intra-firm flows of goods and services (Baccini, Pinto, and Weymouth 2017; I. S. Kim et al. 2019; Mosley 2010; Peters 2017).

These are research agendas that require classic constructivist skepticism; a “critical” tradition does not mistake the way the world is for the way the world has to be. Political and social choices both in and out of academia have accumulated assumptions in building out the concept of FDI, and challenging those assumptions can be incredibly productive. These are also research agendas that will benefit from creative quantitative data collection efforts and empirical strategies in the “scientific” tradition. In my view, the key issue going forward in the substantive area of FDI is to leverage comparative advantages in each research tradition to re-center work on where longstanding agreement on “F,” “D,” and “I” falls short.

NOTES

1. The international statistical standard is that meaningful managerial control exists when foreign ownership meets or exceeds 10 percent. There has been some discussion around raising the threshold; e.g., in 2004, the IMF Committee on Balance of Payments Statistics recommended moving the threshold to 20 percent, although the IMF has not made this change (BOPCOM-04/31, from the Seventeenth Meeting, Pretoria, 26–29 October 2004). The OECD conforms to the 10 percent threshold for cross-border FDI transactions and positions, but it has put considerable effort into establishing definitions for foreign investments that do not meet that threshold (e.g., “Glossary of Foreign Direct Investment Terms and Definitions,” 4th edition of the OECD Benchmark Definition of Foreign Direct Investment).
2. Secretary-General Antonio Guterres, in his cover letter accompanying the *World Investment Report 2019*. (https://unctad.org/en/PublicationsLibrary/wir2019_en.pdf)
3. Cited literature is selected from recent journals and presses (2013–2019). A more extensive bibliography is available online at the author’s website, with thanks to UT Austin’s Innovations for Peace and Development research lab. This targeted strategy draws the reader to recent advances at the expense of highlighting the shoulders on which these advances sit.
4. Revolutions in quantitative methods, qualitative methods, and integrated mixed-methods research designs are raising the bar on the quality of evidence necessary to substantiate arguments from either starting point. I should also acknowledge lurking defensiveness on my part, given that I have done work that can be collapsed into either stereotype.

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CHAPTER 29

WORK AND WORKERS IN INTERNATIONAL MARKETS

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IN their Introduction to this volume, Pevehouse and Seabrook argue that we can best understand International Political Economy (IPE) as “problem-driven,” as opposed to a field organized around grand theoretical or methodological fights. When it comes to the interdisciplinary literature on labor in the international economy, I concur. At first glance this seems remarkable. After all, the political economy of work and workers has a longstanding association with major “-isms,” specifically offshoots of Marxist thought¹ and Weberian notions of occupation and class. Early work was also noteworthy in its explicitly critical view of “neoliberalism” and “Washington Consensus” policies. But concern with “world historical” theorizing has largely receded and, as I will attempt to show in this essay, a “problem-driven” orientation can help us collate existing scholarship and identify lacunae.

“Globalization” frames the IPE purview on labor questions. Inside this frame, we can arrange the literature on work and workers around four sets of ostensible problems:

1. The nature of (paid) work and its connection with workers’ political orientations
2. The role and resilience of labor unions
3. Labor market standards and regulation
4. Migration

This list is heuristic and there are, of course, connections across all these problem areas.

For example, does in-migration weaken labor unions (Finseraas, Røed, and Schøne 2020)? When do unions in rich, industrialized countries advocate for or against more liberal immigration policies (Peters 2017)? There are also a variety of issues implicating all four, including climate change, development, and technological change. Fortunately, Menz provides an extensive discussion of migration in this volume,² so I will focus on the first three as sufficient to highlight strengths and weaknesses of the current literature. Among the strengths are explicit study of particular policy interventions and expanded interest in the politics and structure of workers’ organizations. Indeed, the IPE literature on work and workers has made significant contributions to both organizational theory and the design and evaluation of public policy.

I disagree with Pevehouse and Seabrook's claim that the current state of affairs is "first best." Identifying a set of organizing "problems" is not the same thing as intellectual coherence or knowledge accumulation. In the IPE literature on work and workers there remain walled gardens. For example, work on voter behavior and policy preferences tends to employ quantitative tools and emerge from the open economy politics (OEP) perspective (Lake 2009) whereas scholars of transnational cooperation among labor groups tend to emerge from a qualitative, industrial sociology or contentious politics traditions. We can certainly do better as a field, especially if we care deeply about specific problems. The literature on labor standards in global supply chains is one area where many of these barriers are lower and a variety of research traditions and approaches converge explicitly and productively.

Nor does a problem-driven approach mean that fundamental normative and conceptual issues have vanished. In the concluding section of this essay I reflect on how decisions around what constitutes appropriate "problems" reflect researchers' normative perspectives in ways familiar to our intellectual ancestors. The IPE literature on work and workers largely reflects concerns with distributional conflict, equity, and justice in the context of dynamic economic relations characterized by private property ownership. But the identification of worthwhile "problems" is largely implicit, making any apparent consensus unstable. It also limits interest in the field, as those who do not view a particular issue as a self-evident "problem" may end up ignoring important scholarship that has broader contributions.

WORK, WORKERS, AND POLITICAL PREFERENCES

By far the largest IPE literature touching on labor issues is the study of political preferences, specifically whether and how someone's position as a "worker" is sufficient to characterize her political orientation toward major international economic issues. This contemporary "factor-based" study of mass political preferences is most closely associated with the OEP perspective. But we are also seeing a re-imagining of an older sociological literature on work experience and occupational identity (Kitschelt and Rhem 2014; Ahlquist and Levi 2013). This revival is likely the confluence of at least two trends: controversy as to the explanatory power of "material interests" and rapid upheaval in the structure of production, the boundaries of the firm, and the nature of employment in both rich and developing countries. We can trace IPE interest in mass political opinion to three connected "problems":

(i) theories of comparative advantage showing that, with the right political exchanges, free trade can be welfare-enhancing for all; (ii) the self-defeating collapse of trade during the Great Depression, especially among democracies; and (iii) normative concerns with democratic representation. Initially, scholars focused on preferences over trade openness, in part because the Heckscher-Ohlin and Ricardo-Viner trade models offered up clear and contrasting predictions about liberalization's distributional consequences that depend how easily workers and capital can move between industries.

(Alt et al. 1996)

The literature has since expanded the ways in which analysts specify workers' material interests. It is now common to differentiate among workers based on "skill"—usually operationalized as formal educational attainment. The awkwardly-named "new new trade

theory” has reinvigorated the study of workers’ connections to specific firms. There is increasing recognition that switching industries or sectors is difficult, even in the allegedly flexible US labor market (McLaren 2017). And IPE scholars have looked at workers’ opinions around all areas of economic relations, including exchange rate policy (Bearce and Tuxhorn 2017), foreign direct investment (FDI) (Scheve and Slaughter 2004; Pandya 2016), migration (e.g., Hainmueller, Hiscox and Margalit 2015) and economic shocks more generally (Margalit 2011; Ahlquist, Copelovitch, and Walter 2020).

All this has left us in an ongoing debate about the relative explanatory power of the “pocketbook” versus “sociotropic” or ideational concerns, in the shadow of demonstrable ignorance and apathy among voters. In the broader political science literature, some go so far as to argue that mass preferences are essentially irrelevant for understanding policy (Achen and Bartels 2016). This debate is far too expansive to review here, but it highlights the fact that first-generation theories and measurement around “work” and material interests have proven insufficient. Survey experiments demonstrate that informational stimuli can evoke different conceptions of “interest” among respondents (Rho and Tomz 2017). On this basis, newer research has looked into specific industries and policy contexts to show how workers acquire credible information about economic policy (Ahlquist, Clayton, and Levi 2014; Kim and Margalit 2017). There is renewed interest in how globalization interacts with the spatial distribution of work and production (Moretti 2012; Autor, Dorn, and Hanson 2016), with political consequences (Colatone and Stanig 2018; Autor, Dorn, Hanson, and Majlesi 2020). The field appears to be searching for richer behavioral models and more nuanced understandings of “interest” while paying greater attention to local context.

In the meantime, the structure of work and employment is changing along with technology. In the developed world, internal labor markets are breaking down and employment is polarizing (Autor 2015; Kalleberg 2011). The boundaries of the firm are shifting, with employees reclassified or re-hired into supplier-like relationships, while performing the same tasks (Weil 2014; Goldschmidt and Schmieder 2017). Some proclaim the rise of the “precariate” (Standing 2011; Thelen 2019). The political economy (and regulation) of “data labor” is just beginning to receive sustained attention (Arrieta Ibarra et al. 2018; Fourcade and Healy 2017).

Theoretical labor economics is reorienting toward “tasks” (Autor 2013; Grossman and Rossi-Hansberg 2008). A “job” is now a (perhaps incomplete) contract that defines the bundle of tasks a worker will do and the terms under which she agrees to do them while an “occupation” is a task that someone believes she will be able to perform for a roughly stable wage for an extended period of time (Ahlquist 2018). By this definition, an occupation is a cognitive construct that may depend on both formal institutions around labor contracting and informal conventions about what constitutes “good” and “valuable” work, echoing the earlier sociology of work and class. Technological changes and the imperatives of global competition and opportunity alter the incentives for firms to unbundle jobs into smaller sets of tasks and adjust their contracting decisions accordingly. Occupations appear to be less stable and returns to training harder to predict.

In an ironic twist, this unbundling of jobs and the regulatory gray zone of new labor contracting in rich countries bears similarity to the contingent and frequently informal employment seen across the developing world (Bacchetta, Ernst, and Bustamante 2009; Lee and Kofman 2012). In China, the work unit was not just the site of (state-controlled)

employment, but also housing, education, etc. As China's political economy has transformed so too have these old relationships, with varied and incomplete replacements (Hurst 2009; Friedman and Lee 2010). There are both conceptual (Milner and Rudra 2015) and empirical (Lee 2016; Pham 2017; Freeman 2009) reasons to believe that globalization—especially integration into international supply chains—will amplify the level of informal employment in much of the developing world.³ Workers in developing countries are already involved in platform-mediated “ghost work” alongside their counterparts in the developed world (Gray and Suri 2019). One important finding from this new literature: households and even individuals frequently work in *both* formal and informal or “gig” employment, so scholars must take a more holistic view of the household's employment portfolio and risks when considering political orientations (Baker and Velasco-Guachalla 2018; Ahlquist, Hamman, and Jones 2017). This will require a more explicit consideration of questions of gender, unpaid labor, and intra-household bargaining that we have seen to date in IPE.⁴

In IPE, these changes in the nature of work and the structure of labor contracting have received relatively limited attention, notwithstanding some initial work on “offshorability” (Owen and Johnston 2017; Rommel and Walter 2018) and supply chains. This is due, in part, to serious data and measurement limitations. Our existing survey and administrative data reflect a mid-twentieth-century understanding of what it means to “go to work” or “have a job.” As a result, we cannot reliably describe the scale and trajectory of things like “gig” and “platform” work (Abraham et al. 2018), much less how households cobble together a living over time. Informal employment is notoriously difficult to detect. Anthropologists and industrial sociologists are doing the hard, descriptive work (e.g., Barley and Kunda 2006; Gray and Suri 2019; Lee 2006; Neff 2012), much of which has yet to filter into IPE.

“The job” is a critical site of political socialization and preference formation, perhaps second only to the immediate family. Given changes in the nature of work and our growing appreciation for the nuances of both globalization and human cognition, I find the inconclusiveness of the IPE literature on labor and political orientations unsurprising. Reinterpreting the cognitive, social, and contractual nature of employment will be key to improving both our data infrastructure and our theoretical claims.

THE PRESENT AND FUTURE OF ORGANIZED LABOR

The relationship between globalization and organized labor is the second class of problems attracting attention in IPE. Interest in unions stems from normative concerns with equality, theoretical debates about the role of unions in the evolution of capitalism, and the historical fact that unions are the most effective organizations yet devised for sustaining worker mobilization around economic concerns.

Can workers build and sustain their bargaining power in a world of trade openness, mobile capital, and increased migration (Schulze-Cleven 2017)? And do unions alter the local impact of international economic forces (Garrett 1998; Hall and Soskice 2001; Ahlquist 2017)? The answer to the first question is contested. The answer to the second is a qualified “yes,” but with anxiety about unions' medium-term prospects.

Globalization → Unions

The incentives for firms to shift production to locations where labor is cheaper and more compliant is a preoccupation in the IPE literature (Silver 2003). Similarly, the long-term, secular decline in union membership in rich countries, especially the US, has produced a large literature (Wallerstein and Western 2000; Hirsch 2008). Some have argued that globalization—whether in the form of increased North-South trade, capital mobility (Slaughter 2007), or migration (Lee 2005; Briggs 2001)—is responsible either due to accelerated deindustrialization or because firms can more credibly threaten to exit in the face of aggressive union demands or industrial action (Wood 1994).

More systematic empirical work has turned up little evidence that directly links globalization to the erosion of union membership. Early work focusing on the period from the mid-1970s to the late 1990s consistently found weak or null relationships between immigration (Burgoon et al. 2010) or trade and unionization (Baldwin 2003; Slaughter 2007; Scruggs and Lange 2002), although there is some evidence that FDI is associated with declining unionization (Slaughter 2007) and increasing worker anxiety in developed economies (Scheve and Slaughter 2004). But this first wave of work lacked clear identification strategies and focused on the “pre-China” era. Ahlquist and Downey (2019) find that the so-called “China shock” (Autor, Dorn, and Hanson 2016) did cause deunionization to accelerate modestly among US manufacturing workers, but also resulted in an increase in unionization outside manufacturing. The net effect is that the Chinese trade shock caused deunionization to slow. Although recent migrants are less likely to join unions (Gorodzeisky and Richards 2013), Finseraas, Røed, and Schøne (2020) find that new immigration into the Norwegian construction sector had a negligible effect on unionization rates among native Norwegians, contrary to some expectations.

In the developing world, Silver (2003) shows that worker unrest has followed the shifting sites of production in the auto and textile industries. Outsourcing may undermine unions in “middle-wage” Latin America (Anner 2011), but unions in Indonesia have made notable policy gains, even if divided and representing a small proportion of workers (Caraway and Ford 2017). Some have noted the apparent uptick in worker protest in China (Gallagher 2014), although others urge skepticism (Lee 2016; Chen 2009) and it appears that worker action in China is taking a novel form, distinct from both traditional unionism and the state-sanctioned All China Federation of Trade Unions (Fu 2018). More generally, the structure and legacy of authoritarian politics helps us to understand the divergent paths of organized labor in the Global South (Caraway, Cook, and Crowley 2015).

Claims that globalization killed the labor movement are incorrect, but that does not mean that globalization has not put pressure on unions and their members. Based on the decline in the labor share of income across a variety of countries (International Monetary Fund 2017, ch. 3), workers’ “structural power” appears to be on the wane.

Understanding these pressures and unions’ responses is an ongoing area of research.

Unions → Globalization

Have unions—and the broader wage bargaining institutions—altered the contours and local impact of globalization? The preponderance of evidence indicates that yes, unions (Farber

et al. 2018; Ahlquist 2017) and institutions (Beramendi and Rueda 2014) matter for politics and, ultimately, distributional outcomes. Unions can affect workers' political opinions about trade (Ahlquist, Clayton, and Levi 2014; Kim and Margalit 2017) as well voting behavior (Feigenbaum, Hertel-Fernandez, and Williamson 2019). The "power resources" approach claims that strong labor unions combined with leftist political parties produced the modern welfare state (Korpi 2006). Meanwhile, the compromise of "embedded liberalism" enabled post-War globalization by respecting the same domestic welfare state (Ruggie 1982). Where unions remained strong, left-leaning governments better compensated globalization's economic losers in the 1980s–'90s (Kwon and Pontusson 2010). More unionized countries (Owen 2015) and industries (Owen 2013) maintain more restrictions on inward FDI and were less affected by the China trade shock (Ahlquist and Downey 2019; Pierce and Schott 2016). And unions in the developed West were often successful in their opposition to immigration (Briggs 2001; Peters 2017).

Unions and domestic institutions interact. The Ghent system of unemployment insurance sustains union membership (Western 1997), including among recent migrants (Gorodzeisky and Richards 2013). The presence of coordinated wage bargaining institutions can induce unions to side with employers around trade policy (Dean 2016) and make unionized countries more export-competitive, while the need to maintain export competitiveness can affect the emergence of coordinated bargaining in the first place (Swenson 1991). More concentrated union membership sustains "strategic capacity" (Ahlquist 2010) and the ability to affect policy, including around FDI (Owen 2015). Iversen and Soskice (2015) argue that electoral institutions mediate whether lower-income workers see their purported interests represented in redistributive welfare policy in a "knowledge economy."

In the developing world, unions—where they exist—are often agents of labor control or concentrated in the formal and public/state-owned sectors. In this context, unions' defense of their members' privileges can aggravate insider/outsider divisions. Protections for labor market insiders may affect the extent to which globalization exacerbates informal employment (Goldberg and Pavcnik 2003). And the structure of the labor movement—and connection with political parties—shapes how unions responded to liberalizing reforms in Latin America (Murillo 2001).

Although domestic institutions and labor organizations managed to shape globalization in the past, there is concern about whether this will continue. Macroeconomic imbalances and heterogeneous export performance within the Eurozone may be exacerbated by weakly coordinated wage bargaining in Southern Europe (Johnston, Hancke, and Pant 2014). Which industrial relations systems have enabled the most egalitarian adaptation to new technology and production processes? How will organized labor and wage bargaining institutions affect domestic responses to climate change (Mildenberger 2020)?

Progress in this area encounters a common challenge in IPE: interdependence. Standard analyses—both statistical and qualitative—looking at globalization's putative effects on domestic outcomes proceed under the assumption that one country's institutional arrangements, union actions, or policy responses are (conditionally) independent of those elsewhere. But spillovers, diffusion, learning, strategic reaction, evolutionary adaptation, and asynchronous shocks are key features of a globalized economy. For example, Acemoglu, Robinson, and Verdier (2012) describe a model in which the innovative but polarized and deunionized American economy in fact subsidizes Sweden's egalitarian one. Whether

this occurs remains to be established, but research designs relying on conventional cross-national regressions are unlikely to either resolve the issue or point the way forward.

One area of IPE research putting interdependence front-and-center is the literature on transnational labor organizations. Early work in this area focused on documenting the existence of various forms of transnational cooperation, in the hope that, by demonstrating the possibility of cooperation, traditional unions might start looking beyond their own borders more aggressively (Gordon and Turner 2000). Subsequent work has examined how existing political relationships between parties and unions can condition whether and how unions look abroad for allies, including exploiting the timing of trade agreement negotiations (Kay 2011; Murillo and Schrank 2005).

An open question is whether and when existing unions and other social movements are allies or rivals. In Southern Europe, for example, protest movements emerging out of the global financial crisis and Eurozone debt crisis often viewed legacy unions as part of the problem (Hyman and Gumbrell-McCormick 2017). Brookes (2019) uses a systematic comparison of both successful and failed transnational labor campaigns to present the most articulated model of transnational worker movements. She highlights the interplay between both domestic and international coordination mechanisms as well as how unions derive power from a variety of sources: position in the global supply chain, existing domestic labor market institutions, and connections with other stakeholders (e.g., customers, shareholders).

The IPE literature on globalization and organized labor is strongly oriented toward discovering whether and how existing unions (and industrial relations systems) can survive and adapt in a changing economic environment. The cross-disciplinary literature has arrived at some basic conclusions, namely that globalization, on its own, has not destroyed organized labor and that unions have shaped the evolution of global capitalism thus far. How unions have been able to shape the local experience with global markets varies in numerous ways and interacts with both domestic institutions and outcomes in other countries. Nevertheless, divisions remain. Scholars looking at domestic institutions and unions maintain a limited dialog with those studying transnational labor campaigns. More problematically, scholars in sociology and parts of industrial relations continue to see “globalization” as a cause of labor’s purported weakness whereas those emerging from other disciplines look to technological changes and worker demand. This may be due to different ideas of what “power” means and how it might be exercised in a global economy. A “problem-oriented” approach is unlikely to clarify the situation.

LABOR MARKET STANDARDS AND REGULATION IN GLOBAL PRODUCTION

For goods ranging from biomedical testing equipment to jetliners to t-shirts, production now takes place through transnational webs of suppliers and subcontractors known as global supply (or value) chains (Gereffi 2014). Motivated in part by a concern that global production will engender a “race-to-the-bottom” in labor and environmental standards, there is a substantial literature on the governance of global supply chains and the consequences for

workers. I do not review that literature here⁵ but I do note that the most extreme fears about a race-to-the-bottom have not come to pass. Workers from Bangladesh to Vietnam have higher incomes and access to better opportunities than in the past, although conditions lag far behind the norm in the developed world and horror stories still abound. Here I highlight several aspects of this literature that make it exemplary for other areas of IPE: explicit focus on interdependence and diffusion; policy relevance; and communication and collaboration among scholars from different disciplinary backgrounds.

Supply chains by their very nature connect workers, finance, managers, and policymakers across the globe, even if they never see or even become aware of one another. The anti-sweatshop and ethical consumption campaigns of the 1990s showed that activists and consumers—often in the developed world—could put pressure on certain global brands to improve labor and environmental standards throughout their supply chains. There is now substantial evidence of positive but incomplete diffusion of labor standards; countries and firms trading with high-standards countries tended to improve their respect for labor rights rather than the other way around (Greenhill, Mosely, and Prakash 2009). FDI seems more likely to improve worker rights than arm's-length subcontracting and trade (Mosley 2010; Mosley and Singer 2015). Subsequent work has identified mechanisms involved, including improved labor relations due to “lean production” requirements (Distelhorst, Hainmueller, and Locke 2016) and improved industry codes of conduct (Distelhorst and Locke 2018). Suppliers in developing countries are more willing to invest in improved labor standards when their customers consider labor issues important (Malesky and Mosley 2018).

Policymaking around labor standards now takes place in the context of “private governance”—industry codes of conduct, multi-stakeholder initiatives, certification efforts, and accreditation schemes (Locke 2013; Dingwerth 2008). These initiatives vary in numerous ways, including governance structure, legal form, duration, and financial commitments (Bartley 2018). Many of these initiatives arise in the wake of major disasters, publicized through global media, implicating international brands, and attracting activist pressure (Berliner, Greenleaf, Lake, Levi, and Noveck 2015). Private governance can be effective at limiting some of the most egregious abuses around working hours and wages, but gains are fragile and uneven (Locke 2013). Private governance initiatives appear to work best in the context of stronger domestic bureaucratic and legal institutions (Vogel 2010). And even when supporting other remediation programs, both firms and local governments appear hostile to independent workers’ organizations and attempts to bargain collectively.

The IPE literature on supply chains and labor standards explicitly focuses on transnational economic and political spillovers in ways that have affected the design and implementation of everything from activist campaigns to private governance initiatives. Much of this work emerged from research teams composed of scholars with different methodological approaches and disciplinary backgrounds working in collaboration with firms, governments, non-governmental organizations (NGOs), and workers’ organizations. The vibrant literature on supply chains and private governance attracts an even more diverse set of participants across numerous general interest journals as well as specialist outlets in industrial relations. This literature is a good example of what Pevehouse and Seabrook’s “problem-driven” approach can accomplish in IPE.

CONCLUSION

I argued that we can view the IPE literature on work and workers as framed by globalization and structured around four interlocking problems. A problem-driven approach has produced knowledge accumulation and policy experimentation and evaluation. I pointed to the literature on governance in global supply chains as exemplary in these regards. We should expect a large wave of research on the resilience of international supply chains and the functioning of labor market institutions under conditions of severe and simultaneous strain brought about by the novel coronavirus pandemic.

The problem-driven approach is a remarkable shift from some of the older “world-historical” theoretical arguments. Nevertheless, many of the same normative concerns with distributional equity as well as a generally critical disposition toward the “neoliberal” approach remain just below the surface. In allowing normative commitments to remain submerged, we have certainly avoided a variety of sterile theoretical debates. But there are also costs. As Burgin (2012) shows in the context of the Mont Pelerin Society, a shared set of loosely articulated normative commitments attracted the funding and produced the research foundational to the later “neoliberal” approach. Without clear normative positions justifying the study of a particular “problem,” work comes across as largely technocratic. It may also fail to attract otherwise interested audiences who do not share or are simply unaware—of the normative assumptions motivating interest in a topic. Any normative framework necessarily has blindspots. It is hard to identify these blindspots without first articulating the framework.

None of this is to say that we should go back to the days when the first 10 pages of any article consisted of theoretical throat clearing. Nor is there any reason to think that there is only one or two normative perspectives at work. But we will never know without developing clear normative positions that can then be referenced, debated, and revised as new problems come in to view and problem-driven research continues. As one example, Gourevitch (2018) recently elaborated a normative view justifying coercion as part of the right to strike. Such work can help frame interest in various approaches to labor rights, private governance initiatives, and labor market institutions.

NOTES

1. Specifically neo- and analytical Marxism, world systems theory, and dependency theory.
2. Also see Mosley and Singer (2015).
3. But see Gallagher et al. (2015) on enforcement of China’s 2008 Labor Law.
4. See Iversen and Rosenbluth (2011) for a notable exception.
5. See Berliner et al. (2015); Mosley (2017).

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CHAPTER 30

MIGRATION AND IPE

A Story of Mutual Neglect?

GEORG MENZ

ALTHOUGH immigration is now an issue-area that is firmly within the purview and on the mental radar screen of international relations scholars, the relationship between immigration and International Political Economy (IPE) is one of mutual benign neglect. In time, this may well change. This chapter charts the relation between the two fields, explores reasons for which the dialogue has been limited, and proposes pathways forward. In the third and final section, I also briefly comment on how the different schools of IPE, especially those identified with being more “critical” and “problem-solving” respectively, can benefit from addressing and studying migration.

IR AND IMMIGRATION: A HISTORICALLY CONDITIONED STORY OF MUTUAL NEGLECT

In this first section, I will explore some of the main trends in recent migration literature and offer possible reasons for the relative neglect of International Relations (IR) as a scholarly tradition or even, somewhat curiously, the international arena as a major relevant factor. Traditionally, the scholarly literature on immigration in political science has been somewhat insular. To an extent this might be an artefact of academic “silo building,” a dearth of meaningful transdisciplinary dialogue and the career incentives at the heart of contemporary academia. Specializing early on in an issue-area and ploughing that field thoroughly pays much more handsome and easy rewards when compared to those available to the ambitious few endeavoring to keep their fingers in several pies. Though there is much rhetorical emphasis on “interdisciplinarity,” in truth, work that takes up this gauntlet is hard to find. But the single most important argument I will put forward is related to the historical circumstances, which strongly conditioned the sub-discipline in the English-speaking world and beyond. After 1945, IR became very strongly conditioned by American prerogatives, research questions, and paradigms of making sense of international politics. This theme, expounded in greater detail by the editors in their introductory chapter, resulted

in a relegation of migration studies to the realm of “comparative politics,” rather than either IR or IPE.

The geopolitical situation prior to 1989 diverted attention away from migration. Before the end of the Cold War, immigration was seen as a somewhat minor empirical phenomenon that paled in significance when compared to the confrontation between the two nuclear superpowers and the repercussions of an increasingly deregulated global economy. Migration policy was designed and executed strictly by national governments and the few international organizations that dealt with the matter, for instance the United Nations High Commissioner for Refugees (UNHCR) or the International Organization for Migration (IOM) did not tread on what was widely regarded as a core domain for state sovereignty. In Europe, some countries relied on their postcolonial relationships in recruiting labor migrants, others concluded bilateral treaties to solicit ostensibly temporary labor migration. The international arena mattered very little; migration was a matter of bilateral relations. In the US, the 1965 Immigration and Naturalization Act (also referred to as Hart and Cellar Act) liberalized US immigration policy considerably and drastically broadened its geographic remit to countries outside of Europe. This measure was pushed through by a coalition of left-leaning Democrats, but ultimately encountered little resistance from the political right. There was virtually no impact by international drivers. This is not to deny that foreign policy considerations might play a role in migration policy design, albeit somewhat indirectly. Western governments considered migration policy as a tool to maintain favorable relations with sources countries. This was an explicit factor in British migration policy, positively discriminating in favor of Commonwealth countries in the design of citizenship and thus migration policy (Hansen 2000). In many ways, the absence of international (as opposed to bilateral) factors in migration policy design and implementation might thus account for the mutual ignorance of IR and IPE on the one hand and migration studies on the other. Whatever the reasons, for the longest time much of the secondary literature on immigration was largely descriptive, historicist in focus, and typically consisted of country-level studies (Green 2004; Cornelius et al. 2004; Hammar 2011; Geddes and Scholten 2016). Very few scholars engaged in comparative work, focusing on one or at most two countries as case studies, and neglecting the “international” as a causal factor.

There is also an institutionalist explanation. In Europe, migration policy design was carried out “behind gilded doors” (Guiraudon 1997). Historically, decision-making was elitist, technocratic, and deliberately sheltered from interest groups with only minimal involvement of parliament (Freeman 2006). The same cannot be said of the US, although in practice US immigration policy in practice is largely conditioned by the implementation at the grassroots level, rather than partisan politics at the national level (Zolberg 2008). With this mind, it is easier to see how scholars have long regarded immigration as being the purview of comparative politics and public policy, but not really the stuff of international politics, despite its superficially international character.

Due to the tendency of migration studies to rely on thick and historical description, rather than attempt to “mainstream” the field, Immigration was widely regarded as somewhat of a niche field. It was not really considered a field of interest to scholars of international politics—and vice versa. While IR concerned itself with the grand contours of international politics and conflict and IPE began to make sense of the post-Bretton Woods global financial order, immigration scholars contended themselves with much narrower studies with a distinct national flavor. In conventional US terms, their field, the study of domestic actors,

especially political parties, but also executive branch institutions, rested firmly within the remit of comparative politics, not IR. Meanwhile, the empirical reality misled some. In 1990, US migration scholar Anthony Messina (1990) was comfortable in predicting that liberal (labor) immigration policies were unlikely to ever be revisited again in Europe, seeing that the political Left had abandoned its pro-migration stances, the political Right had embraced restrictionism and explicitly anti-immigrant parties were attracting significant electoral support in a number of erstwhile immigrant-receiving countries. Not only did this prove wrong, it also neglected family reunion and asylum, two legal migration channels that were rapidly increasing in significance. West Germany, a country with Europe's most liberal asylum regulations, had attracted about 20,000 annual applications in the late 1970s. By the tail end of the 1980s, more than 100,000 asylum applications were lodged every year. Other European countries dovetailed this trend in relative terms, though not in absolute numbers. Meanwhile, in the US, the Reagan administration's 1986 Immigration Reform and Control Act promised to clamp down on illegal immigration through tougher border enforcement and workplace spot checks, while offering a regularization of status to millions of illegal immigrants. While with the benefit of hindsight it might seem naïve to assume that such two-pronged approach would curtail illegal immigration and address the problems related to immigration in general and sizable illegal immigrant populations in particular, this is precisely what was widely assumed at the time. Immigration seemed to be dealt with and its politics appeared more national, than international.

Of course, immigration was very far being "sorted." Though it might sound like a hackneyed cliché, the end of the Cold War really did prove pivotal. To be more precise, the messy ramifications of Cold War politics proved important for migration politics and made it very clear that the study of migration would have to take international factors much more seriously than it had before. In Europe, the violent disintegration of the artifice that was Yugoslavia generated hundreds of thousands of emigrants, quickly overwhelming the capacity of west European governments to accommodate all of these refugees. Even the most generous European asylum regulations, those of West Germany and Sweden, were modified in consequence. The fall of the Iron Curtain proved momentous, not only because it enabled central and eastern Europeans to emigrate to western Europe in sizable numbers. The division of Europe, as it turned out, has also acted as a gigantic road block that kept would-be immigrants from Asia, the Middle East, and, somewhat counterintuitively, Africa, out of Europe. With the Iron Curtain consigned to history books and the two superpowers' various "substitute conflicts" having spawned political instability and lasting civil conflicts, Europe's periphery looked strikingly unstable: whether in the Persian Gulf, the Great Lakes region of Africa, in Israel, or in Afghanistan. Meanwhile, the Reagan government's 1986 legislation was implemented very unevenly, pointing to conflicts in the Republican Party between social conservatives who sought to clamp down on illegal immigrants and the business-friendly wing that favored the relaxed stance on enforcement that ultimately carried the day. Yet this relaxed stance on immigration regulation enforcement both at the border and internally came at a high price: the population of Mexicans and other central Americans (most of it illegal) in the US skyrocketed. The 1986 legislation also entailed a major amnesty for illegal immigrants. Given the high numbers of immigrants and the emerging second generation in Europe, the 1980s and 1990s witnessed heated debates about the modalities of integration and the merits of so-called "multiculturalism" on both sides of the Atlantic (Joppke 2017).

Immigration was clearly becoming “hot” in the 1990s. It was also increasingly difficult to ignore altogether. This had two positive consequences for the scholarly literature. On the one hand, it spelt an end to the “stamps, coins, and flags” approach that had characterized the political science literature on immigration for so long. On the other, with IR scholars less concentrated on conventional defined terms of conflict, interest began to focus on non-military forms of threat and sources of insecurity. Conflict scholars hypothesized that immigration could be strategically deployed to destabilize foreign governments (Weiner 1985, 1992; more recently: Greenhill 2016). In this sense, it was possible to use mass emigration as a weapon for strategic purposes. At the same time, emigration and internal migration might also be side-effects of internal conflicts generated by environmental degradation. Civil war factions might strategically use access to natural resources as a strategic weapon to force certain populations off the land (Homer-Dixon 1999). Clearly, civil wars in the 1990s, whether in Yugoslavia or Ruanda, highlighted the forced mass migration of certain populations (“ethnic cleansing”) with a view to creating a *fait accompli* after the war. Little by little, IR scholars came to take note of migration. The flipside to the Weiner argument was put forward eventually. The Copenhagen School (Buzan 1991) submitted a constructivist argument, reminding scholars that so-called threats can also be mere rhetorical artefacts strategically deployed for political purposes. Some analysts have argued that by conflating immigration with a security threat, officials both at the national level and the European Union (EU) level have paved the way for more restrictive and thus “securitized” overall policy approach (Huysmans 2006; skeptical: Boswell 2007), a claim that sits uneasily with the empirical reality of record high levels of immigration to Europe.

In truth, the EU dimension would prove crucial for European national immigration policies, further leading to migration scholars taking the international more seriously. The freedom of labor mobility had been enshrined in the 1957 Treaty of Rome and became effective in 1970. But the 1993 Treaty on European Union (TEU) also created union citizenship and hence the right for citizens of one EU country to reside permanently in another irrespective of employment status. Later in the decade, the EU also commenced its lengthy and contested path toward creating its very own labor immigration, asylum, and family reunion policies (Luedtke 2011; Geddes et al. 2020). A lot of literature has since focused on the top-down impact of European integration on migration, as it permits not only border crossings by EU citizens, but also shapes immigration policies vis-à-vis the rest of the world (Menz 2009). Acutely aware just how unpopular mass immigration was, the US government carefully avoided similar concessions regarding immigration rights in the negotiations leading to the North American Free Trade Area (NAFTA) in 1993. In fact, the Clinton government claimed that NAFTA would promote economic growth in Mexico and thus reduce emigration pressures. US immigration policy thus continued to be conditioned by domestic factors, namely partisan politics amidst emerging regional diversification, as immigration was strongly focused on only seven out of 50 states. From the 1970s onward, ethnic politics and the role of ethnic lobbying groups also became an influential factor (Freeman 1995). By contrast, foreign policy considerations appeared to matter little.

Another factor sparking renewed interest in immigration is the rise of Islamic fundamentalism and Muslim terrorism. The 9/11 attacks on New York and Washington, though not carried out by immigrants to the US in the conventional sense of the term, re-ignited interest in the field by IR scholars because the response to these atrocious assaults there and elsewhere in Europe and beyond appeared to question cheerful predictions about the end of

history (Fukuyama 2006) and the melting away of national borders (Greider 1998). In actual fact, the state re-asserted itself with a vengeance. The increased budgets of border enforcement agencies on both sides of the Atlantic, the rise in state surveillance activity, targeting both immigrants and citizens (Rudolph 2003), and the concerns over poorly integrated Muslim immigrants all seemed to point toward a “Gourevitchian” (1978) picture, in which the “second image reversed” led to domestic politics being shaken up thoroughly by international events. There appeared to be very clear international sources of domestic turmoil. To those on the Left, the increased attention to border security and enforcement seemed to bear out predictions of a securitization of immigration (Bigo 2001) in the wake of the Muslim terrorist attacks in Madrid, London, and beyond. It needs to be said that the impact of 9/11 on immigration policy was actually very minor. Its impact on politics seems similarly overstated. There was little attempt to curtail immigration, on the contrary, the early 2000s witnessed liberalization of migration policies throughout Europe (Menz 2011), involving mainly Muslim immigrants. Similarly, the ostensibly conservative George W. Bush administration in the US was on the verge of a breakthrough in negotiating a guest worker program with Mexico and proposed several liberal immigration policy packages, without, however, securing congressional approval for any of them (Rosenblum 2011). If Muslim extremism and terrorism thus helped attract newly found interest in immigration from security studies scholars, the actual fall-out in terms of policy output was remarkably limited (Lahav and Guiraudon 2006).

IPE AND IMMIGRATION: EXPLORING THE REASONS BEHIND MUTUAL NEGLECT

Thus far, we have explored the waning and more recently waxing of interest in migration from an IR perspective. Similarly, relatively little IPE scholarship has focused on the issue-area. In this second section I will explore reasons for this lacuna. Obviously, a lot of what has been said thus far, also applies to IPE: the Cold War era might have obscured the view of immigration as being a major international issue of major significance. In many ways, IPE is an intellectual child of the 1970s (Cohen 2008; see also Introduction to this volume), which not coincidentally witnessed relatively low levels of immigration in Europe after the recession of 1973 led governments to end active labor migration recruitment. In other words, at the point of inception of IPE there seemed little reason to study a phenomenon that was receding empirically.

The relative dearth of IPE scholarship seems perplexing because migration is a clear exception to the trend of liberalization that has affected capital flows, international trade, and, to a much lesser extent, international service provision. In many ways, migration is the last frontier of globalization. Yet in the context of the fiery debates surrounding globalization in the 1990s (e.g., Berger and Dore 1996), migration commonly did not feature or was barely mentioned as an afterthought. It is plain to see why this would be the case. While successive governments throughout the OECD had liberalized international trade under the auspices of the General Agreement on Trade and Tariffs (GATT) and controls on capital flows had been incrementally reduced from the late 1970s onward, it would be very difficult to argue

that migration control had somehow slipped out of the hands of the state. Even a casual traveler passing through Sidney or New York's JFK Airport will be quickly disavowed of such notions. In fact, if one explores the elaboration of migration control through the history of the passport (Torpey 2018), one is struck by the fact that unlike the late nineteenth century, when immigration control was often enforced only selectively and arbitrarily, the state of affairs in the late twentieth and early twenty-first centuries looked very different indeed (Schain 2019).

Liberal states find themselves in a conundrum (Joppke 1998; Hollifield 2004). On one level, they seek to constrain immigration in order to sustain control and sovereignty. Immigration is also highly politicized and largely perceived as negative in public opinion polls. Clearly, an excessively liberal policy is difficult to pursue. However, courts have liberalized and extended family reunion rights, making it difficult for governments to curtail this numerically important legal channel of immigration. Also, to the extent that governments still regard themselves as engaging in macroeconomic management, procuring an adequate labor supply might be regarded as being part of the government's responsibility. This, too, however, is a contested suggestion. Not everyone agrees that governments should not only permit but encourage labor migration to feed into a domestic labor supply pool. Critics of what Hollifield (2004) calls the "migration state" approach point to domestic unemployment, negative effects on wage levels at the bottom end of the labor market through substitution effects, and brain drain effects for the source countries (Borjas 1999). Immigration regulation is very far from a technocratic endeavor to finetune the labor supply pool.

There is certainly reason to think that domestic economic actors have played a central role in reviving labor immigration and proving Messina wrong. In the 1970s, a number of neo-Marxist scholars emphasized the role of employer organizations in pushing for the liberalization of labor migration to enlarge the reserve army of laborers, to use Marxist terminology (Castles and Kosack 1973; Cohen 1987). The 2000 revival of liberalized labor migration similarly has to be understood by taking the role of employer organizations seriously (Menz 2009; Menz and Caviedes 2010). But the passive and obsequious role of trade unions is equally noteworthy: having declined precipitously throughout the OECD world (with the exceptions of strongholds in Belgium and Scandinavia) through deindustrialization, privatization, the rise of the service sector and the rise of more diverse and atypical forms of employment contracts, unions largely acquiesced to liberalizing labor migration policies (Watts 2002, Haus 2009). This is remarkable because it not only reversed their traditional stance on the issue (Marino et al. 2017), but also was far from inevitable. It is a remarkable case of capitulation in the face of adversity, as unions considered limited and legal immigration preferable to unlimited and illegal immigration.

In studying migration policies, the varieties of capitalism (VoC) lens proved helpful (Hall and Soskice 2001). Business does not necessarily simply want more bodies, employer organizations carefully calibrate their demands according to the predominant production strategy in their respective countries. Liberal market economy (LME) employers will call for both skilled and unskilled immigrants, because both can be accommodated in the labor market. But coordinated market economies (CME) struggle to employ low skilled workers, which is why employers have little interest in them. The various mixed models found in southern and central Europe will generate uneasy amalgams, with southern Europe offering a substantial black labor market that does not exist in this form elsewhere in Europe. Marrying insights from Comparative Political Economy (CPE) to migration studies thus

generated a number of fruitful insights in terms of accounting for the exact nature of employer demands (Menz 2009, 2011). Employers are generally studied with great interest by CPE scholars. On this occasion, this interest pays handy dividends: their role truly is crucial in understanding migration policy design in Europe.

However much improved the dialogue between immigration and IR is, IPE continues to focus more on issue-areas linked to the global architecture of finance, trade, and services. Immigration clearly has financial and economic consequences, but it might appear to be somewhat unique or even *sui generis*. After all, more often than not, immigration is permanent and thus alters the ethnic, political, and social make-up of the receiving country in a way in which importing goods and services from abroad does not. It is thus tempting, but ultimately not quite accurate to think of immigration as being entirely different from other forms of international commerce. Consider that while NAFTA does not entail the freedom of labor mobility, other international trade agreements do, such as ECOWAS in West and Northern Africa and ASEAN in East Asia (Adepoju et al. 2010). The politics of international remittances and the political economy of brain drain are potentially very fruitful roads of inquiry from an IPE perspective. Half a century after the end of colonialism, governments still conduct immigration policy with an eye to foreign policy ramifications, as became evident in the British decision to open its labor market to the new entrants to the EU in 2004. World Trade Organization (WTO) negotiations also entailed focus on immigration of the highly skilled, although the political backlash against this element meant that little with bite has been created. Even so, there is certainly room for more IPE analyses of migration processes and the politics of access.

It is worth noting that migration poses somewhat of a normative conundrum to many on the political Left, which might go some ways toward explaining the relative reluctance of IPE scholars to approach it. It is both plain to see and easier to acknowledge politically that the globalization of trade, finance, and services will create winners and losers. But sadly, self-imposed constraints in the form of political correctness, group think, and self-imposed censorship make it very hard for those on the Left to acknowledge candidly that immigration similarly is far from a universal force for the betterment of humanity. Immigration leads to lower wages at the bottom end of the labor market, social dislocation, anomie, political, ethnic, and religious conflicts and messy and quite often fairly nasty “identity” politics.

THE WAY FORWARD: MIGRATION THROUGH AN IPE LENS

The politics of migration are inherently and deeply political. Some migration scholars emphasize the importance of salience (Givens and Luedtke 2004), but it seems hard to imagine that migration will readily and easily return to being the premise of technocratic governance decisions by ministries of the interior. Who gets what, when, and how, are questions that pertain directly to migration. In cruder terms, migration politics is rife with conflict over distribution. Organized business clamors for cheap labor, ethnic special interest groups call for a relaxation of limits pertaining to the fellow ethnics they purport to represent, so-called humanitarian non-governmental organizations (NGOs), including those with ties to

churches, are often motivated by the substantial spoils, grants, jobs, and political influence that the “resettlement” of refugees affords them. Descending to the granular level of politics, local- and state-level politics is similarly driven by politicians pandering to, manipulating, and exploiting ethnic divisions (Money 2003). At the same time, global governance, ineffective and flawed as it may be, will in time come to influence immigration politics and policy as well. The United Nations Humanitarian Committee on Refugees (UNHCR) ranks as the most prominent institutions among them.

In time, scholars of IR and IPE will take more note of migration as its economic effects will become more evident and as its prominence in global politics will continue to increase. Let us focus on the first aspect first. As intra-First World migration has slowed to a trickle, current and future immigration will likely mainly comprise Third World emigration. If First World countries succeed in limiting immigration to predominantly highly skilled individuals, the politically highly loaded question of brain drain will re-emerge with a vengeance (Collier 2013). There will be more need to examine to what extent remittances might counteract some of the negative repercussions of emigration and who benefits how from the flow of remittances (de Haas 2005). Remittances already easily dwarf the total size of foreign aid and have become a major source of income for certain sending countries. At the same time, closer attention will need to be paid to the economic ramifications of immigration in the receiving countries. Most of the voluminous literature on xenophobic anti-immigration parties in Europe and the US has focused on bigotry and prejudice (Mudde 2013), but rare are the studies that highlight the economic sources of discontent over immigration (Kitschelt 1997), as voters fearing or experiencing socioeconomic decline focus on immigration as a highly visible corrosive element of socioeconomic change. It is here where future scholarship can commence. Similarly, more scholarly attention could be paid to the economic—as opposed to nakedly political—sources of partisan politics in immigration. Received wisdom held that centre-left parties supported immigration and immigrants, but the last decade has seen a considerable sea change amongst centre-right parties in the OECD, though this might prove to be ephemeral in light of the current migration crisis affecting Europe.

At the international level, migration is likely to be one of the twenty-first century’s predominant policy problems. “Globalization” in the broadest meaning of the term is relevant here in that it has disseminated the recent rapid advances in data processing, telecommunications, instant real-time data exchange, and cheaper travel. All of this encourages emigration from politically and socioeconomically unstable countries, of which there continues to be no shortage. Tough normative decisions will have to be made regarding offering localized protection and the benefits of military intervention for humanitarian reasons. Somewhat counterintuitively, improving economic fortunes for middle-income countries will generate more, not less emigration, as upwardly mobile middle-class citizens will endeavor to pursue more promising economic opportunities elsewhere (de Haas 2007). At the same time, total fertility rates in a number of sub-Saharan African countries show no signs of abetting and are well above five children per woman. With a sizable number of African countries thus experiencing exponential population increases, there is substantial potential for major immigration to Europe and beyond, unless countervailing measures are being taken. The impact of ecological degradation is another as yet slightly under-researched factor (McLeman 2014) that may affect emigration from affected regions in south Asia, the Pacific islands, and northern and sub-Saharan Africa. With true international governance of migration being constantly hampered by starkly divergent national interests, it is also far from certain

whether a more robust and durable international “regime” (Boucher and Gest 2018) on immigration is likely to emerge. In fact, it is not inconceivable that individual governments might take the opposite route and extract themselves from the obligation that existing international law imposes, for example the foundation treaties of contemporary asylum law, the 1948 Geneva Convention and the 1961 New York Protocol.

Both from the British and the American IPE tradition, immigration should make for an interesting field of inquiry. As immigration is linked to the international, yet defies easy generalizations about any melting away of state sovereignty, it can serve as an interesting “critical case” from a methodological point of view. It could also be fruitful to explore in more detail why free movement of labor, even where it was part of free trade agreement, as in the case of the EU, ECOWAS, and ASEAN had either not been implemented at all, only unevenly, or at least has spawned major political conflict. Similarly, attempts at international regime building and global governance have proceeded only very haltingly. Immigration creates both winners and losers domestically and touches upon core matters of state sovereignty. It can also be dealt with as a corporate strategy as a substitute for the outsourcing of production or to counteract the liberalization of trade (Peters 2017). It makes for a fascinating area of study.

The study of migration for the longest time attracted interest primarily amongst a small group of specialists. The end of the Cold War had led to a greater diversity of topics considered by IR and IPE scholars alike and this certainly applies to this issue-area. “When it bleeds, it leads” might be a cynical piece of advice for aspiring journalists. Yet it captures a truth applicable also to the intellectual history of migration studies: major international events and crises, such as the short-term effects of the fall of the Iron Curtain and the aftermath of the 9/11 Muslim terrorist attacks, will generate renewed interest in the field and invite IR and IPE scholars into the realm. The electoral fortunes of anti-immigration parties have attracted significant scholarly interest and will continue to do so. Much of the historical lack of interest can be ascribed to both the idiosyncrasies of contemporary academia and the preoccupation with issue-areas that seemed more pressing during the Cold War era. I have outlined a number of promising areas for future cross-over research endeavors. It is conceivable that in the future there might be a stronger emphasis on quantitative or mixed-methods approaches than in the past, as the veracity and reliability of migration data improves (cf. Boucher and Gest 2018). Even so, national idiosyncrasies in data collection will continue to impede cross-national quantitatively based comparisons. By contrast, major conflicts over methodology and approaches would appear unlikely to emerge. It seems unlikely that migration studies will remain a sheltered niche within political science and it does not appear to be desirable, either. Immigration might be considered a somewhat unconventional topic of study for IR and IPE scholars, but there are a number of important angles that would amply reward scholarly inquiry. Considering the state of global politics, it also seems likely that immigration will continue to be a pressing and major policy issue, unlikely to recede in importance in the foreseeable future.

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CHAPTER 31

FOREIGN AID

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FOREIGN aid is a diverse topic of study in International Political Economy (IPE), encompassing work on donor allocation decisions, the impact of aid on political and economic outcomes in developing countries, and differences across donors and recipients operating in the foreign aid landscape. Scholars have focused on the political and economic importance of aid as both an outcome and an input, as a dependent and as an independent variable. Within each of these topics are numerous subtopics, methodological approaches, and theoretical outlooks.

While squarely within the field of IPE, scholars studying foreign aid regularly intersect with those studying international institutions, international security, foreign policy, development economics, climate change, and global health. Although there were important early exceptions (e.g., Baldwin 1985), for the most part foreign aid has been a “mainstream” topic in political science IPE for less than two decades; before that, most work came from economists in academia, think tanks, and global financial institutions. This new engagement has produced a large number of important insights in a short period of time, but the relatively young nature of the field of study means that gaps remain and several avenues are open for fruitful research.

The editors argue in the Introduction to this volume that silos within an issue-area at times represent a “first best” solution, allowing parallel tracks of research to evolve without devolving into tedious debates. Such tracks have developed in the foreign aid literature, producing an abundance of research relatively quickly. This disjointed approach has limited learning across silos and has at times failed to avoid heated debates within and across research agendas. Whether the benefits outweigh the somewhat significant costs depends on the next generation of studies. If future research seeks to learn across tracks, bridge the gaps, and provide synthesis, then the benefit of the productivity boost to date may validate the editors’ claim.

This chapter will review multiple strands of foreign aid literature while highlighting three key areas for future research: integrating studies of allocation and effectiveness; bridging the gap between studies of micro-level project impacts and aggregate effects on development; and improving understanding of the optimal design of aid architecture to address ongoing global issues such as climate change, food security, and enduring poverty.

The first of these issues, separate evolution of studies on allocation and effectiveness, has played to the strengths of political scientists and economists, respectively (although with

some on both sides crossing the aisle). Fully harnessing these insights will require more studies linking across these strands of research. Studies of effectiveness will benefit from better understanding and incorporating the impact of shifting donor intentions and how these influence allocation decisions and, ultimately, aid impacts. Studies of donor intent will do well to incorporate insights from the effectiveness literature to better understand whether donor allocation decisions are guided by outcomes in recipients.

The second gap discussed is the division between scholars studying micro-level and macro-level effects of aid. The examination of economic impacts has been contentious and draws attention to a key missing piece in our understanding of aid impacts. Scholars of micro-level results often focus on measuring the impact of individual aid projects or programs; among this group the use of randomized controlled trials (RCTs) has become a frequent tool of choice. While RCTs provide evidence regarding the impact of individual aid-funded projects, they cannot on their own determine whether project success improves overall development. Evaluation of an individual project also cannot reveal whether the opportunity cost of funding the project includes not funding other projects with potentially larger development impact. Evaluating macro impacts of aid has been difficult, with little agreement on appropriate measures, unclear theory regarding the correct timing for assessing outcomes, and the impossibility of controlling for all potential omitted variables that could affect both aid and development. Our understanding of aid effectiveness will advance significantly as researchers find better ways to harness the information from project-level evaluations to produce assessments of aid's impact at a societal level.

Studies of the impacts of aid on political outcomes in recipients do not face the sometimes heated micro-macro divide observed when examining economic impacts. This may reflect the relative nascency of subnational studies on the political effects of aid. There are points of disagreement between scholars studying country-level outcomes as well as significant gaps in what we know about subnational political impacts. Moving forward, increased understanding of the within-country effect of aid on institutions and political outcomes at the local and national level will be an important line of research.

A third gap apparent when reviewing the literature relates to the overall architecture of the foreign aid landscape and its suitability for addressing current development-related global issues. Many foreign aid institutions are decades old, tracing their origins to the Cold War when the geopolitical landscape, donor preferences, recipient needs, and issues in need of global collective action differed considerably from the current situation. This is true both for domestic aid agencies within donors and transnational development organizations. The tendency for institutions to change slowly suggests that these pre-existing structures may be suboptimal for confronting the array of issues that donors currently wish to finance with aid. Scholarly work on bilateral and multilateral donor characteristics has increased our understanding of the current landscape of aid architecture and future scholarship can push toward better knowledge of optimal institutional design for development-linked problems facing the global community.

Despite the disagreements and remaining gaps, significant advances have been made in our understanding of the motivations for and impacts of foreign aid, and in our knowledge of the differing actors in the aid landscape. I will draw out some of the key findings from the literature, attempting to synthesize them to point the way forward for breaking down divides and filling remaining gaps.

MOTIVATIONS FOR GIVING AID

The systematic use of “official development assistance” as a foreign policy tool by industrialized states has its origins in the years following the Second World War. While earlier instances have been recorded, the modern set of norms and institutions for aid were pushed forward by the Truman administration in the US and the formation of post-Second World War economic and development organizations. Early assessments by economists such as Carl McGuire (1952) and Milton Friedman (1958) noted the role of foreign aid as a tool in geostrategic Cold War strategy. David Baldwin (1985) highlighted the importance of aid as a form of “economic statecraft,” a policy tool to convince other governments to take actions favorable to the donor. Empirical studies during and in the decade following the Cold War often compared the relative importance of “donor interest” and “recipient need” in determining aid allocation, with the almost universal conclusion that concerns for recipient need were secondary, when they were present at all (e.g., McKinlay and Little 1977, 1978; Maizels and Nissanke 1984; Schraeder, Hook, and Taylor 1998; Alesina and Dollar 2000; for an alternate view see Lumsdaine 1993).

Prominent voices in political science were largely absent from early empirical studies of donor motivation and aid allocation decisions. Given the inherently political nature of these decisions—both domestically and in strategic foreign policy—this was unfortunate. The decades following the Cold War have seen an increase in political scientists contributing to the literature on aid and a corresponding increase in richness of our understanding of political motivations. Bruce Bueno de Mesquita and Alastair Smith (2007, 2009, 2016) have noted the transactional nature of foreign aid, with aid being used as a lever to extract policy concessions from recipient country governments. Scholars have demonstrated that geopolitical considerations drive donors to condition aid on votes in the UN General Assembly (Carter and Stone 2015; Dreher, Nunnenkamp, and Thiele 2008) and to increase aid to countries that hold rotating seats on the UN Security Council (Alexander and Rooney 2019; Dreher, Sturm, and Vreeland 2009; Kuziemko and Werker 2006; Vreeland and Dreher 2014).

Important insight has come from the realization that critical historical junctures influenced donor motivations for aid giving. Scholars note changes in foreign aid when the Cold War ended, including declines in domestic political support for aid (Lancaster 2007; Radelet 2003) and accompanying decreases in aid quantity (Berthélemy 2006; Boschini and Olofsgård 2007). They document the decline in aid to authoritarian regimes (Brautigam and Knack 2004) and shift in focus of aid to economic and humanitarian priorities in the 1990s (Woods 2005; Bearce and Tirone 2010). Maurits Van Der Veen (2011) shows that small and medium European donors, like their larger counterparts, have variation in motivations and policy choices with respect to aid allocation across both time and donor.

Following the attacks of September 11, 2001, experts considered the possibility that development would lose some of its new-found importance as a motivation for allocating aid (Buzan 2006). Carol Lancaster (2008) documents the opposite, arguing that the war on terror led the Bush administration in the US to increase its emphasis on development, a point further supported by Desha Girod, Stephen Krasner, and Kathryn Stoner-Weiss (2009). Studies by Robert Fleck and Christopher Kilby (2010) as well as Todd Moss, David Roodman, and Scott Standley (2005) find that foreign aid increased after 2001. Paul Clist

(2011) shows that donors became more sensitive to poverty when allocating aid in the post-2001 period. Sarah Bermeo (2017, 2018) develops the concept of targeted development, demonstrating that in the post-2001 period donors allocate aid consistent with increased development intent but target aid where they expect development to be most beneficial to themselves.

These shifts in donor motivations gain further support from recent literature examining the link between aid and governance in recipient countries. In a pathbreaking study, Alberto Alesina and Beatrice Weder (2002) find evidence that during the Cold War donors did not give less aid to more corrupt recipients, arguing that this was consistent with donors placing low importance on the development impact of aid. The link between governance and aid allocation gained significant attention from economists and political scientists for several years, although an unfortunate side effect was arguments over how best to measure “governance” and “corruption.”¹ Building on these important early studies and taking advantage of the increased availability of more finely-grained data, the next generation of work finds substantial evidence that donors alter the composition of aid, rather than the overall amount, to reflect concern over poor governance in a recipient (Akramov 2014; Nordtveit 2014; Bermeo 2017; Winters and Martinez 2015). Simone Dietrich (2013) finds that donors are more likely to choose delivery channels that bypass the recipient government when corruption is high. The improved quality of data in recent years makes it difficult to compare disaggregated aid flows across time, although within these limitations there is some evidence that donors’ decisions to alter the composition of aid to account for quality of governance in recipients represents a shift from the Cold War period (Bermeo 2017).

A related branch of recent study examines the link between migration and foreign aid. Donor statements often link aid with a desire to decrease the push factors spurring migration. Panos Hatzipanayotou and Michael (2012) model aid as a donor response to decrease migration through increased development in migrant sending countries. Sarah Bermeo and David Leblang (2015) find that donors use foreign aid as part of a broader strategy to decrease the desire to migrate by promoting development in countries where the likelihood of migration to the donor is high. To foreshadow the importance of linking across allocation and effectiveness studies that I return to below, despite donors contention that development will deter migration, there is limited evidence to support this claim. Although Jean-Paul Azam and Ruxanda Berlinschi (2009) conclude that foreign aid leads to less migration, this finding is controversial. Jean-Claude Berthélemy, Monica Beuran, and Mathilde Maurel (2009) concluding that aid increases migration; a finding in line with a more general conclusion that increases in income can make migration more affordable and, therefore, more likely (de Haas 2007; Clemens 2015). Still other scholars pick up on the need to disaggregate aid when studying its link with migration: Jonas Gamsø and Farhod Yuldashev (2018) find that increased aid allocated to improve governance is associated with less emigration from a recipient, while finding no relationship between emigration rates and aid allocated for other purposes.

The link between migration and foreign aid is one area linking broader allocation and effectiveness studies with the domestic political economy of foreign aid. Sajal Lahiri and Pascalis Raimondos-Møller (2000) model a situation in which migrant groups in a donor lobby the government to increase aid to their home countries. Bermeo and Leblang (2015) show an empirical link between migrant political influence and foreign aid flows. Lauren

Prather (2020) demonstrates that among individuals, increased transnational ties lead to increased support for aid in general, not simply to the countries where their ties exist.

A broader literature has examined multiple facets of the domestic politics behind foreign aid decision-making. One set of studies sees foreign aid policy as an extension of a government's views on the welfare state more generally, expecting left-leaning governments to favor more foreign aid (Lumsdaine 1993; Thérien and Noël 2000; Noël and Thérien 2002; Chong and Gradstein 2008). Yet the empirical record on the relative quantity of aid given by left and right donor governments is mixed (Breuning 1995; Noël and Thérien 1995; Goldstein and Moss 2005; Lundsgaarde, Breunig, and Prakash 2007; Moss 2007; Chong and Gradstein 2008; Tingley 2010). Helen Milner and Dustin Tingley (2010) explain variation in support for foreign aid across US Congressional districts with reference to their economic characteristics and right-left ideological leanings. Another group of studies argue that purpose, rather than quantity, drives differences observed across ideologically distinct donor governments. Robert Fleck and Christopher Kilby (2006) find that Republicans in the US favor commercial interests and geopolitical priorities in aid policies. In cross-national work, Zachary Greene and Amanda Licht (2018) show that left-leaning governments increase disaster aid while right-leaning governments favor aid that increases trade, and Dustin Tingley (2010) finds that donor country ideological changes have an influence on aid effort to low income countries but not medium income countries. Johannes Kleibl (2013) shows that variation across donors can be driven by responsiveness to domestic interest groups within donor countries.

SUBNATIONAL ALLOCATION

Recent advances in data availability, particularly from the AidData project (Tierney et al. 2011), have made possible the examination of subnational allocation patterns in aid recipient countries. Studies use geo-coded aid projects in individual countries or across countries in a region to examine determinants of where aid flows. Ryan Jablonski (2014) demonstrates the inherently political nature of the allocation of aid projects in Kenya, finding that aid flows disproportionately to areas with high vote shares for the incumbent and areas where the population is largely of the same ethnic group as the incumbent. Similarly, Ryan Briggs (2014) demonstrates that Kenya's leader in the immediate post-Cold War period was able to exercise significant control over the location of aid projects, turning them into a form of political patronage for the leader's base. The generalizability of this finding is unclear. Afua Branoah Banful (2011) finds that fertilizer programs in Ghana targeted districts that had not favored the ruling party in the previous election. Takaaki Masaki (2018) finds a similar impact in Zambia, arguing that politicians lacking the ability to particularly target swing voters will funnel aid projects to districts that oppose the incumbent. In a cross-national study of 126 countries, Hodler and Raschky (2014) find that leaders channel more aid to their region of birth in countries with weak political institutions, suggesting one potential reason for variation in subnational allocation across countries.

The literature on subnational aid allocation also studies the question of whether poorer regions within countries receive more foreign aid. This is particularly important since some middle-income countries have large areas of poverty; while the country may

experience increased average income, poverty-focused donors may still choose to target the poorest regions within a country. Briggs (2017, 2018) as well as Hannes Öhler and Peter Nunnenkamp (2014) demonstrate that aid projects from the World Bank and African Development Bank were no more likely to flow to poorer regions within the African nations studied. Nunnenkamp, Öhler, and Maximiliano Sosa Andres (2017) examine World Bank projects in India and find no evidence that they disproportionately favor poorer districts; they also find no evidence of political patronage in allocation patterns across Indian states. As these findings make clear, and given the large number of aid recipient countries and even larger number of subnational units using foreign aid to finance objectives, further research is necessary to understand the conditions under which existing findings hold and can be generalized.

BUILDING ON ALLOCATION RESEARCH

The literature reviewed in this section provides a rich base on which to build. Going forward, it will be beneficial to advance research agendas that embrace the two-way relationship between aid allocation and effectiveness. Allocation patterns and donor statements are consistent with changing donor intent over time, which should inform studies of aid effectiveness: as intent changes, so may outcomes. The flip side of this is that studies of aid effectiveness can help us understand whether donors update their allocation patterns to account for evidence on when and how aid is most effective. Is there evidence of donor learning and adaptation to results from the field? Future research can enhance our understanding of the domestic political considerations underlying support and opposition for foreign aid and the effect this has on quantity, type, and cross-national allocation of aid. It can also examine whether the domestic political economy of aid has changed over time in keeping with changes in foreign policy goals. Finally, study of the subnational allocation of foreign aid is ripe for advancement, with new data and some important early studies paving the way for a fruitful line of research moving forward.

IMPACT OF FOREIGN AID IN RECIPIENT COUNTRIES

Examining the effects of foreign aid has led to often contentious arguments in academic literature and in broader coverage of the benefits and costs of aid. The most severe critics of aid argue that it has been wasteful and even done more harm than good. William Easterly (2006) decries the lack of results from more than \$2 trillion in aid spent in the five decades following the Second World War. Dambisa Moyo's (2009) concept of "dead aid" advances the idea that aid has had a net negative impact in Africa, fostering corruption and poor governance; here again the argument draws heavily on evidence from the Cold War period. Angus Deaton (2014) contributes to the chorus of critics arguing that aid has undermined domestic institutions and, therefore, caused harm in recipient countries. At the opposite end of the spectrum are scholars such as Jeffrey Sachs (2005), arguing that aid works and that the problem has not been too much but too little aid to help some countries escape from

a “poverty trap” and begin their climb on the ladder of development. Paul Collier (2007) writes of a “bottom billion” living in countries trapped in poverty and the need for aid to help them escape.

There is much to unpack in the aid effectiveness literature and many findings are still disputed—some quite vehemently. The harshest criticisms of aid focus on its perceived negative effects on corruption and political institutions, but changes in allocation patterns and recent findings on the aid-institutions relationship question the generalizability of these criticisms across time and place. Those studying economic impacts of aid disagree on the correct way to measure both aid and outcomes, and the best methods to employ in studying aid effects. I will separately examine work on the political and economic impacts of aid and highlight productive areas for future research.

POLITICAL OUTCOMES

Many of aid’s critics, such as Deaton and Moyo, focus on the perceived negative effects of foreign aid on political institutions in recipient countries. The negative effects could manifest through various channels. Perhaps most obviously, aid money could be used by governments for their own purposes to diminish opposition and maintain their power. This possibility led to studies of the links between aid and democracy, where critics argue that aid given to countries that are not fully democratic can thwart democratic change. Even without having an aggregate anti-democratic effect, foreign aid might decrease confidence in local government or crowd out domestic funding, leading to dependence and weaker domestic institutions in the sectors most affected. Aid could also foster corruption or even conflict.

The theory that aid entrenches anti-democratic regimes in recipients rests on the (sometimes implicit) assumption that donors cannot or do not stop recipient governments from using aid flows to their own political advantage. Aid is either intentionally given as a free resource or is allocated for a specific purpose but is fungible. This reasoning led to a series of influential studies that treated aid as a financial flow to governments, similar to other non-tax income like revenue from the sale of natural resources.

This literature draws heavily on the idea of the political resource curse advanced with respect to oil revenue by Michael Ross (2001, 2012). Theoretical models include the work of Alastair Smith (2008), depicting aid as one type of unearned income that can be added to the budget of an authoritarian regime to dampen the likelihood of revolutionary onsets. Kevin Morrison (2007) models aid as a form of non-tax income that can be used to provide goods and services that reduce the demand for redistribution and decrease the likelihood of democratization.

A wave of empirical studies concluded that aid can, in fact, decrease the likelihood of democratic change. Simeon Djankov, José Montalvo, and Marta Reynal-Querol (2008) conclude that foreign aid may be even more anti-democratic than oil revenue. Kevin Morrison (2009) argues that aid decreases the likelihood of regime transition, similar to state windfalls from oil sales. Bruce Bueno de Mesquita and Alastair Smith (2010) find that aid dampens the democratizing effects of mass political movements and decreases the likelihood of leader turnover in non-democratic countries, an outcome they also link to similarities with oil windfalls. Daniel Kono and Gabriella Montinola (2009) show that sustained aid flows help

autocrats survive. Different findings emerge from work by Stephen Knack (2004), where no significant relationship between foreign aid and democratic change is found.

While the above studies use different definitions of both aid and democratic change, they have something in common: none look for differences in the aid-democratization relationship over time. The findings on aid allocation outlined in the previous section show clear changes in patterns over time, consistent with changes in donor intent. Scholars have pointed out that the end of the Cold War meant aid from democratic donors could be less focused on propping up “friendly” dictators and could plausibly shift to promoting, or at least not hindering, democratization (Radelet 2003; Brautigam and Knack 2004). As pointed out by Paul Collier (2006), the motivations of aid donors may result in less fungibility than is observed with more obviously fungible sources of non-tax revenue. This could result in variation in the aid-democratization relationship to reflect differences in intent across donors and over time.

Studies that separately examine the Cold War and post-Cold War period find evidence of a change in the aid-democratization relationship. Studying African recipients, Thad Dunning (2004) shows that aid is associated with increased democracy in the post-Cold War period, but not earlier. Sarah Bermeo (2016) shows that higher aid flows were associated on average with lower likelihood of shifts toward democracy during the Cold War but not afterward. Erasmus Kersting and Christopher Kilby (2014) find differences in the aid-democratization relationship based on the geopolitical importance of the recipient, consistent with donor credibility on pro-democracy conditions driving the relationship. This is consistent with the finding in Bermeo (2016) that even in the post-Cold War period, a negative relationship between aid and democratic change can be observed in strategically important recipients.

Scholars have also examined nuances within the aid-democratization relationship. Sam Jones and Finn Tarp (2016) find that stable aid flows aimed specifically at governance are the most likely to have a positive association with democratic change. In a contrary finding, Mark Buntaine, Bradley Parks, and Benjamin Buch (2017) find that efforts by the World Bank to encourage institutional reform in recipients often result in shallow reforms that do not increase overall institutional quality. Haley Swedlund (2017) shows that the likelihood of cutting off aid if political conditions are not met varies with both the type of aid flows and the number of domestic veto players in the donor, with likelihood increasing if the aid is more fungible (e.g., budget support) and if the number of veto players is low. Desha Girod and Jennifer Tobin (2016) find that recipients are more likely to comply with World Bank conditionality when they want to maintain or attract FDI and less likely when they have high rents from natural resources. Joseph Wright (2009) concludes that the prospect of increased foreign aid encourages democratization if the incumbent leader expects to win elections.

An emerging question revolves around unintended consequences related to other measures of domestic government quality and capacity. In an early study of the aid-governance link, Stephen Knack (2001) finds that countries with a higher percentage of their government spending coming from foreign aid scored lower on indicators of governance. Other work has looked at individual sectors. Aid critics such as William Easterly and Angus Deaton acknowledge contributions of foreign aid to improvements in health outcomes. Yet even in the health sector there remains the possibility that aid substitutes for domestic resource mobilization, potentially increasing the dependence of the recipient country on aid and slowing the maturity of institutions. Marwa Farag et al. (2009) find evidence that aid

reduces domestic government spending in the health sector. The opposite claim is put forward by Amanda Murdie and Alexander Hicks (2013), who show that foreign aid in health can lead to increased domestic government spending in the sector by changing the policy-making climate and boosting the efforts of local activists. At a country level, Chunling Lu, Benjamin Cook, and Chris Desmond (2017) find that foreign aid is associated with increased government funding for health in rural Rwanda.

Studying subnational localities in African countries, Ann-Sofie Isaksson and Andreas Kotsadam (2018) find increased levels of corruption in localities hosting active Chinese aid funded projects; they do not find this association between aid and increased local corruption for World Bank funded projects. Studying preferences in Uganda, Helen Milner, Daniel Nielson, and Michael Findley (2016) find that citizens who view the government as corrupt or clientelist are likely to prefer projects financed by foreign aid to government-financed programs, suggesting that individuals may view aid projects as a less politically motivated substitute for government spending. Simone Dietrich, Minhaj Mahmud, and Matthew Winters (2018) use an experiment in Bangladesh to demonstrate that aid projects improve citizen feelings of favorability toward their local government. As with subnational allocation studies, these early studies on the sector-level or subnational political impacts of aid are relatively new and will benefit from expanded studies that examine the conditions determining the various relationships observed across countries and over time.

ECONOMIC OUTCOMES

Study of economic outcomes is often divided into two camps: those attempting to measure aggregate effects on a country's economy and those studying the impact of specific interventions, often employing randomized controlled trials to isolate effects. The micro-macro argument is not confined to academic outlets, as exemplified by a 2018 article in *The Guardian* written by fifteen economists (including three Nobel prize winners), arguing that the "micro" approach to aid effectiveness leads to an overemphasis on short-term goals and detracts focus from long-term, macro-level determinants of poverty and growth.² Proponents of the RCT approach argue that it is important to ascertain the effectiveness of individual aid interventions so that limited funds can be targeted toward programs that work. Additionally, it is not possible to infer causality from the observational nature of country-level studies, whereas RCTs allow a cleaner connection between cause and effect. In this section I will review some of the findings in this literature, highlight some issues and divisions, and offer some thoughts for moving forward.

Early studies of the economic impact of aid date back a few decades, first covering primarily the Cold War period, such as the work of Peter Boone (1996) showing that aid was associated with increased government consumption but not with poverty reduction. Shortly after this came the argument advanced by Craig Burnside and David Dollar (2000) that aid accelerates economic growth only in countries with existing sound economic policies. This finding was challenged by multiple studies, including William Easterly, Ross Levine, and David Roodman (2004) as well as Raghuram Rajan and Arvind Subramanian (2008), both of which find no positive association between aid and economic growth in the recipient even in the presence of good policies. As the Cold War slid further into the past, scholars began

to incorporate the potential impact of donor motivation on aid outcomes. David Bearce and Daniel Tirone (2010) find that aid was associated with economic growth in recipients in the post-1990 period but not before, a change they interpret as evidence that donors could more credibly condition aid on economic reforms in the post-Cold War period. Christopher Kilby and Axel Dreher (2010) model the difference between need-based and donor interest-based aid, finding that need-based aid contributes to growth while aid given to advance donor self-interest does not. Examining World Bank projects, Axel Dreher, Stephan Klasen, James Vreeland, and Eric Werker (2013) find that politically motivated aid is only less effective than aid given to less strategically important countries when the recipient has a weak macroeconomic position.

Difficulties arise in determining the best measures of the aggregate impact of aid as well as in deciding on the right time lag between receipt and expected impact. There is no universally accepted answer for overcoming these problems in empirical analysis; there are also no well-established instruments for foreign aid that would allay concerns of endogeneity. Because of these problems, critics point out that observational studies of the impact of aid can only establish correlations (or lack thereof) between aid and measures such as economic growth or human development.

Various studies have attempted to overcome some of these challenges. Michael Clemens, Steven Radelet, Rikhil Bhavnani, and Samuel Bazzi (2012) seek to address the timing issue by isolating “early impact” aid that is likely to have an effect on growth in the near term; they conclude that aid has a positive impact on economic growth. Sebastian Galiani, Stephen Knack, Lixin Colin Xu, and Ben Zou (2017) review previous uses of and criticisms toward instrumental variable approaches in studying the aid-growth relationship; they then exploit the fact that countries graduate from eligibility for World Bank IDA lending to develop a new instrument and test the aid-growth relationship, which they find to be positive. Channing Arndt, Sam Jones, and Finn Tarp (2015) note the difficulties involved in using growth as a measure of effectiveness and find that aid is associated with lower poverty rates and increased productivity, as well as with higher growth.

Given the problems with measurement and assessing causality when studying country-level impacts of aid, a different camp of scholars privilege studying the effects of individual projects. Advocating for the use of RCTs to study aid outcomes is most commonly associated with the work of Abhijit Banerjee and Esther Duflo (e.g., Banerjee and Duflo 2011). While it is outside the scope of this essay to catalogue the many RCTs that have been conducted on foreign aid projects, it is important to note that this approach to studying aid effectiveness has garnered many supporters in the development community. This extends beyond academia, with many aid agencies employing RCTs to evaluate projects; Banerjee and Duflo (2009, 152) note at one point 67 RCTs out of 89 program evaluations being conducted by the World Bank in Africa. Critics of RCTs note that many of the biggest development challenges—for example, climate change, corruption, quality of institutions—are not always amenable to study through RCTs; they also point out that the RCT method is not immune to methodological difficulties (e.g., Barrett and Carter 2010; Deaton and Cartwright 2014). Some of the toughest criticisms of the role RCTs have played in shaping the development agenda may be overblown. While critics argue that RCTs have led donors to eschew broader reforms in favor of easily measurable projects, it seems equally plausible that the causal arrow runs in the other direction: donors have embraced RCTs because they themselves lack political will to tackle long-term, often intractable development challenges.

Despite the arguments between the micro and macro approaches to measuring aid effectiveness, some recent contributions are working toward middle ground. Andreas Kotsadam et al. (2018) examine the impact of aid projects on infant mortality in Nigeria, using variation in proximity to aid projects to demonstrate a positive relationship between aid and reduction of infant mortality. Robert Marty et al. (2017) also use subnational data and find that areas in Malawi which received health aid showed decreases in malaria prevalence, although aid had not been targeted to the neediest areas in the country. These country-level studies that exploit subnational differences in aid allocation provide a mid-point between cross-national regressions and project-specific analysis. Another promising direction for research on aid effectiveness lies in aggregating results across countries from micro-level studies on the same type of intervention. For example, Abhijit Banerjee, Dean Karlan, and Jonathan Zinman (2015) compile the results of six randomized controlled trial studies on microcredit in as many countries and are able to draw on these collectively to argue that microcredit has some modest, but not transformative, positive development impacts.

ADVANCING THE AID EFFECTIVENESS RESEARCH AGENDA

This section has drawn out some of the findings and debates in the literature on aid's political and economic impact in recipient countries. It has also highlighted some areas where studies are just beginning to emerge and where more work is needed to draw robust conclusions. The future of research on aid effectiveness will advance our knowledge by continuing to explore new ways to measure the development impact of aid—ways that move beyond evaluation of project effectiveness to help estimate societal impact, both good and bad, but where the causal pathway is more direct than simply examining changes in economic growth. Scholars may also consider the extent to which aid induces unintended consequences, such as crowding out domestic spending and capacity.

Effectiveness studies should account for, and possibly even exploit, changes in donor motivations and allocation that may lead to changes in outcomes. For instance, if the proportion of aid that is “early impact” is influenced by donors’ decisions to allocate funding to different sectors in recipients based on governance, then the same indicators of effectiveness are unlikely to work across poorly-governed and well-governed countries. Future analysis should also take opportunity costs seriously: in a world of finite resources, positive impact of a project is not, on its own, justification for funding that project over the many others that could have been chosen.

DONOR LANDSCAPE

The landscape for giving foreign aid comprises multiple different types of actors, both as donors and as channels of delivery. The biggest categories of donors are individual state governments (giving bilateral aid directly to recipients) and multilateral organizations/banks

(often financed by donor governments but with some autonomy over their rules for aid allocation and implementation). I will focus here on comparing bilateral and multilateral aid, understanding the functioning of international development institutions, and providing an overview of the growing body of research on the difference between authoritarian and democratic donors.³

Early work on differences between bilateral and multilateral donors concluded that the latter were more concerned with quality of governance and rule of law when allocating funds (Dollar and Levin 2006). This tallies with the finding by Helen Milner (2006) that donor governments allocate aid to multilateral institutions because their publics prefer the presumed development motives of the institutions over the self-interested goals of the government. More recent studies have drawn a different conclusion. Kurt Annen and Stephen Knack (2015) note the similarity between aid disbursement patterns for many multilateral and bilateral agencies. Christina Schneider and Jennifer Tobin (2016) show that bilateral donors allocate aid to multilateral organizations that share their own development priorities. Matthew Winters (2010) provides evidence in a middle ground, finding that the donor-funded International Development Association (IDA) branch of the World Bank is less responsive to measures of good governance in recipients than its market-rate lending arm, the International Bank for Reconstruction and Development (IBRD). Helen Milner and Dustin Tingley (2013) find that support in the US for multilateral foreign aid falls when a domestic group has preferences that diverge from those of the institution. In recent years, significant growth in trust funds financed directly by donor countries but administered by multilateral organizations has further blurred lines between bilateral and multilateral efforts (Reinsberg, Michaelowa, and Eichenauer 2015; Reinsberg, Michaelowa, and Knack 2017).

The desire to understand the connections and dividing lines between individual donor priorities and outcomes at international development institutions has led to a number of important studies on the structure of international institutions in the foreign aid arena. Influential in this area is the work by Daniel Nielson and Michael Tierney (2003), which uses the case of the World Bank to develop the concepts of collective and multiple principals as applied through a principal-agent model of the relationship between donor governments and international institutions. Several studies have since applied the concepts of delegation and agency to the study of foreign aid.

Randall Stone (2008, 2011, 2013) argues that international institutions, including multilateral development institutions, operate under a system of both formal and informal governance: powerful states adhere to formal rules during ordinary times but intervene to move decisions toward their own self-interest when the stakes are unusually high. Robert Fleck and Christopher Kilby (2006) find that rather than a broadly delegatory approach, the US exerts significant influence over World Bank lending decisions. Kilby (2013) finds evidence that this goes beyond the use of formal mechanisms, with the US using informal influence over the disbursement phase of World Bank lending on a regular, rather than exceptional, basis. Christina Schneider and Jennifer Tobin (2013) show that heterogeneous preferences among European Union (EU) member states allow the European Commission significant discretion over aid allocation, while groups of member states with homogenous preferences are able to form coalitions to skew the distribution of aid toward their own national interests. Daniel Lim and James Vreeland (2013) present evidence that Japan uses its influence in the Asian Development Bank to increase loans from the institution to states holding temporary membership in the UN Security Council. Dan Honig (2018, 2019) addresses the

principal-agent model from a different angle, arguing that when organizations retain tight control on their field agents there is a tendency to focus on formal requirements rather than maximizing beneficial outcomes and an efficiency loss in not allowing agents to take initiative based on their superior on-the-ground knowledge.

These studies, which shed light on the organizational structure of international development institutions, should play a central role in analyzing the push for change advocated by some academics and policymakers. Scholars at organizations such as the Brookings Institution and Center for Global Development have called for international development institutions, including the World Bank, to take a more central role in addressing global challenges such as pandemics and climate change.⁴ Such studies call for changes in governance at these institutions to better serve these goals. Considering the role of development institutions as providers of public goods is not new, as multiple scholars have pointed out that development itself has characteristics of a public good (e.g., Annen and Knack 2015; Olson and Zeckhauser 1966; Steinwand 2015). Yet the findings outlined above suggest that institutions are often swayed from their stated goals and bent to serve the self-interest of powerful actors. Political economy insights also suggests that formal governance change may be difficult to achieve and even then may not address many underlying, informal, governance issues.

Nilima Gulrajani (2015) argues that scholars of international institutions like the World Bank have paid insufficient attention to those who study bureaucracy and bureaucratic change. A notable exception is Catherine Weaver (2008), who concludes that change at the World Bank is made difficult by a combination of external actors and organizational culture. Mark Buntaine (2016) provides some evidence of development bank learning and adaptation, showing that under certain circumstances member states can force an institution to increase selectivity and deliver more effective results. Dan Honig (2018, 2019) shows that organizational structure can impede responsiveness, with tight control by principals over agents in the field making it difficult to adapt to changing contexts. Christopher Kilby (2013) argues that pervasive informal influence by powerful states means that reform at international financial institutions would need to go beyond changes to the formal rules if the goal is to limit the ability of powerful actors to sway outcomes toward their own self-interest, as would be necessary to tackle problems of global public goods. All of this suggests that a fruitful area for future research lies in better understanding the optimal structure of international institutions for confronting important transnational problems, and how to either reform existing structures or build new structures to deliver desired outcomes.

Before concluding this section on the landscape of foreign aid, it is important to briefly mention the increase in research on non-democratic aid donors, much of which has been made possible by the creation of AidData and, particularly, its database on Chinese foreign aid (Dreher et al. 2021). The rise of aid from China (including its often publicized Belt and Road Initiative), along with other instances of aid from non-democratic donors, raises the potential of what Moisés Naim (2007) refers to as “rogue aid”—aid that not only comes without conditionality attached but that may undermine the attempts at conditionality by other donors. Recent work shows that foreign policy considerations drive the cross-national allocation of aid by China (Dreher et al. 2018) and also that subnational allocation of Chinese aid is skewed toward the birth region of the political leader in African recipients, demonstrating significant political influence in project locations (Dreher et al. 2019). Denghua Zhang and Graeme Smith (2017) shed light on the internal decision-making process of crafting aid policy within China. The abundance of working papers examining

China's aid practices suggests a time of increased productivity and rapid learning over the next few years.

The flurry of work made possible by new data should not obscure the fact that scholars have been studying authoritarian donors for some time. Pathbreaking work by Deborah Brautigam (2009) demonstrates that China's involvement in Africa has been ongoing for decades, is shaped by its own experience with both aid and development, and is much less aid-heavy than many popular accounts suggest. Furthermore, she argues that China is far from the only donor that does not follow best practices with regard to governance when allocating aid, calling into question the "rogue" designation. Experts have also examined aid from other authoritarian donors. Eric Neumayer (2003b) finds Arab country donors more likely to give aid to recipients that are poorer, Arab, Islamic, and/or in sub-Saharan Africa. Sarah Bermeo (2011) compares the link between aid and democratic change for both oil-rich Middle Eastern donors and OECD democratic donors in the post-Cold War period, finding that a negative association exists between aid from authoritarian donors and democratization but is not observed when aid comes from democratic donors.

A full understanding of the influence of China and other authoritarian donors on political institutions in recipients must include an analysis of how democratic donors react. When donors see aid as part of a geostrategic game, their own efforts at conditionality lack credibility and are likely to fail (Kersting and Kilby 2014); this has led many democratic donors to support strategically important authoritarian regimes, a phenomenon that has decreased but not disappeared. If, for instance, democratic donors respond to the rise of China's development influence similarly to the way they reacted to Soviet or Chinese influence in developing countries during the Cold War, it becomes a real possibility that even if Chinese aid does not undermine political institutions on its own, it may still have an indirect effect by weakening credible conditionality on the part of rivals for influence in these states (Bermeo 2019).

MOVING FORWARD WITH RESEARCH BRIDGES

The literature on foreign aid has grown substantively in the last two decades and the topic itself has come to be seen as a key aspect of foreign policy for donors and an important component of the international political economy field. Significant progress in understanding motivations, outcomes, and the aid landscape has been made by an interdisciplinary combination of scholars. The goal of this essay has been to group findings in a cohesive way that suggests pathways forward to continue the evolving study of foreign aid. Key gaps include: a relative lack of emphasis on understanding the two-directional relationship for studying aid allocation and aid impacts; a continuing struggle to quantify the effects of aid on both subnational and country-wide political and economic outcomes; and remaining uncertainty regarding the aid landscape and its ability to deliver desired results, particularly in relation to global public goods.

Many of the questions raised by these gaps are relevant for policy as well as scholarship, and those arising from expert disagreements seldom pose the stark trade-off that is often portrayed. Compromise between experts separately concerned about long-term and short-term development impact from aid would be beneficial: those focused on short-term, measurable results can acknowledge that sustained growth goes beyond measuring the effects of

individual interventions and those focused on long-term growth can admit that knowing how best to provide health services, food, and education to people in the short run can have long-run benefits. Democratic donors struggling to determine the best response to the rise of China as an important development actor need to understand both the direct impact of China's financial flows and the indirect impact if they decide to re-orient their own aid programs in a geopolitical response to China's influence. Grappling with the benefits and drawbacks of formal and informal structures of existing institutions will help drive institutional reform and the design of new organizations to meet global challenges that need coordinated action to provide public goods as solutions.

At the outset of this chapter I questioned the assertion by the editors that research silos represent a "first best" outcome in the area of foreign aid scholarship. After reviewing the literature, my conclusion is unequivocally that it depends on what comes next. Segmenting the field has served to jump start an impressive amount of scholarship on a variety of sub-topics in the last 20 years, but has created gaps that can only be filled by moving between those silos. Given the rapid increase of political scientists studying foreign aid, there is reason to be optimistic that the next decades will produce important answers as well as a new generation of stimulating questions to guide further research.

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NOTES

1. Claessens, Cassimon, and Campenhout (2009); Dollar and Levin (2006); Berthélemy and Tichit (2004); Svensson (2000); Collier and Dollar (2002); Neumayer (2003a); Easterly (2007); Clist (2011); de la Croix and Delavallade (2014); Easterly and Pfutze (2008); Easterly and Williamson (2011).
2. *The Guardian*, "Buzzwords and tortuous impact studies won't fix a broken aid system," July 16, 2018; available: <https://www.theguardian.com/global-development/2018/jul/16/buzz-words-crazes-broken-aid-system-poverty>.
3. Although not addressed here, studies such as those by Sarah Bush (2015) on agendas pursued by non-governmental organizations in foreign aid delivery and by Tim Büthe, Solomon Major, and André de Mello e Souza (2012) on the increase of non-governmental funding sources ("private aid") offer interesting insights regarding other actors in the aid landscape.
4. See, e.g., Bhattacharya et al. (2018); Center for Global Development Report, "Multilateral Development Banking for this Century's Development Challenges: Five Recommendations to Shareholders of the Old and New Multilateral Development Banks," October 2016.

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PART VI

ACTORS

CHAPTER 32

THE EVOLVING STATE OF THE STATE IN INTERNATIONAL POLITICAL ECONOMY

HEATHER BA

IMPLICIT in every theory or explanation of any international political and economic phenomenon is a certain conception of the nation-state. Three established conceptions of the nation-state have a long and well-established tradition in the field of International Political Economy (IPE), each aligning with a major research paradigm within IPE or the broader field of international relations.¹ Realists, assisted by British school and structuralist IPE scholars, forged the first conception of the nation-state as a unitary actor. Open economy politics scholars, in contrast, highlighted the role of the state as a unique political regime. Dissatisfied with both lop-sided perspectives, constructivists clamored for a third “structurationist” conception. In the words of Alexander Wendt (1987: 365):

“Theorizing the state” implies a research endeavor which seeks to develop a theoretically and empirically grounded understanding of the causally significant properties (such as powers, interests, practices) of the state as an organizational agent or entity. Ideally such a theory would define exhaustively the possible ways of acting of state agents, rather than generate determinate predictions about particular state behaviors. The possible ways of acting of an agent are constituted and therefore explained by the social structural context in which it is embedded and by its internal organizational structure, and as such they are amenable to structural explanation.

A fourth, emerging, research paradigm called the “New Interdependence Approach” or the “Political Economy of Complex Interdependence” offers a promising new conceptualization of the nation-state that encompasses the best insights offered by traditional paradigms on the role and function of the nation-state, and may finally provide an epistemological and empirical approach that is satisfactory to all. This article reviews the evolution of scholarly thinking about the nation-state within the field of IPE and proposes a new, more encompassing conception of the nation-state as a cross-section of the international political economy multiplex.

THE STARTING POINT FOR THE STATE: THE UNITARY ACTOR MODEL

The first and earliest conception of the nation-state within IPE literature is the “unitary actor” model. The unitary actor model is the cornerstone of the realist paradigm in international relations. Though realists have used the unitary actor model exclusively, scholars of the British school, and early liberalism have also relied heavily on this conception of the nation-state. In this model, nation-states are simply governments that possess territorial control. Governments are the primary actors operating in the global arena, and they act primarily in accordance with interests that are monolithic, undifferentiated, and persistent over long periods of time. For realists such as Thompson and Morgenthau (1985), Morgenthau (2006) and Waltz (1979), these interests include survival, preservation of autonomy, and hence, relative national military power. The unitary actor model generally rejects the relevance of domestic political events and characteristics for understanding the dynamics of international relations (Waltz 1979, 65), focusing instead on states’ universal search for survival and a balance of power between states that can produce security (179, 93–9).

The unitary actor model selects national governments as the default actor because it is premised on the notion of “Westphalian sovereignty,” a legal principle supposedly codified in the Treaty of Westphalia at the end of the 30 Years War. Westphalian sovereignty has a long tradition in the field of International Relations (IR). Many IR scholars cite the Treaty of Westphalia as the beginning of the international system (Wendt 1999; Jackson 1993; Philpott 2001; Bull and Watson 1984; and Clark 2005).² Thompson and Morgenthau (1985, 29) writes the “rules of international law were securely established in 1648 . . . the Treaty of Westphalia . . . made the territorial state the cornerstone of the modern state system.” Mark Zacher (1992, 59) adds that the Treaty of Westphalia marks the point in history when territorial governments replaced the Church as the primary and most powerful actor in the international system: “the Treaty of Westphalia recognized the state as the supreme or sovereign power within its boundaries and put to rest the church’s transnational claims to political authority.”

More recently, however, some scholars have contested the historical significance of the Treaty and the origins of the legal notion of sovereignty, arguing instead that the idea of territorial autonomy existed long before the Treaty, and that the agreement itself did not grant sovereignty to any European country (Osiander 2001; Beaulac 2004). Osiander (2001) explains that even after the Treaty of Westphalia, constitutionally established legal and juridical procedures of the Holy Roman Empire continued to limit the sovereignty of estates within the empire.^{3,4} Instead, Osiander (2001) and others (Beaulac 2003; Kayaoglu 2010) propose that the idea of legal sovereignty, which undergirds the unitary actor model, originates from the legal innovations made by jurists and legal scholars, such as Emer de Vattel, following the Industrial Revolution:

IR theory, and its still-dominant paradigm realism, developed against the background of what may be called the ideology of sovereignty. It was not realized that, far from being traditional, this ideology had its roots only in the transient nineteenth-century heyday of state autonomy. . . . Industrialization is about division of labor, which it brings about on an ever greater

scale. In the nineteenth century, this process raised the level of the most important economic circuits from the local to the “national” (that is, state) level; this evolution made the state more integrated and strong and gave us the sovereign state (rather than prince) as, intellectually, we know it.

(Osiander 2001, 282)

During the Industrial Revolution, technological innovation proliferated and connected formerly isolated rural communities. Transportation technology facilitated urban migration and the emergence of urban metropolises. Naval technology facilitated the mass movement of people beyond borders, producing colonization and slavery. The larger, and more interconnected communities produced by these processes required more consolidated and powerful central governments that could establish property rights, set labor laws, regulate trade, and provide a public money (Buzan and Lawson 2013, 625–6). Technology, industrialization, trade, migration, colonization, and slavery during the Industrial Revolution enlarged the size and role of governments and necessitated the development of legal terminology to justify and codify invasion, occupation, and variegated levels of citizenship. These changes, rather than the Treaty of Westphalia, more directly produced the legal concept of national sovereignty that early twentieth-century IR scholars first used as the conceptual building blocks of their theories.

Realism emerged in the twentieth century in response to the evolution of international law over the course of the eighteenth and nineteenth centuries, and to the tenuous accomplishments of international legal theorists in establishing international institutions after the World Wars (Domingo 2011; Burley 1993). Realists merely adopted the legal definition of a nation-state for their own analytical purpose: to demonstrate that the logical consequence of national sovereignty is international anarchy, a condition of uncertainty that forces states to be preoccupied with their own survival.

This first conceptualization of the state as a unitary actor also reflected early IR scholars’ preoccupation with explaining the most destructive form of international relations—major power war. Militaries raised by states to defend territorial sovereignty have fought the most consequential wars. Thus, for realists writing between and soon after two of the deadliest wars in history, autonomous sovereign nation-states seeking to defend their territorial integrity constituted the most conspicuous actors.

The unitary actor model premised on the legal definition of a sovereign state does capture salient features of a nation-state: consolidated political and governmental control over a geographic territory with boundaries recognized by other governments is a universally accepted definition of nation-state, enshrined in the United Nations charter. However, many IR scholars now concede the myth of Westphalian sovereignty, and view the unitary actor conception of the nation-state as over-simplified and incomplete for the purposes of understanding international relations (Krasner 1993 and 1995; Osiander 2001; Kayaoglu 2010, 194).⁵

The criticisms of the unitary actor model of the state are essentially four-fold. First, the unitary actor model “reifies” the structural relations of nation-states (Wendt 1987). Second, it ignores the often influential actions of state constituents who are not official governmental representatives (Milner and Keohane 1996). Third, it ignores the fact that various constituent agents have diverse interests and often adhere to a set of rules and norms that affect states’ behavior toward one another (Milner 1997). Fourth, it ignores the fact that the nation-state is

a social construct that evolves as human society evolves, and may eventually fade in importance and relevance (Wendt 2003). These shortcomings have motivated several subsequent alternative models of the nation-state.

REFORMING THE UNITARY ACTOR MODEL: THE STATE AS A POLITICAL REGIME

Early liberal scholarship, which spawned the contemporary field of international political economy, understood well the limitations of the unitary actor model of realism. In response, proponents broadened IR's sphere of inquiry beyond international conflict and departed from the unitary actor model of the state. Keohane and Nye's (1977) *Power and Interdependence*, which epitomizes the contemporary liberal paradigm, distinguishes between *interstate relations*, which are the official diplomatic relations between governments consistent with the unitary actor framework of the realists, and *transnational* and *transgovernmental* relations, which better characterize international relations when states do not act coherently and non-government actors also constitute influential actors (168). Most research within contemporary IPE acknowledges these truisms of liberalism; Private economic actors that function across borders constitute important and influential actors capable of influencing international economic conditions and economic foreign policy.

However, some early and influential IPE theories retained the unitary actor conception of the state, even if merely for analytical convenience. Early structuralist and institutionalist perspectives heavily relied on the unitary actor model of the nation-state. The hegemonic stability theory research program spawned by Kindleberger's (1975) *World in Depression* largely treated nations as unitary actors facing a collective action problem of coordinating trade, investment, and fiscal policy. Keohane's (1982, 1984) theory of international institutional cooperation similarly treats states as individuals facing a collective action problem. Moreover, early British IPE scholarship also emphasized economic relations between countries and how the pattern of these exchanges conferred power and privilege to particular states (Cohen 1977).

Liberal and IPE structuralist scholars did envisage a state to be something more than a government that retains sole control over a piece of territory. They understood that states do more than raise and operate a military; They also set rules and norms about economic activity. These scholars therefore understood that a nation-state is also a market, and that market actors located in and legally incorporated under the laws of a specific sovereign state, but which engage in economic activities that transcend national boundaries, also constitute important international actors. However, these research programs did not manage to offer a clear, comprehensive, alternative model of a nation-state that is distinct from the unitary actor model. The end of the Cold War changed this, as IR scholars turned to mid-level theory to round out the initial contributions of the unitary actor model and more fully explain variation in foreign economic and security policy.

Within IPE, IR scholars' post-Cold War emphasis on mid-level theory culminated in a new research program called Open Economy Politics (OEP).⁶ OEP succeeded in forging a wholly different, second conception of the state, as a "domestic political regime." This

conception of the state arose in direct opposition to the unitary actor model (Elman 1996; Rose 1998). While OEP views nation-states as central actors in the international system, it seeks to explain variation in countries' foreign economic policies, and therefore presumes that states possess varied economic interests determined by different *domestic* political and economic characteristics. OEP views a nation-state as a mechanical policymaking apparatus, consisting of governmental institutions that filter actors' interests and preferences into a set of evolving foreign policies (Frieden, Lake, and Schultz 2010). These foreign policies ultimately reflect the preferences of the actors who have the most control and influence within the policymaking apparatus (Milner and Keohane 1996). In OEP theories, non-state actors can be extremely influential actors in the international arena, but they are primarily so only in interaction with the national government, as they wield power within the policymaking apparatus. In this sense, the nation-state in OEP theory is primarily an official representative of an especially powerful set of domestic market actors. The domestic political regime determines which actors are best represented. Non-state actors that operate in the international arena have limited influence over outcomes apart from their ability to influence the governments that preside in the geographic area where they operate.

What OEP accomplished with its new model of a state as a domestic political regime is the articulation of a simple framework for beginning to understand the microfoundations of foreign economic policy: actors organize to advance their interests within domestic political institutions, which ultimately create policies. However, OEP has attracted several critics who question the proposition that national-level outcomes are, as OEP implies, a linear function of domestic political actors' interests and institutions (Oatley 2011; Cohen 2017; Abdelal, Blyth, and Parsons 2011). Surely individuals also respond to the context and events that occur in the broader international environment. Furthermore, not all economic policies are the product of material self-interests, and people's misperceptions of their environment prompt responses. In fact, constructivists created a third conceptualization of the nation-state with these criticisms in mind.

A THIRD WAY: THE STATE AS A SOCIAL CONSTRUCT

The most well-established heterodox research paradigm in IR and IPE, constructivism, presents a third unique conception of the nation-state as a "social construct" (Abdelal, Blyth, and Parsons 2011). According to the social construct view, a nation-state and an economy are the products of a non-ergodic aggregation of shared beliefs and ideas. Individuals have material interests, but those interests are not solely the product of rational, utility-maximizing preferences, as orthodox research paradigms imply. According to this perspective, the legal concept of the sovereign nation-state is nothing more than the product of a distinct, shared social ontology, and what distinguishes one from another may be sets of ideas, norms, and values more than geographical boundaries (Blyth 2011, 83). Moreover, the fluid notion of ideas, norms, and values may mean that the traditional legal definition of nation-states becomes less meaningful as technology enables people to communicate and share ideas, norms, and values regardless of physical location.

The constructivist conception of a state presents a radical challenge to both the unitary actor and political regime models of the state. First, it places a very different emphasis on the types of actors who are influential in the international arena, with an array of non-state actors, including firms, non-governmental organizations, professional and advocacy networks, epistemic communities, religions, expatriates, and individual leaders operating in unofficial capacities all potentially influencing an array of political and economic phenomena. Thus, non-state actors have equal amounts of influence as official actors in affecting international outcomes.

Second, the constructivist paradigm accommodates a much broader range of motivations on the part of actors, including material interests and normative beliefs. Because actors' uncertainty and beliefs about their environment and the consequences of their own actions affect their policy preferences, economic behavior, and proclivity toward cooperation and violence, a state is much more than a set of rules that determine the relative successfulness of actors' profit-seeking policy-preferences. The state represents an emergent social system whose behavior is highly contingent on past events, surprising, erratic, and altogether different than the behavior of its individual citizens.⁷ The constructivist approach rejects the linear, equilibrium models of state behavior implied by domestic political regime and unitary actor conceptions of the state (Wendt 1992; Wendt 2003; Blyth 2011, 85).

Lastly, the constructivist paradigm suggests that since the nation-state is a social construct, which humans have created to help reduce the scale of the cooperation and coordination inherent in human survival and betterment, the nation-state need not be a concept that persists. Humans could transform the entity and even abandon it entirely as we learn and evolve. This emphasis on the evolution of the nation-state is a point made by British school scholars (Strange 1996) and liberals, who hypothesized the decline of state autonomy in the face of global capitalism. However, constructivists championed the argument, exploring in more detail the changing form and function of the social entity we call the nation-state (Weber 1998), ultimately arguing that its dissolution or transformation to a world-state is inevitable (Wendt 2003).

The constructivist critique of the orthodox IR and IPE research paradigms is an important one. There are many international phenomena that exhibit non-normality, non-linearity, contingency, and evolutionary characteristics. Constructivists were also the first to articulate clearly the need for a "third-way" to study international relations, one that would facilitate the examination of the nation-state as an evolving human construct—a product of past human behavior that shapes contemporary human behavior:

Despite their many differences, however, the "agent-structure," "parts- whole," "actor-system," and "micro-macro" problems all reflect the same meta-theoretical imperative—the need to adopt, for the purpose of explaining social behavior, some conceptualization of the ontological and explanatory relationship between social actors or agents (in this case, states) and societal structures (in this case, the international system).

(Wendt 1987, 338–9)

Constructivists challenged advocates of the unitary actor model to substantiate the existence of the unobservable social structure that determined states' obsession with security and survival, while simultaneously urging advocates of the political regime model to "accept the reality and explanatory importance of irreducible and potentially unobservable social structures" that motivate state behavior (Wendt 1987, 356).

However, the constructivist paradigm offers no single epistemological framework or empirical strategy for understanding and analyzing international political and economic phenomena. Though Wendt originally articulated a structurationist vision for the paradigm, supported by historical and structural analysis, the paradigm has evolved to become an eclectic compilation of different qualitative approaches that emphasize the common principle that theories of aggregate social behavior should better reflect the realities of human cognition and the complexity of human social organization (Abdelal, Blyth, and Parsons 2011, 5–9).

COMING FULL CIRCLE: THE STATE AS A CROSS-SECTION OF THE INTERNATIONAL POLITICAL ECONOMY MULTIPLEX

A new, burgeoning paradigm of political economy, which has also evolved as a critique of orthodox paradigms, could serve as the basis for a fourth conception of the state. This new paradigm might best be termed the “new interdependence approach” (NIA), which is a name coined by Farrell and Newman (2014a, 2014b, 2016), but is best exemplified by Thomas Oatley’s (2019) contribution, which offers the most well-developed analytical framework for the paradigm he calls the “political economy of complex interdependence” (PECI).⁸ Both the NIA and PECI approaches are an attempt to bridge the levels of analysis gap in the fields of IR and IPE, and to construct theories that address the interaction between domestic and international politics (Singer 1961). As a recent state-of-the-field assessment indicates, the problem of how to study domestic decision-making while accounting for varied international contexts continues to subsume contemporary IR research (Grieco 2019). While some may see “analytical eclecticism” as the best prospect for surmounting this obstacle (Sil and Katzenstein 2010, 2011; Grieco 2019; Abdelal, Blyth, and Parsons 2011, 8), the NIA/PECI approach could accomplish the seemingly-elusive, single analytical framework that bridges the divide between domestic and international politics.

The new interdependence approach, as articulated by Farrell and Newman (2016), is an attempt to *endogenize* globalization and to explain how this process of interconnectedness between states has shaped domestic level politics. They argue that the contemporary international political economy is premised on a set of overlapping rules that sometimes conflict with national or individual private actors’ interests and can change the incentives for maintaining certain domestic policies. Furthermore, the structure of globalization, premised on overlapping rules, creates opportunities for actors to form transnational alliances. Lastly, domestic institutions interact with the new system and create power asymmetries. Despite the fact the NIA claims to integrate these assertions “into a cohesive whole” (2016, 716), the framework remains somewhat piecemeal and ad hoc. Rule overlap (between domestic and international institutions), opportunity structures (for transnational alliances between similar groups of actors), and power asymmetries (produced by institutions) are certainly characteristics of our contemporary globalized world, but whether they are sufficient to accomplish the task of *endogenizing globalization*, by providing a singular framework that is applicable to both domestic and international politics, and which is capable of sorting out the interaction between the two is far from clear.

On the other hand, Oatley's (2019) answer to this problem is the adoption of a broader scientific paradigm that now pervades the natural sciences: complexity science.⁹ Complexity science is the study of self-organized systems. Oatley makes a strong case that the international system satisfies the criteria of a complex system: "a system in which large networks of components with no central control and simple rules of operation give rise to complex collective behavior, sophisticated information processing, and adaptation via learning or evolution" (Mitchell 2009, 13). The macro-level behavior or characteristics of complex systems are often surprising and are either unpredictable or very hard to predict. In complex systems, components of the system learn, evolve, and react to other components of the system in infinitely varied ways that are mathematically more akin to non-linear functions than to the simple, linear functions that have traditionally served as the mathematical intuition for most mainstream, Newtonian IR and IPE theories.

Complexity science often studies systems, whether universal, environmental, technological, or human(social), as networks. Units in a system are represented by nodes in the network. Nodes affect and are affected by the behavior of other nodes in the network. The relationships between nodes are represented by ties. The nodes themselves often evolve or change their behavior over time as a result of their interactions. Many complex systems are open systems, meaning units of the system enter and leave the system over time. The social network framework is a natural analytical framework for thinking about politics across levels of analysis and over time.¹⁰

Politics are a social allocative behavior—the process people invent and use to decide who gets what (Laswell 1936). The power of people—human autonomy from and influence over one another (which is perhaps the social science equivalent of natural laws such as gravity or electromagnetism)—determines the nature of the process. Power is, in turn, affected by the process. Sometimes these processes are informal and understood, other times these processes are formalized, transcribed, and codified. Thus, political systems and the institutions (formal or informal) that constitute these allocative processes represent the distribution of power within human relationships. People are units in a social system, or nodes in social networks, and the influence they hold with one another are the ties between the nodes. The structure of this social network reflects the distribution of power and influence within a social system, and can therefore also represent institutional arrangements. As power dynamics shift among people, so do ties. The overall network structure and institutional arrangements will transform as well.

A market is also a form of social allocative behavior that can be represented as a network and analyzed as a complex system. Nodes in a market network can represent a variety of different market actors (consumers, firms, banks), while the ties between them can represent various economic exchanges (sales, purchases, trades, loans, repayments). As a discipline, economics, like political science, has been slow to recognize this point and embrace complexity science. However, there are more than a few economists who have used the framework and methodology of complexity science and networks to shed light on puzzles in the fields of economics and finance, especially on outlier events that are not well explained by traditional equilibrium based theories and models, such as the role of money in an economy, stock market crashes,¹¹ and financial crises.¹² Network analysis of markets can illuminate important characteristics of markets, such as inclusivity, diversity, and sensitivity or dependency on the transactions of particular market actors.¹³ Network methods have also been used to simulate aggregate cyclical market behaviors that have proved difficult to model using traditional game theory.¹⁴

A simple network is a fitting model for a political or market system, but politics spans a wide variety of issues, all of which are interrelated. Political economists, who have traditionally been preoccupied with the political underpinnings of economic markets, understand this reality well.¹⁵ Since economic and political influence is typically transferable and the social structure of markets can reflect the sociopolitical structure of society (and vice versa), political and economic networks are especially likely to co-evolve (Modelska 1996, 331). The NIA/PECI framework can adapt to handle multiple, interconnected networks. A network can represent the relevant social community for a particular issue, but if issues are related, then relevant social networks are too. We can analyze multiple, interrelated networks as a multiplex—or a multidimensional network. In a multiplex, nodes are connected within and across networks. The international political economy is in fact a multiplex¹⁶—trade, foreign direct investment, portfolio investment, interbank lending, global value chains, preferential trade agreements, and bilateral investment treaties are all interrelated legal and de facto networks that constitute the global political economy.

Applying the complexity and network frameworks to political economy in this manner also helps to illuminate the fact that domestic and international politics are very similar in nature. This makes sense if we think about the fact that many natural laws, like gravity and electromagnetism, are applicable to the natural world at many different scales, be it subatomic particles or planets. As Murray Gell-Mann eloquently explains:

Although the “reduction” of one level of organization to a previous one—plus specific circumstances arising from historical accidents—is possible in principle, it is not by itself an adequate strategy for understanding the world. At each level new laws emerge that should be studied for themselves; new phenomena appear that should be appreciated and valued at their own level. It in no way diminishes the importance of the chemical bond to know that it arises from quantum mechanics, electromagnetism, and the prevalence of temperatures and pressures that allow atoms and molecules to exist. (1996: 12)

Similarly, humans’ solutions to allocative problems will be similar regardless of the size and scale of the community involved in making the decision.^{17, 18} Studying allocative behavior at one level will likely help us understand it at another level, but there may also be some unique dynamics at work at each level of aggregation because the scale of complexity increases as social systems become larger. The main distinction between international and domestic politics is simply that in the latter, we observe the allocative behavior in some small part of the global human system whereas the former observes allocative behavior across the entire global human community. Therefore, political and economic phenomena at any level of analysis need not be de-emphasized, deprioritized, or analyzed in isolation from one another.¹⁹

The NIA/PECI approach provides a single analytical framework for making sense of politics on any level, which can be extended to help think about how, when, and why domestic politics or allocative processes occurring in one part of the global system might have consequences for allocative processes in other parts of the system, or across the entire system in its entirety. Bridging the levels of analysis, “endogenizing globalization” (in the words of Farrell and Newman), or “conceptualizing the ontological and explanatory relationship between social states and the international system” (in the words of Wendt), all entail accomplishing precisely this exercise. Using the NIA/PECI, IPE scholars can zoom in on a particular community within the global IPE multiplex, to examine how local conditions—unique properties of the structure of the community—impact policy outcomes, market

performance, and distribution, while at the same time considering how the embeddedness of the community within the IPE multiplex might also affect these outcomes.

Additionally, using the NIA/PECI framework, we can begin to think about how and why allocative processes that occur in one cross-section of the international system might have more or fewer consequences for allocative processes occurring in other parts of the system. Contagion and diffusion are well-documented network processes within global politics. They typically occur either because allocative solutions diffuse across sub-systems (via learning or emulation) or because one group's decision to allocate resources in one part of the ecosystem will inevitably have consequences for the allocation of resources writ-large (via competition), since all humans rely on and are part of the same environmental ecosystem.

²⁰ In order to make sense of how these contagion and diffusion processes unfold, we need to have an understanding of the overall structure of interdependence, which is provided by the NIA/PECI paradigm. Events that occur in nations that are more tightly enmeshed in the international system will likely be more influential than those that occur in countries that are more isolated.

A unique strength of the NIA/PECI paradigm is that it encompasses the best insights of past paradigms, while also resolving many of their shortcomings. As the realist and structuralist paradigms advocate, NIA/PECI provides a framework for considering the entire international system and understanding dynamics between nations in the system. It improves this approach by providing a compatible quantitative empirical methodology that allows scholars to accurately describe system structure and to determine the distribution of international power within it. This allows scholars to move beyond traditional, oversimplified notions of polarity for describing system structure.²¹ For the OEP paradigm, the NIA/PECI facilitates a power analysis of the sub-systems that collectively constitute the international system, and, to varying degrees, drive the dynamics of the system. To improve this framework, the NIA/PECI provides a framework for thinking about when and why some local events might matter more or less for the broader system level. Finally, as constructivists argue, the NIA/PECI framework provides a model for thinking about socialization processes at the global level. Socialization reflects the underlying patterns of social interactions and connectedness. The power and influence of ideas, norms, and values reflect social structures. NIA/PECI embraces the complexities of social systems, and acknowledges the role of non-linearity, non-normality, contingency, and evolution that pervade aggregate social behavior of all stripes. At the same time, NIA/PECI provides a single analytical framework that might envelop and organize the analytical eclecticism that now pervades and perhaps hinders the attainment of the paradigm's worthy ambitions.

Similarly, the NIA/PECI research paradigm provides a new, improved, and more-encompassing understanding of the nation-state. Within the NIA/PECI paradigm, the nation-state is simultaneously a unitary actor, a domestic political regime, and a social construct that may be fading into irrelevance. In the NIA/PECI research paradigm, the nation-state is best conceptualized as a cross-section of the human IPE multiplex, which is embedded in a larger, global human system, constituted by smaller, increasingly interconnected human communities with their own form of social allocative rules, and an expression of the material interests, ideas, norms, and values of the social creatures that form its most basic unit of analysis.

Thinking of the nation-state as a cross-section of the IPE multiplex satisfies the criteria of the unitary actor framework because scholars can choose to divide the multiplex into

whatever components they decide are most useful for examining and explaining a particular phenomenon. If and when the applicable cross-section is the traditional, legal definition of a sovereign nation-state, the NIA/PECI framework can accommodate this. Nodes of the IPE multiplex can represent the traditional, legal definition of a sovereign state, linkages can represent the flows of goods, money, assets, people, legal agreements, or violent interactions between them. The structure and pattern of these linkages provide an empirical tact with which to answer classic realist and structuralist questions regarding the structure of the system, system performance, and system stability.²²

Viewing the state as a cross-section of the global IPE multiplex also satisfies the criteria of the “political regime” conception of the state, in that it acknowledges that the state is its own complex social system with its own set of allocative rules. A nation-state in this context is a policymaking system and its own complex political-economy. In other words, the nation-state is, as NIA/PECI proposes, its own social network, which reflects the social, political, and economic rules that citizens of the state abide by.

Finally, if a nation-state is a cross-section of the IPE multiplex, this can account for the contingent, hard-to-predict, non-linear, non-equilibrium characteristics of politics at all levels of analysis. It may further help us to think about and understand why non-rational outcomes frequently prevail. That is, humans are social creatures prone to imitation and subject to ecological conditions that encourage competition. The ideas we construct to make sense of and survive our environment will be reflective of the structure of our relationships with one another (our social networks). Our ideas are endogenous to the power structures within our communities. Moreover, the NIA/PECI framework is not reliant nor dependent on the traditional, legal notion of the nation-state, which the constructivist perspective suggests may be fleeting. Should we change agreed-upon national boundaries or dissolve them altogether, or should they merely become irrelevant via the expansion of economic or political unions, the NIA/PECI framework can adapt and yet still continue to analyze the same human networks.

A brief example of how various paradigms address a fundamental question in international relations and political economy—the nature and origin of state power—illustrates the transformative potential of the NIA/PECI paradigm. For realists, hard power dominates international relations. For Liberals and Marxists, softer forms of power (economic, ideological) are just as critical. Constructivists emphasize the extent to which all forms of power and influence are socially conferred. But scholars who adopt the NIA/PECI framework see these distinctions as false and futile, as most thoughtful critiques of power analysis in international relations have pointed out (Barnett and Duvall 2005). More than just rejecting these distinctions, the NIA/PECI framework provides a comprehensive framework that simultaneously incorporates multiple types of power, which may be the closest we can come to finding a neutral meaning of the term.²³

Among the dozen articles and books on international relations and political economy published in the last 10 or 15 years that have used a network perspective to further advance our understanding of the sources and uses of national power, two particular articles published in the last five years, which adopt the NIA/PECI framework, stand out. Farrell and Newman (2019) and Winecoff (2015) both address the role of international economic interdependence in generating national power, and both articles use a network perspective to produce novel insights about state power. Winecoff (2015) maps the international banking system before and after the 2008 financial crisis and demonstrates how America’s importance in the

international banking system increased as result of a large international financial crisis that originated in the American economy. While for some realist scholars (Layne 2012; Kirshner 2014), the 2008 financial crisis represented the nail in American hegemony's coffin, and some liberal scholars speculated that America's power had merely globalized (Starrs 2013), Winecoff's analysis goes a step further than either of these perspectives to confirm an increase in American financial power, and to explain the structural and relational reasons for the increase despite the massively destabilizing event. According to Winecoff, the international banking network exhibits a high degree of centralization around the US, and dynamics of positive reinforcement and preferential attachment within the banking network serve to reinforce this network structure in times of financial uncertainty. This analysis implies the persistence of American financial power, a key determinant of broader economic and military power. Similarly, Farrell and Newman (2019) analyze the SWIFT and internet networks and conclude that these networks exhibit similar hierarchical asymmetries, such that a single or few countries are highly central in the networks. These structural asymmetries generate new forms of coercive power for states who occupy central hub positions within these networks and can utilize domestic institutions to leverage their position. Farrell and Newman's analysis demonstrates both the inadequacies of traditional liberal and realist notions of power state power, which tend to emphasize the decline of state power (especially American power) in the age of globalization, and the potential of the NIA/PECI paradigm to provide a more complete, robust, and accurate accounting of national power.

CONCLUSION

The evolution of IPE scholars' understanding of the nation-state may finally be coming full circle. With the unitary actor model, IPE scholars grappled with the reality that human evolution and advancement had finally reached the point where the problem of order, which has plagued isolated human communities since the dawn of civilization, needed to be solved at the global level. The unitary actor model tells us that as order develops at the global level, it will reflect the pattern of interconnectedness between the formerly isolated communities. With the development of the domestic political regime model of the nation-state, IPE scholars have realized that studying the relations between the formerly isolated communities is too simple an empirical strategy, since these communities are mini-systems themselves, and their behavior toward one another will be reflective of internal dynamics to some extent. With the constructivist model, we have come to understand the tenuous, socially constructed nature of the nation-state. We have grappled with the idea that human social behavior exhibits a level of complexity that is not well explained by theories built on assumptions of linearity and purely rational microfoundations. Moreover, we have contemplated the possibility that the nation-state may be a temporary distinction that we eventually dispose of as we learn to coordinate and cooperate with one another to efficiently and fairly allocate limited resources worldwide. The NIA/PECI paradigm promises to preserve these insights offered by past paradigms, and to help scholars put the role of the state into the proper context.

Thinking about the IPE as a multiplex allows IPE scholars to more accurately situate the nation-state within our theories. The nation-state is one important actor within the IPE

multiplex, but it is far from the only important one. Nation-states may function as unitary actors on some foreign policy matters, but on most issues, interest groups, individuals, and firms vie for influence and control. It is the complex, dynamic interplay between governments, firms, interest groups, and individuals that ultimately determines who gets what in the international economy.²⁴ The nation-state is best thought of as a cross-section of the IPE multiplex—one formerly isolated sociopolitical and economic network that is now increasingly embedded with others. For now, this cross-section remains an identifiable community, relevant to understanding broader, global dynamics. Analyzing the IPE as a multiplex will allow us to study the extent to which human interaction remains concentrated within tradition, legal boundaries, or instead, transforms into a single global society.²⁵

Thinking about the nation-state using the network framework of NIA/PECI may also have two other important benefits for the discipline. First, by acknowledging that causation in the international system can occur at both the micro- and macro-level, the NIA/PECI may provide a unifying analytical framework that advocates of all existing paradigms can endorse, allowing us to move beyond the levels of analysis debate and deconstruct the false dichotomy between comparative and international political economy.

Secondly, this NIA/PECI paradigm may help to build bridges between the American and British schools of IPE. While it is not clear that the British-American school distinction is really very important in the contemporary field of IPE (Seabrooke and Young 2017), these two traditions are historically well-established and still retain their own style of scholarship. While the American school is heavily dominated by scientific positivism and the application of quantitative empirical methods, which are not by any means uncommon among British IPE scholarship, the British school remains more analytically eclectic and does not readily eschew normative or critical IPE theory as quickly as adherents of the American school. This is perhaps attributable to the early influence of scholars such as Susan Strange and Robert Cox within the British school, who rejected ontological reductionism as readily as they rejected the oversimplified state-centric framework of early American IPE scholars (Cohen 2007). A core insight of complexity science is that complex systems are historically contingent—highly dependent on earlier states, and thus are very difficult, and sometimes impossible, to predict. This insight affirms the potential contribution of historical, qualitative research that evaluates the international system as a whole—principles that undergird much British school scholarship. By providing a unifying analytical framework that is not a “grand theory” in the traditional sense, in which analytical eclecticism can fruitfully operate at both levels of analysis while contributing to a common ontological end goal, the NIA/PECI framework may prove a unifying force that readily attracts converts on both sides of the pond.

As technological innovation and economic liberalization continue to drive economic globalization, the IPE grows increasingly complex. The PECI/NIA paradigm acknowledges this complexity and, in order to make sense of it, looks to incorporate knowledge produced by scientists who study other natural complex systems. Our job is immense. The IPE is composed of multiple layers of networks each composed of the most biologically complex species on the planet. But the NIA/PECI paradigm has the unique potential to incorporate some of the best insights of past research paradigms and to unify researchers who have been traditionally committed to different methods or analytical frameworks. In our attempt to untangle the complex webs of global human economic and political relations, and understand “the dynamic who gets what of the global economy” we would be remiss if we were to

adopt just one perspective on states' behavior. The NIA/PECI framework may help us keep focused on the larger, multi-dimensional picture.

NOTES

1. These paradigmatic distinctions exist most distinctly within the American school of IPE. The British school, to its credit, remains far more fluid, open-minded, and eclectic.
2. Bull and Watson (1984).
3. The Treaty did guarantee religious freedom to members of recognized protestant denominations but it did not grant religious freedom broadly.
4. As Osiander explains: "If the European system as a whole can be called a loose, informal regime with few institutions . . . the empire was essentially a more developed regime with more elaborate institutions, providing a system of governance for matters of common interest while leaving internal government to each of the participating actors individually. With the military strength of most estates of the empire negligible or indeed nonexistent, evidently their actorhood was exclusively ascriptive: based on rules, not power" (2001, 279).
5. See Kayaoglu (2010, 194).
6. For a detailed history of the evolution of OEP, see Winecoff (2017).
7. Wendt (2004) rejects state personhood arguing that such an approach is reductionist. Instead he promotes a more "robust" view of the state as a "superorganism."
8. Jervis's *System Effects* is really the first work to apply the concepts of complexity science to the field of IR.
9. Oatley's PECI approach is foreshadowed and shaped by the work of Art Jervis (1998) and George Modelski (1996), both of whom have previously applied complexity science insights to international politics.
10. Hence the explosive growth of the social network analysis with the political science discipline over the past two decades.
11. See Didier Sornette (2017) *Why Stock Markets Crash* and Mandelbrot and Hudson (2007) *The Misbehavior of Markets: A Fractal View of Financial Turbulence*.
12. See Taleb (2007).
13. See Andy Haldane's (2013) analysis of the banking system.
14. See Steryakov (2012).
15. The Lucas critique (1976).
16. See Oatley, Winecoff, Pennock, and Danzman 2013, who describe the international economy roughly in these terms.
17. Elineaor Ostrom's research (1990, 2009) has demonstrated this tendency.
18. See Clauset et al. (2009, 79), Clauset and Wiegel (2010, 179), and Johnson et al. (2013)'s assertion that a "simple mathematical law benchmarks human confrontations." Find similar mathematical properties of individual and collective violence in Zwetsloot (2018).
19. An argument made by most IPE scholars.
20. The political economy research on diffusion, epitomized by Simmons, Dobbin, and Garrett's (2006) and Elkins et al.'s (2006) research on liberalization and diffusion, lists four mechanisms of diffusion: coercion, competition, learning, and emulation.
21. See Winecoff (2015).

22. This is actually how most network analysis in IR an IPE is formatted. Maoz (2010) actually uses networks to rehash traditional realist hypotheses.
23. Guzzinni (2005, 496): "After a short preface on my take on constructivism, the following article exposes in some detail the results of such a threefold analysis. Turning to the classical focus of conceptual analysis on the meaning of power, the first section disputes the viability of attempts to find a neutral meaning across meta-theoretical divides. Although such attempts clarify thought, and would undoubtedly facilitate scientific communication, as well as and enhance our capacity for the construction of variables, the meaning of most central concepts in the social sciences is dependent on the theoretical or meta-theoretical context in which they are embedded. When applied to concepts in explanatory theories, this results in explanatory perspectivism. As long as we have to live with our metatheoretical dilemmas, such as the agency-structure debate, concepts cannot be neutral."
24. References a phrase coined by Susan Strange (1982).
25. See Wendt (2003) and Hedley Bull.

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CHAPTER 33

THE FIRM

GLENN MORGAN

INTRODUCTION

THE editors of this handbook define the essential insight from which International Political Economy (IPE) research develops in simple terms as the “recognition that politics mattered for economics and vice versa.” This is particularly obvious in relation to the concept of the firm. In the discipline of Economics, “the theory of the firm” describes a distinctive framework of ideas, concepts, and methods that seldom reach beyond the boundaries of the firm as an economic set of relationships responding efficiently to supply and demand signals emerging from the market, defined as an aggregated space in which individual sellers and buyers rationally pursue their own self-interests. By contrast, a political economy of the firm is concerned with the firm as a political actor engaging with states and markets in order to shape markets and favorable outcomes. The goal of this chapter is to focus on these processes at the level of the *international* political economy. It is therefore primarily concerned with multinational firms that engage in business outside their home base and seek to influence their home governments, host governments, and international regulators. They seek to influence their home governments to support their efforts to enter markets overseas and to establish overseas locations by exerting pressure on host governments as well as by seeking to shape the rules of international regulators in ways which favor their business model. Firms seek to influence host governments to be favorable to their foreign direct investment (FDI) and market access activities. Finally they seek to influence international organizations to develop regulations which are supportive of their activities. In terms of international political economy, therefore, the interest in multinational firms concerns what role they play in shaping markets and international economic regimes through their influence over states and international organizations.

Following the Editors’ Introduction, this chapter is interdisciplinary in scope. In particular, it seeks to draw on developments in the fields of International Business and Organization Studies, to produce a framework for the analysis of the role of firms in international political economy. Secondly, it follows a problem-driven approach in terms of “starting with an outcome of interest in the world.” This problem is how do firms influence the formation of markets and economic regimes in the international context and with what effects. The

chapter seeks to resolve this into a number of empirical themes about how different types of firms engage with home governments, host governments, and international organizations. This focus on types of firms opens up a wide agenda for empirical research in order to explore more deeply the role of firms in the international economic system.

The chapter therefore consists of the following sections. In the first section, the concept of the firm is subjected to scrutiny and two broad dimensions are drawn out which are important for understanding the relationship between firms, governments, and international organizations. The first dimension relates to issues of governance and ownership; it identifies four main types of firm in relation to these properties—shareholder-driven firms, hybrid firms with stakeholder input, family-owned and other types of personally-owned firms with limited financial transparency, and state-permeated firms. It is argued that these firms stand in different relationships to home and host governments and have differing effects on the international political economy. The second dimension relates to the business strategy of the firm and the central concerns about the international system which derive from the need to sustain rules and processes that support the firms' business strategies. In the following section, a number of ways in which firms exercise power and influence over home and host governments and over international regulatory organizations are discussed. The conclusion, following the argument of the Editors' Introduction, summarizes the overall argument and notes in particular the value of improving the dialogue between IPE and the fields of International Business and Organization Studies in order to establish appropriate problem-driven research questions.

THE FIRM AS AN ACTOR: OWNERSHIP AND ACCOUNTABILITY

What sort of an actor is a firm? Whilst there are a number of ways in which to distinguish the nature of a firm, in this chapter, two sets of distinction are identified. The first is in terms of how its ownership and governance structure impacts on the objectives and policies of the firm. Whitley, for example, states "the powers, duties and socio-economic functions of private companies continue to vary considerably between national jurisdictions and over time . . . In particular, the extent to which corporate entities combine legal personality, unified authority, limited liability, investor ownership and easy transfer of private property rights . . . is highly variable between differently organized market economies and subject to change as circumstances alter" (Whitley 2010, 454). The extensive literature on how firms are shaped by their institutional context has become increasingly important over recent decades. From the point of view of this chapter, a key part of this debate concerns how firms set their objectives and what this means for their international activity. Early discussions based on varieties of capitalism debates (Hall and Soskice 2001) examined this from the point of view of the distinction between shareholder-driven firms (as in liberal market economies) and those with a more stakeholder approach (as in coordinated market economies). Whilst shareholder-driven firms have strong similarities, there can be a wide variety of stakeholder type firms (see the discussion ranging from those with a strong role for labor, e.g., Germany, Denmark: for degrees of variation about how labor is brought into

this see Thelen 2014) to those where labor is excluded but various mechanisms ensure the firm considers other stakeholders such as suppliers and customers (as in Japan: Witt 2014). With its focus on national contexts, much of this research tended to pay little attention to the internationalization of firms. Efforts to extend this approach into an understanding of how multinationals from different institutional contexts change and develop as a result of the internationalization process have grown (see, e.g., Wood and Demirbag 2012). This strand of research rarely considers questions concerning the impact of multinational corporations (MNCs) on states or on the hard and soft rules embedded in the international economic order. It is also the case that it tends to ignore two other important forms of ownership which have attracted increased attention. The first is the growth of state-owned and/or state-directed MNCs, particularly from emerging economies (Bremmer 2010; Cuervo-Cazurra 2018; Cuervo-Cazurra et al. 2014; Mariotti and Marzano 2019; Mussachio and Lazzarini 2014). In much of this literature, the emphasis is placed on the fact that state-owned MNCs have different resources, governance principles, and objectives compared to privately-owned firms. They are able to draw on the financial resources and diplomatic weight of their home governments. The former feature means that they are therefore not subject in the same way to the disciplines of market competition as private companies. The latter means that if the firms face obstacles to market access or to other aspects of productive resources in a host country, then they may call on their home government to use its powers to force change. Obviously this is particularly relevant in relation to large powerful countries such as the US, China, Russia, etc. State-owned or state-directed firms upset the idea of a level playing field as their resources are not limited to market-based sources. Their key objectives may include sustaining employment and technological capacity in the home base and in overseas contexts acting as a surrogate arm of their home state in a variety of ways. Specific focus has increasingly been directed at Chinese MNCs and their connections to the state, either formally as state-owned corporations or through the indirect influence of the Chinese governments over private companies, leading to concerns that such companies are, in effect, agents of Chinese foreign policy. Nolke and colleagues have developed their existing theoretical extensions of the varieties of capitalism approach (Nolke and Vliegenthart 2009) into an increased focus on types of what they describe as ‘state-permeated capitalism’ in large emerging economies (Nolke et al. 2020) though they do not highlight the impact this has on the organization of the international political economy as opposed to its implications for forms of national capitalism (see, by contrast, Nolke and May 2018, who aim to combine a focus on national institutional contexts with an international political economy of the corporation).

Finally, it is important to take note of firms that are “private” in the strict sense that they are owned by a family or a small group of individuals and their degree of transparency is very limited. Such a model is not incompatible with stock market listing though it does require a high concentration of shares (or a golden share) being held by a single family, individual, or foundation. Outside shareholders have very limited influence on the actions of the owners of such firms. These firms may be highly active in bond markets and other financial markets as owners manipulate debt/equity ratios in order to maximize their personal wealth. Their vulnerability therefore lies in their capacity to continue to meet interest payments on loans which in turn depends on their ability to generate revenue. Protecting this revenue has often led family firms, particularly in emerging economies, to cultivate close links with particular politicians and parties, providing them with funds in return for measures such as creating a

protected home market, ensuring that licenses and privatization opportunities go primarily to these firms and exclude foreign multinationals.

It is also relevant to identify a second distinct group of private owners that exist in financial markets—private equity partnerships and hedge funds. Private equity’s model is to set up as a partnership group with minimal public transparency requirements and to borrow substantial long-term funds from institutional investors and elsewhere to provide baseline funding which they can use to leverage more funds in the financial markets in order to buy firms and restructure them. In the process they earn fees for managing investors’ money, use incoming revenue from sell-offs and existing operations, and from forms of financial manipulation to pay off loans (often at the expense of jobs and reinvestment) and increase their own ownership share prior to a sell-off when they can pay back their original investors and pocket for themselves the rest of the value of the firm. Hedge funds also draw on the funds of institutional investors and rich individuals but, rather than restructuring productive enterprises, they engage in maximizing advantages in financial markets by taking a range of calculated risks about movements in the markets. Hedge funds and private equity are minimally transparent in terms of their public disclosures of risk profiles and, unlike banks, do not have to meet capital requirements. Nevertheless, they are highly significant in the main financial markets as sources of price shifting and volatility. It is also important to note that both these ‘private’ forms of firms are significant contributors to a wider sphere of global financial institutions and transactions concerned with managing private wealth, shifting it around between assets and locations in order to minimize transparency and tax obligations (see, e.g., Harrington 2016; Seabrooke and Wigan 2014, 2017). Whilst large corporations also engage in many of these activities, it is within the private sphere of wealth management that individuals who gain from top executive salaries hold, protect, and grow their capital. The fact that many of these institutions have been able to protect themselves from detailed scrutiny by tax authorities and others has been crucial to the growth of inequality in recent decades and they provide the relay mechanism whereby corporate earnings translates into personal wealth.

Table 33.1 summarizes these arguments and suggests initial implications for the relationship between these types of firms and their home governments:

Table 33.1. Types of Firms and Impacts on Governance and Relations with Governments		
Type of firm	Governance and accountability	Relation to home government
Shareholder-driven firm listed on stock exchange: US, UK	Shareholders tightly connected to financial markets and short-term returns to shareholders	Mainly separate Influence on government through visible and invisible lobbying
Stakeholder firm listed on stock exchange: Germany, Denmark, Japan	Balance of stakeholders, institutional investors, banks, networks of suppliers and customers, local government, labor	Close connections between firms, business associations, and different levels of the state to facilitate coordination

Table 33.1. Continued

Type of firm	Governance and accountability	Relation to home government
State-permeated firm: China, Brazil, France	Various forms of connection to the state making them less vulnerable to market forces Direct ownership—majority, minority, golden share Indirect ownership through state banks Indirect influence through legal provisions, licensing for exports, etc.	Closely connected to the political interests of current controllers of the state. May become instruments of state policy Need to avoid antagonizing home government
Private firms with limited outside influence even where stock market-listed	Type 1: Firms predominantly owned by individuals, families, or foundations Type 2: Financial firms such as hedge funds and private equity—limited regulation and transparency	Type 1: Dependent on legal structure to allow golden shares, etc. May cultivate personal links with political parties to reinforce their goals on market access and competition regulation Type 2: Dependent on limited financial regulation: lobbies to resist this at home and in international contexts

THE FIRM AS AN ACTOR: SECTORS AND BUSINESS MODELS

It is also relevant to distinguish multinational firms according to the sorts of activities which are central to their business model fields (see Morgan 2018) and in particular how they relate to home governments, host governments, and international regulators. The term ‘multinational’ is often used as a synonym for manufacturing companies and the issues which face them regarding accessing foreign markets, making locational decisions, organizing global value chains, and managing the transfer of technologies, knowledge, managers, and financial resources across national jurisdictions. Manufacturing companies face very different challenges from other types of multinationals in terms of how they relate to home and host governments and to international regulatory systems. Multinational financial institutions, for example, are highly dependent on states to provide them with licenses to operate in various complex markets where the overall risk held by the firm may in theory be monitored and constrained by national and international regulators. However, taking action in time to reduce risk where assets and liabilities are spread across diverse classes, timescales, and locations proved impossible in the run up to the 2007–2008 global financial crash (Tooze 2018). The key aspects of relationships with governments and regulatory organizations for financial multinationals focus around the rules under which they can participate in markets.

A third group of important multinational firms are professional services firms particularly in management consultancy, accounting and auditing, and in law. The key role which these firms undertake is to provide advice and support to governments and other multinational firms in terms of the sorts of ideas, rules, and regulation they should follow. These professional services companies (like the key financial institutions) are invariably headquartered in the US, the UK, and a small number of European countries from where they have traditionally been closely connected to developing the infrastructure of modern capitalist organizations (Boussebaa and Morgan 2015). Over the last few decades and particularly since the collapse of the Soviet Union, they have become key vectors for the spread of neoliberal ideas into other locations, initially Eastern and Central Europe and later into China and other emerging economies. Their license to operate in other countries is based on their skills in mediating between firms and national and international financial markets and regulatory bodies. They knit together the global capitalist order, producing ideological justifications, technical instruments, and visions of the future that sustain existing power relations (see, e.g., Morgan 2018). Further, the social and cultural capital required to enter these organizations is derived from high prestige universities in the US and Europe; they are obligatory passage points into the top positions of these professional services multinationals, contributing to the creation of relatively homogeneous social groupings that work as networks of support and reinforcement for a particular sort of global meritocracy.

A fourth group of multinationals which knit together the capitalist world are the key technological innovators of recent years which are currently known as the FAANG group—Facebook, Amazon, Apple, Netflix, Google. These have been amongst the fastest growing multinationals of the last few years in terms of share price and turnover even though their workforces (other than Amazon) are relatively small, located mostly in a handful of the advanced economies and based on the platform services they provide which are used across the world (with China being the main exception). These firms have mostly had huge first mover advantages which they have protected by buying up potential rivals. They have therefore gained near monopoly positions in their fields. States have been relatively powerless to constrain them (Foroohar 2019). Their global footprint has expanded rapidly as the internet itself has become more reliable and more available across large parts of the world. Most governments are relatively powerless to shape this process even if they are concerned about a wide range of implications—though the European Union (EU) has sought to take action to reduce the monopoly power of some of the FAANGs and there has been some pressure in the US. On the other hand, the FAANGs have major financial resources which they can use to dispute and challenge court rulings. They also use these funds to employ highly professional and effective lobbyists in major jurisdictions to support their arguments to keep the internet deregulated or to legitimate the new working practices which firms like Amazon and Uber have introduced into unfavorable legal jurisdictions. It is also relevant that in many contexts, consumers have increasingly become used to the sorts of services provided by these companies and are therefore often reluctant to see them regulated (see, e.g., Culpepper and Thelen 2020), particularly when their lives have become increasingly dependent on these platforms during the COVID-19 pandemic. Questions about their responsibility for social media posts, for exploitation of workers in their supply chains, warehouses, and delivery systems, for maintaining privacy whilst using personal data for commercial purposes, for operating in ways to crush competition at an early stage, mean that the FAANGs have increasingly had to engage with governments and international regulators. These multinationals are also connected more broadly to other multinationals in telecommunications and in the

development of the powers of computers, the internet, and Big Data/Artificial Intelligence (AI). This in turn pulls them toward becoming part of a state's cyber security policy, as the US has accused China's Huawei, leading in turn to retaliation and the tightening of the link between the US state and some of the FAANGs and conversely the expulsion of most of the large US platform companies from China and their replacement by Chinese-owned corporations.

The final group of multinationals which can be distinguished are those that are engaged in mass agriculture, mineral extraction, and energy production. These groups have specific interests in the environmental policies and land ownership questions in particular countries, often in emerging economies (where their activities bring them up against indigenous communities) but also in some of the developed economies such as Canada and Australia. These groups often financially support governments and parties which are willing for them to continue with their existing business model. At the international level, they have been involved in seeking to undermine consensus on climate change and environmental damage by setting up alternative circles of experts that challenge the science behind these movements (see Leonard 2019 on the role of the Koch brothers in this process). They frequently require huge amounts of capital funding in order to support the building of large-scale projects such as mines, energy infrastructure systems like nuclear power stations, and massive dams on large rivers to create hydro power. This in turn leads them to look to governments to provide guarantees about the profitability of such major projects (e.g., in the UK, the government was desperate to find new sources of electricity in the early 2010s and therefore guaranteed EDF a certain strike price for the production of electricity from the nuclear power station which it is currently constructing. This is irrespective of how electricity prices may fluctuate in the future where it is now expected that due to the development of new solar and wind technologies these will substantially reduce). These multinationals interact much more frequently with governments than standard manufacturing multinationals and therefore employ a wide range of tactics and personnel in different countries to manage these processes. Much of their work is undertaken in emerging economies with weak democratic institutions, where corruption is a frequent way to approach local government elites at the expense of popular opposition. They also often appeal to their own home governments in the developed economies to engage in these processes to ensure that potential hosts in emerging economies open themselves up and grant contracts and licenses on profitable terms.

We summarize these types of firm in Table 33.2 and elaborate on a number of these points in the following sections:

Table 33.2. Types of Multinationals, Their Main International Activities, and Their Requirements from Governments and International Regulations

Types of multinationals	Main international activities and business models	Requirements and arenas for political action
Manufacturing	Organizing supply chains according to logic of labor costs, tax incentives, market access	WTO and/or bilateral/multilateral trade agreements

(continued)

Table 33.2. Continued

Types of multinationals	Main international activities and business models	Requirements and arenas for political action
Financial	Being present in wealthy locations to Hoover up savings and redistribute them as financial products	International regulations on nature of financial institutions and financial products National regulations Licenses to operate
Professional services	Being present in main centers of international economic activity with capacity to move temporarily into areas of market growth	Reputation as problem solvers in fast policy contexts, as reliable key intermediaries between financial markets and borrowers and as thought leaders. Strong networks with key decision-makers in governments and international bodies
FAANGs	Creation of global platforms for sales, advertising, and information without physical presence in each space Potential for monopolies	Consumers willing to participate in the platforms Governments that have little capacity to shut them down Weak international regulations
Agricultural, extractive, and energy	Large-scale investments that are disruptive for traditional methods of living	Governments that are willing to allow this disruption in part because of limited democratic institutions of protest and in part because of powers of corruption Global social movements and international soft law regulations that are not strong enough to overcome government inertia and corporate determination

**THE MULTINATIONAL FIRM AND
TYPES OF INFLUENCE OVER STATES AND
INTERNATIONAL ORGANIZATIONS**

This section considers how multinationals seek to exercise influence over governments and international organizations. In line with the previous discussions identifying different sorts of multinationals, this influence can be exercised in a variety of ways. In what follows these are categorized as using lobbying, using the structural power of capital, and using the power of expertise and ideas. These processes interact in various ways with forms of ownership and types of business model. It is not claimed that any particular influence or power method is the exclusive prerogative of one or other type of multinational.

Lobbying

As discussed previously, multinationals from all sectors are likely to engage in overt and covert lobbying with both home and host governments in order to improve the conditions under which they do business. One major difference in relation to home governments is how far this lobbying entails collective action on the part of firms or is characterized by the efforts of individual firms. In societies that have developed high-functioning business associations, these associations constitute social partners that are included in various governmental institutions and become part of the decision-making process. In relation to the earlier discussion, this reflects the hybrid nature of such firms in terms of their governance and accountability structures which may bring in worker representatives, local government officials, as well as shareholders. By contrast in societies characterized by shareholder-driven firms, collective action on the part of firms has declined and lobbying is much more focused on the specific business interests of the particular organization. In turn effectiveness of lobbying is based not so much on long-established relationships of partnership but more on the amount of money and resources that can be brought to bear on particular policies in the legislature or on particular elections, as in the US (Leonard 2019; Mayer 2016).

State-owned companies have more direct access to their home government through various mechanisms ranging from state-appointed managers and directors through to the oversight exercised by particular ministries. The intertwined interests of the state and the state-owned firms is reflected in the development of policies which support limited competition in the home market and the use of state resources and state diplomacy to open up other countries to the presence of the state-owned firm. In contexts, where family-owned private businesses have established strong ties with particular parties supporting them into government, similar mechanisms may occur that bring the state into a symbiotic relationship of support with particular firms and family owners (as has characterized the struggles in South Korea to criminalize corrupt relations between the leaders of chaebols and certain state politicians up to the level of president: Carney 2019).

In host countries, multinationals suffer from the 'liability of foreignness' which in this context refers to the lack of legitimacy that is accorded to such firms in terms of acting to influence a host government that represents the local population. Therefore such forms of direct lobbying have to be carefully justified. In some areas, such as regulating the rules of the market, it is much better from the point of view of multinationals if they can influence international regulators via their home government and these international bodies can engage with host governments to organize their markets in ways which are open to foreign multinationals. This is clearly reflected in the conditionality clauses that organizations such as the World Bank and the International Monetary Fund (IMF) have built in to their support to economies faced with financial crises, insisting on opening up economies to outside competitors, privatizing services, and establishing a stable macroeconomic environment and a balanced state budget. Business lobbying in the institutions and policies of the EU has also been strong, both as a means of shaping the rules of the EU itself but also as a way of forcing member states to follow a standard model of market deregulation, privatization, and balanced state budgets (Apeldoorn 2000). Multinationals may also engage in forms of collective action in host countries by creating foreign chambers of commerce but

these are more likely to offer networking possibilities than to be actively involved in local politics.

On the other hand, it is clear that large multinationals can often be highly influential in host country settings, even to the extent of fomenting rebellion and governmental change by sponsoring military coups, as US companies have done in Central and Latin America and in Asia. Notably, these multinationals have tended to come from the extractive and agricultural sectors. In terms of oil, companies have been fearful of the ease with which their assets could be nationalized by radical leaders and in the postwar decades this led to close relations between the companies and the home governments to protect these assets through supporting friendly governments and undermining parties and governments committed to nationalization. As oil and gas extraction has become more complicated, and new technologies have become required to maximize yields particularly in deepwater locations (e.g., Brazil) or other locations where the initial easy yield has dried up, host governments have tended to make deals with the big oil companies in order to access their expertise. State oil companies license the big private oil companies and the host of smaller expert subcontractors to extract the oil and then deals about distribution and sales overseas are drawn up.

The rise of state-owned multinationals has added a further complication to this picture. Many state-owned MNCs from countries like Brazil are primarily concerned with market access and earning foreign currency. Brazilian MNCs, which are predominantly in the extractive and agricultural sectors, have, for example, used cultural and linguistic connections to access Luso-African markets and their territories. The state and the main development bank in Brazil, BNDES, has played a major role in this by providing supporting finance to overseas governments such as Mozambique. Whilst these developments support Brazil's efforts to be recognized diplomatically as a rising power in the councils of the United Nations (UN), etc., other state-owned MNCs are more closely tied to the specific foreign and military policy objectives of their governments, for example in Russia and China. Ownership of Russian MNCs and the direct role of the state is often opaque but they have clearly used their powers over the delivery of energy fuels to the EU, the Ukraine, and other countries to pursue the diplomatic policies of the Kremlin. Chinese MNCs are more diverse, working across a variety of manufacturing and infrastructural industries though, again, the opacity of ownership means that any consideration of state influence has to go beyond direct ownership and consider the effects of more informal connections (as has been the case with Huawei which is, in theory, owned by its employees). In relation to Chinese MNCs, after a period of being welcomed into places like the UK during the first half of the 2010s, they are increasingly subject to suspicions about the risks of allowing such companies to take significant positions in key industries such as telecommunications, nuclear energy, and major infrastructural projects. Such a position potentially gives them (and the Chinese state) a permanent foothold over key sectors in host contexts. On the other hand, many emerging economies have welcomed the entry of Chinese MNCs, whether state-owned or not, as they have brought large-scale investment and the potential for technological and infrastructural upgrading in key areas of their economies (e.g., through the Belt and Road Project which, amongst other things, involves building major deep water ports along the coast of the Indian Ocean and supporting inland rail and road improvements).

The Structural Power of Capital

In relation to their influence over national governments, multinationals exert influence through their ability to provide a range of benefits to host governments and the dominant elites that compensate for the losses which they also inflict. Foreign direct investment generates employment, wages that are normally higher than those prevailing in the host country, increased tax revenues, and technology transfers and spin-offs through supply chain networks. For these reasons, over the last few decades, barriers to FDI have been reduced by governments and sets of incentives have been put in place to encourage MNCs to enter. Where governments challenge the interests of MNCs and business generally, then capital flight may occur. This is a major power in the hands of MNCs. Recent research has explored this “structural power” in more detail (Culpepper 2015; Culpepper and Reinke 2014; Fairfield 2015; Przeworski and Wallerstein 1988; Young et al. 2018). On the one hand, it emerges from the decisions taken by individual firms in response to their assessment of the broader political and economic environment. In these circumstances it is often financial multinationals that are the first movers in this as they have the ability to respond quickly, divesting themselves of shares, bonds, and currencies in countries where they foresee government action that threatens their interests. It is therefore often in the financial sector that local financial crises are precipitated in response to the potential election of radical governments proposing to control MNCs in some way (e.g., by increasing taxes, imposing more stringent labor and environmental standards, etc.). In response, overseas financial institutions respond in ways that have immediate effects on the population by the calling in of debts causing the collapse of loan and credit markets, interest rate increases, and currency depreciation. Manufacturing multinationals respond at a slower pace as they cannot liquidate their assets in the short term as financial institutions can do. Instead they engage in signaling, threatening not to renew investments or to withdraw from the country. Signaling can, however, be very effective in forcing politicians to change course as happened, for example, in Brazil when threats of economic collapse led Lula, the leading presidential candidate at the time, to water down his radical policies and promise to sustain macroeconomic stability (Campello 2015).

The structural power of capital (particularly in the form of manufacturing and financial firms) is a challenge for all governments not just in terms of forcing governments to follow business-friendly policies but also in the impact which the decisions of firms have on the regional distribution of production. The advent of global value chains reflects the ability of firms to move capital in a relatively unrestricted way to find locations that provide cheap labor, positive tax incentives, and weak regulatory regimes. This has led to the decline of many old manufacturing and extractive industries in Western countries which in turn has had a major impact on the politics and economics of these places, as revealed in the recent years of political turmoil and rising populism in many countries. Firms also exercise their power to pull places into the global division of labor, whether they want to come in or not. Most obviously this occurs in the extractive industries and the despoliation of areas of natural resources and the destruction of indigenous life styles in these areas. Governments become dependent on these processes to generate foreign currency, provide employment, and boost tax revenues, allowing for state expenditure. As a result localities often compete against each other to gain a foothold in the global value chain. Some localities might win

such competitions in the short term and find themselves temporarily pulled into global value chains, only at a later stage to find themselves abandoned as MNCs move on to other areas that appear cheaper or more suitable (described by Bair and Werner 2011, as a process of disarticulation).

The decisions of firms therefore give rise to a competition for investment amongst governments and has been associated with a “race to the bottom” in the sense that governments are encouraged to keep wages low, labor discipline high, regulations limited, and tax breaks high. Indirectly, therefore, the competition for MNCs weakens the power of labor. Global social movements seek to redress this balance by heightening consumer awareness and creating certification systems based on International Labour Organization (ILO) standards of fair labor. However, as studies show, as fast as such systems are devised, firms at various levels of the global value chain find new ways to take advantage of differences and the weaknesses of governments and other enforcement agencies (Bartley 2018; Piore and Schrank 2018). None of this is to deny that firms’ decisions to grow FDI has been associated with a massive increase in demand for labor in many emerging economies which has been satisfied by a shift of the working population from underpaid and subsistence agriculture to urban areas. Whilst the conditions under which the urban poor live are often barely above subsistence, and many other problems arise from this shift, the overall economic effect has been to move billions of people out of poverty and to create in places such as China a new huge middle class characterized by high consumption and by increasing levels of skills leading to rising wages amongst this group. It is also the case that the wealthiest elites in emerging economies have benefited enormously from the FDI decisions of MNCs which has allowed them many more opportunities to accumulate the sort of financial, social, and cultural capital that is transferable across societies and across generations and useful for sustaining the current system. However, the overall result has also been to increase the structural power of capital by making it more difficult for governments to challenge the actions of MNCs without potentially causing severe economic damage to their own population. This structural power does not need to be exercised actively; it is effective as an underpinning assumption to which governments must adjust. The process of adjustment depends on a range of factors including the degree to which host governments can operate countervailing power, as for example China, as a major economic power with an autocratic and politicized legal system, has been able to do.

The Power of Expertise, Ideas, and Thought Leadership

Multinational firms also exercise power through the expertise and knowledge which they possess. This is particularly important in relation to professional services firms (Boussebaa and Morgan 2015; see the extensive discussion in Seabrooke and Henriksen 2017). The following levels of influence and power can be identified. First, professional services firms are integral to the functioning of the state in advanced capitalist societies. When it comes to forming regulatory structures to make markets work (as discussed by Vogel 2018), insiders from professional service firms are recruited to governmental bodies as they are deemed to have the latest knowledge and understanding. “Regulatory capture” and the phenomenon of revolving doors where actors move from the private to the public sector and back again is a frequent part of this context as private sector expertise is embedded

into global organizations (Ban 2016; Kentikelenis and Seabrooke 2017; Kentikelenis and Babb 2019). More complex forms of influence arise in activist legal systems such as the US and the UK where specialist law firms engage in identifying new potential areas of activity for their clients that stay within the law whilst pushing at its boundaries, for example in issues of tax avoidance (Christensen and Hearson 2019; Palan 2003; Palan et al. 2010; Seabrooke and Wigan 2014, 2017). Such actions have fundamental effects on the fiscal position of states (removing much potential tax revenue). Professional service firms have also increasingly been employed by governments to carry out functions that were previously the domain of the civil service. As states have cut down their permanent workforce in response to deregulation, privatization, and austerity, they have left gaps that are filled by the private sector, see, for example, the massive amounts spent by the UK government on management consultancy advice during the COVID-19 pandemic and on employing large accounting and consultancy firms such as Deloitte to run the track and trace system. These same organizations have also been central to the diffusion of neoliberal practices to different parts of the world (Faulconbridge and Muzio 2015). Dezalay and Garth (2002) explored this phenomenon in relation to law firms entering Latin America in the 1970s and 1980s. As the Soviet system collapsed, these companies moved quickly into Eastern and Central Europe, offering advice and legal templates about how to privatize, how to build markets, how to create firm governance structures, etc. Halliday and Carruthers (2009) traced in detail how global norms about bankruptcy were spread by the actions of professional service firms into national legal jurisdictions. Professional service firms work alongside clients from other sectors such as finance, energy, IT, and the FAANGs to continually respond to political challenges to the legitimacy of their actions as well as identifying ways to change regulations and laws that open up for them new market opportunities.

These influences clearly reach into international regulatory bodies where close connections between industry insiders and the regulators is reflected in the role of these companies in supplying personnel, advice, and training. Recent work has explored these connections, emphasizing the inter-connectedness of expertise with social and cultural capital that facilitates the easy move between international professional services firms and international regulatory agencies (Cook et al. 2012). One key avenue for this consists of shared educational achievements across a small range of elite universities and business schools where alumni networks are created and reinforced (Spence et al. 2017). Another aspect is developing an international career from an early age by joining an international professional services firm or a key international institution and then moving over the course of time into other high-status firms or organizational positions (Spence et al. 2016). The appropriate social capital for such moves is variegated and research suggests that growing up in a wealthy, connected, and well-traveled family, supported by a high-quality private education coupled with achievement in high-status universities, provides a better basis for high-ranking professional roles than just attendance at a top school (Friedman and Laurison 2019).

These specific forms of influence are supplemented by what some large management consultancies describe as their 'thought leadership' role. McKinsey for example possesses a Global Institute which continuously produces high-quality reports on matters of current and future interest. It seeks to set the agenda for resolving key problems in ways which are coherent and consistent with the existing capabilities of their clients and with their goal of

generating new revenues by identifying forthcoming issues. Morgan et al. (2019) traced an example of this in relation to the climate change debate and showed how McKinsey sought to shape this agenda by producing a technical account of the different sources of CO₂ emissions. In their analysis, they identified deforestation as an area of policy action which could deliver relatively quick and significant decline in emissions if it could be discouraged by prohibition and by providing local populations with alternative ways of making a living. They were highly influential in developing what became the UN-REDD program which in turn opened up a new area of business for them and other consultancies in terms of measuring the benefits of stopping deforestation and dealing with the complex consequences of such policies for local and national governments. The capacity of professional services firms to recruit highly educated and skillful graduates, put them to work quickly on specific problems, and come up with solutions that they can promise to implement has made them increasingly indispensable to the running of international agencies and national governments even though the performance outcomes of many of their projects appear to be relatively low. Their claims to expertise and thought leadership make them powerful in relation to the policy-making of national governments and international regulators.

CONCLUSIONS

This chapter has sought to draw International Political Economy insights from International Business and Organization Studies. In these disciplines, the nature of the firm is examined in some detail and not treated as an unproblematic unitary category. Following that insight, the chapter first distinguished between firms in terms of their ownership and governance. Here the chapter emphasized the growing debate on the role of state-owned and state-influenced firms, particularly from China, in terms of their impact on the global political economy. It also noted the importance of family-owned and private firms which lack transparency and wider accountability. Traditionally, IPE has focused on firms as though they were all shareholder driven and profit maximizing but that position is modified by the continued existence of stakeholder firms from more coordinated forms of capitalism. It is also more fundamentally challenged by the rise of family-owned multinationals and privately-owned financial institutions as well as state-owned firms. State-owned firms raise issues as their role of carriers of the economic and political interests of their home governments arises. In the case of China, this has increasingly led to a backlash from a number of countries led by the US, though many emerging economies are quite willing to welcome Chinese state-led investment. These developments indicate that it is increasingly impossible to treat the international economy as predominantly about markets and competition between firms; it is a struggle between potential hegemonies and their allies about the rules under which the international economic regime operates.

In turn, however, this requires a more detailed understanding of firms in different sectors and how markets are constructed in these different sectors and the role which governments and international regulatory agencies undertake. In the chapter, a number of different sectors were identified and the nature of their internationalization, the problems which this generated, and the sort of political activities which they pursued was identified. This revealed that MNCs vary significantly in terms of the political arenas which are relevant to them and how they interact with them. Much research has examined the power of MNCs to lobby

home and host governments and international organizations to provide favorable market conditions. However the chapter also emphasizes two other aspects of this power—the structural power of capital and the power of expertise. The chapter suggests that manufacturing MNCs, together with the extractive industries and the FAANGs, are highly active in the lobbying arena. By contrast, financial MNCs are able to exercise power over governments by their ability to rapidly shift funds around the world and thereby disrupt economies at very short notice. Financial MNCs, some of which—such as hedge funds—are highly lacking in transparency, are central in creating financial crises that increase interest rates (and debt), weaken currencies and pull loans, thereby reducing investment. Professional services firms exercise power through the role of their expertise and the fact that that expertise is deemed essential by most governments and regulatory bodies. They have become essential sources of knowledge to regulators leading to ‘regulatory capture’ not just through their provision of knowledge and advice but also because their personnel frequently move into such bodies, leading to the revolving doors between these firms and the organizations that are supposed to regulate their firms. They have also been influential in resetting institutions and markets in countries that have undergone the disruption of economic collapse and political chaos. Further they are highly active in shaping future agendas through their thought leadership programs which define new problem areas and offer solutions that fit their business model and their ability to retain and make money from existing clients.

In conclusion, IPE can, as suggested by the editors, develop further by opening up to the insights of other disciplines such as International Business and Organization Studies. The concept of the firm is an ideal place where this can occur as it involves business strategies and governance, state policies, and the nature of markets and competition across borders. From this position, it is possible to generate the problem-driven model of research in IPE advocated by the editors. How do different types of firms exercise power and influence over governments and international regulators in order to shape markets in ways which suit their business models. By distinguishing between first, different types of governance and ownership and secondly, different types of business models, it becomes possible to consider these processes in more detail. The chapter suggests that influence is exercised through lobbying, through the structural power of capital and through the power of expertise and thought leadership, but how exactly this works in particular arenas can only be illuminated by empirical research that takes seriously the forms of variation identified in firms. Such an agenda for research offers the possibility of fertile inter-disciplinary connections, methodological pluralism, and relevant problem-driven outcomes.

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CHAPTER 34

INTEREST GROUPS AND ORDER IN GLOBAL ECONOMIC RELATIONS

IAIN OSGOOD

INTRODUCTION

WHY do countries close themselves off to global commerce? How can open and orderly economic relations among nations be established? The foundational literature in International Political Economy (IPE) pointed to the activities of organized and politically active *interest groups*—industry associations, issue groups, labor unions, and broader coalitions—in order to answer these questions. The vivid conflicts over trade policy witnessed in the nineteenth and twentieth centuries were the proving grounds for seminal contributions to interest group politics, both for IPE but also for political science as a whole (Schattschneider 1935; Krueger 1974; Rogowski 1989).

Interest group activity in the politics of global economic order remains fundamental. This is true across countries, issues areas, and over time, and so useful models of foreign economic relations must consider interest groups. That said, there is variation in the extent to which interest groups are mobilized across issues and polities, and much of this variation has yet to be described in comparative perspective. Significant opportunities remain to develop innovative explanations for where interest groups are more or less active. This is particularly so in examining where the benefits of working as a group are highest and where resource mobilization and political institutions make organization the hardest. Crucial to this enterprise is acknowledging the “group” in interest groups, and incorporating the costs, benefits, and challenges of collective organization more systematically into theories of interest group activity.

In turn, interest group activity can be used to explain global economic order. These explanations come in distinct varieties. Some scholarship focuses on the relative size or preference intensity of groups to explain outcomes while other work focuses on the ability of groups to get organized as the determinant of political success. Interest group scholars also examine how domestic and international political institutions shape whether pro- or anti-order groups win out. Each of these explanatory factors—preferences, organization,

domestic and international institutions—can be further divided into explanations which view these conditioning factors as fixed, or as variable products of the activity of interest groups themselves. I employ this schema below to review scholarship on how interest groups have contributed to order in global economic relations.

Since the early literature emphasizing interest groups, IPE has developed important traditions focusing on other factors, including mass interests and voting; state interests and grand strategy; and international organizations. My review of the recent literature on interest groups reveals how scholars in the interest group tradition have bridged these traditions by examining the important effects of interest groups on these other factors, and by investigating how these factors have in turn shaped interest group activity. Still, a focus on interest groups as the foundation of models of IPE remains a distinct perspective. It is also a vital one which continues to inform the field's most innovative scholarship.

WHAT ARE INTEREST GROUPS?

A focus on groups has deep roots in the study of politics, extending back to Aristotle on class; Adam Smith on guilds; the Federalist papers on faction; and Hegel and Friedrich Schlegel on organized associations ("corporations"). The term "interest group" emerges in foundational debates in American politics. Scholars emphasizing the contestation of groups (i.e. pluralism) or the dominance of well-funded and well-connected groups (elite pluralism) questioned the majoritarian character of American democracy. Several synonyms for "interest groups" emerged: pressure groups, special interests, lobbies, economic interests, organized interests, and, pejoratively, "big oil" and the like. A variety of European corporatist political philosophies, particularly fascist corporatism and post-war reinventions of corporatist political models, also placed groups at the center of the analysis and organization of politics. The politics of trade played a key role in both American and European accounts of groups, establishing IPE as a paradigmatic site for the importance of interest group politics (Pareto 1971; Schattschneider 1935; Krueger 1974).

The scope of the term "interest group" is an oft-noted conceptual challenge. At the broadest level, an interest group is any single entity, organization, or group of organizations seeking to influence public policy to any degree through means other than voting or holding office. This *broad definition* is juxtaposed with ordinary voters, who operate through the ballot box, and with political parties and movements, who actively seek control of the offices of state. While industry associations, peak business associations, and coalitions are clearly interest groups, this definition might also include a single corporation, a labor union, or a non-governmental organization (NGO) that is politically active. Most scholars of IPE employ this broad definition when they refer to "interest groups."

The breadth of this definition is useful but creates issues. It loses track of the "group" in "interest groups" which is unappealing semantically (how could a single corporation be a "group"?) and obscures key questions about the process and effects of forming groups to engage in politics. This definition also includes actors whose genesis lies entirely in a motive to influence policy alongside groups that have a totally separate origin outside of politics. Likewise, the definition includes groups that are mostly or entirely focused on politics, and entities that have only the most glancing interest in politics. Finally, "interest groups" and

its synonyms suggest different things in different contexts. Sometimes it means producer interests only; at others it refers to issue organizations with a strictly political purpose.

These considerations suggest a *narrow definition* of interest groups: any collectively organized group of pre-existing entities or organizations formed with a significant and on-going motive to influence public policy. This definition would exclude a single corporation or a voting bloc (not a group and not organized, respectively), and would exclude an industry association, labor union, or NGO if its activities were not importantly political in nature. I will use this narrow definition to focus on key issues in the first half of this chapter. That is, I investigate groups that are composed of other pre-existing actors that are deliberately organized; and, that are primarily or importantly motivated to influence policy, and which undertake sustained efforts to influence policy after creation. In the second half of the chapter, I use the more typical broad definition where I survey the literature. The most important point in this discussion is that conceptual clarity cannot be taken for granted when referring to “interest groups,” particularly in the study of IPE, where a very broad range of groups are implicated.

TYPES OF INTEREST GROUPS IN INTERNATIONAL POLITICAL ECONOMY

I illustrate this array of groups with examples from across issue areas, from different countries, and engaging in different types of political activities.

The largest part of the literature has focused on groups that are defined by their position in the economy. The study of groups of producers has focused on industry, trade, or professional associations. These groups represent the interests of member companies or of non-corporate producers like farmers or professions. These groups represent narrowly defined industry categories, and while many undertake significant political activity (including on IPE issues) their associations have many non-political purposes. As examples, the Korea Iron and Steel Association publicly advocated for the Korea-US Trade Agreement; the Associated Builders and Contractors has lobbied the US Congress on immigration issues and gives significant campaign contributions. CPA Australia (representing accountants) and the French General Association of Wheat Growers (AGPB) illustrate non-corporate producer associations. Broader business associations (also known as peak associations) represent entire sectors or categories of producers (e.g., all of agriculture or manufacturing; or all small businesses). They tend to be much more politically active. As examples, the Confederation of British Industry was prominent in opposing Brexit, while the Egyptian Businessmen’s Association meets regularly with Ministry of Trade and Industry officials on export promotion.

Groups also play a critical role in representing the interests of workers in the economy. Labor unions are often active in policy debates, though their primary mission is to bargain with employers. The United Auto Workers is represented on the Labor Advisory Committee for Trade Negotiations and Trade Policy; the Confederación Sindical de Comisiones Obreras in Catalonia joined in public protests and a general strike against the Spanish government after the 2017 independence referendum. In national political debates, labor unions

are often represented by broader confederations. The Swedish Trade Union Confederation recently announced a set of policy proposals on how to pursue globalization consistent with economic equality and support for human rights; India's Central Trade Unions Confederation opposed efforts to insert a "social clause" on labor rights in World Trade Organization (WTO) treaties in the 1990s. An apparently smaller role is played by groups representing consumers, for example, the Consumers Union of Japan (which has fought the introduction of GM foods) and the European Consumer Organisation (BEUC) (which has pushed hard to ensure that European Union (EU) trade agreements preserve consumer protections in the area of digital data and food safety). The most prominent "consumers" in contemporary debates on globalization are often organized groups of importing businesses, like the American Association of Exporters and Importers.

A second category of groups are defined by their positions on particular issues or classes of issues, rather than by a shared position in the economy. The politics of trade and foreign investment sees significant participation by pro-trade groups (e.g., in the US, the Emergency Committee for Trade and the Peterson Institute for International Economics) and groups opposed to globalization (Citizens' Trade Campaign; FairTrade America). Immigration policy has significant group activity in favor (e.g., in the EU, the Churches Commission for Migrants in Europe and the European Council on Refugees and Exiles) and opposed. Environmental groups, like the Canadian Institute for Environmental Law and Policy or Brazil's Amazon Institute of People and the Environment, have been highly mobilized on global trade and environmental cooperation. They are often joined by groups interested in food regulation or safety. IPE issues have also attracted the attention of groups interested in human, political, economic, and labor rights, as well as more general left-wing or right-wing mass organization groups.

The permanent membership groups with relatively broad remits above can be contrasted with temporary ad hoc coalitions which focus on one or a few issues and which tend to be composed themselves of pre-existing organizations and groups. These coalitions—for example, the Business Coalition for US-Central America Trade, which supported CAFTA-DR or a Citizens' Trade Campaign coalition of 1,500 progressive groups that opposed the Trans-Pacific Partnership (TPP)—are a highly regular feature of trade politics in the US. Climate and energy politics also has lively coalitional activity, although one with much longer-lived coalitions. The German Industry Initiative for Energy Efficiency (DENEFF) and French Entreprises pour l'Environnement (EpE) are national examples, though trans-national coalitions—the International Climate Change Partnership and the Global Investor Coalition on Climate Change—are also prominent.

A final category of interest groups is defined not by their place in the economy or by a specific issue position, but by their place in society. Identity groups representing cultures, ethnicities, or other communities are active in debates over globalization. Religious groups also regularly participate in global debates on trade, immigration, and the environment.

Two points are critical to summarize. First, the participation of groups in debates over international economic policy is ubiquitous and apparently fundamental. This is true across issue-areas, across countries, and over time although clearly there is variation across these dimensions. The importance of these groups has long been acknowledged by scholars, but it bears repeating. Second, a particular formation—the public, ad hoc coalition—is also apparently widespread, and merits close attention as a focal point of political contestation where all of these different varieties of groups join forces on issues of particular interest.

How Do Interest Groups Seek to Influence Politicians or Policy?

As the above examples suggest, interest groups undertake a variety of activities to influence politicians and policy. Hall and Deardorff (2006) classifies these as theories of exchange, persuasion, and work. In theories of exchange, interest groups trade some good that politicians want in exchange for moving policy in their preferred direction. These goods may include direct or indirect contributions to campaigns; votes (if they are controlled or influenced by the group); endorsements; offers of future employment or other benefits; bribes; and any other forms of political capital. Interest groups may also mobilize other groups (voters, other interests, other politicians) to work on their behalf.

Theories of persuasion argue that interest groups primarily influence policy by providing information. Interest groups arrange meetings with politicians or the bureaucracy (i.e., lobby) in order to describe their interests. They provide information to politicians, regulators, and courts which are seeking to gather facts or understand the likely effects of policy changes or judicial decisions (Woll and Artigas 2007). Interest groups may also provide work to politicians. Hall and Deardorff (2006) argue that politicians often lack the resources to effectively carry out an agenda which they already support. Interest groups then step in to help their allies design legislation and regulation; formulate political strategies and public campaigns; and anticipate objections and countervailing interests.

WHEN ARE GROUPS ACTIVE? BENEFITS, COSTS, AND CHALLENGES OF ORGANIZATION

Groups are active across issues, countries, and time as described above, but clearly groups are more active in some issues areas or polities than in others. In some cases, groups trade off activity with their unorganized constituent parts, for example, firms in an industry may take the lead on lobbying rather than their industry association. Here the core question for scholars is: What conditions make collective political action more likely than individual political action? In other cases, a lack of group activity might leave no special interest activity at all. The relevant question here is: Under what conditions is interest group organization impossible rendering a set of actors unrepresented? In this section, I outline explanations for political activity *as a group*, focusing on collective organization's particular benefits, costs, and challenges. Each of these factors helps to answer the two questions posed above.

The Benefits of Collective Organization

Groups are composed of underlying constituent parts: firms in an industry might form an association; NGOs and identity groups might form an issue coalition. Because the political activities of groups in principal could be undertaken by the constituent members, there must be some benefit to forming a larger collective. The literature describes several classes of explanations to explain the benefits of forming an interest group.

One set of explanations is that groups create political power that is greater than the sum of the constituent units' power. Forming a group often means creating an administrative apparatus which specializes in politics, a useful division of labor for constituent units (e.g., firms) that would prefer to focus on their core mission (e.g., manufacturing and selling). Groups can then take advantage of economies of scale in practicing political influence, which is particularly helpful when institutions and information are complex and costly to navigate (Dür and De Bièvre 2007). There are also benefits to coordinating political strategies: working together facilitates the optimal allocation of resources, and avoids duplication or cross-cutting strategies. Groups are natural sites for the pooling of information, arguments, social capital, and other resources. They create networks of social and political connections across a broad array of actors (Dreiling and Darves 2011).

Group formation may also allow constituent units to increase their bargaining power relative to policymakers or other groups. There are benefits to bargaining collectively with politicians, for example, creating a large group raises the costs of bargaining failure to politicians and defeats tactics of "divide and conquer." Groups that are authorized to speak authoritatively on behalf of their constituents also show politicians that there is weight behind some policy demand. The goal of group formation in this instance—as with issue coalitions—is to collect members and build the largest possible aggregate not only to threaten but also to inform politicians. Kollman (1998) also emphasizes information, but argues that creating groups serves as a costly signal to incompletely informed politicians that groups' members have strongly held preferences on an issue.

Group formation may also allow members to increase their power in society. Groups publicize issues of concern or develop policy positions to inform and influence other groups and mass audiences. They also develop ideologies, platforms, or talking points to influence public debate. Groups also develop networks (often of like-minded elites, but not necessarily) to spread the group's message. Group formation may have an expressive or affective function, too. Membership in a group can generate psychic benefits: action is being taken against perceived injustice. Groups also create and reaffirm social bonds, generating solidarity which may itself have material consequences in subsequent political conflicts.¹

Finally, it is important to note that the benefits of group formation are conditioned by the extent to which political institutions give groups a voice. Dür and De Bièvre (2007), for example, focuses on institutional opacity and complexity as potentially conducive to groups relative to diffuse or disorganized interests (Dür 2008; Poletti and De Bièvre 2014). Woll (2009) and Poletti, De Bièvre, and Hanegraaf (2016) represent competing accounts for how EU institutions have shaped the benefits of collective organization, and the consequent patterns in interest group activity. Though more focused on questions of representation, the long-running literature on plurality and proportional electoral institutions can also be used to understand the benefits of group organization (Rogowski 1987; Mansfield and Busch 1995; Kono 2009; Rickard 2010; Ehrlich 2011; Betz 2017). Institutions which favor "narrow interests" make forming a group more valuable.

Costs of Group Formation and Collective Organization

Forming a political interest group also has costs. At the practical level, creating a group requires paying for physical and organizational infrastructure. Successful staffing and

establishment of a clear plan of action take money and time. Groups also require investment in social activity, compromise, and coordination. Group formation, at least initially, may also reduce the speed and dexterity of response to political issues. It might be easier for a large corporation or NGO to respond to issues as they arise than to wait for a trade association or issue coalition to formulate a united response.

Scholars of IPE may find it productive to incorporate insights from social movement theory, which has long emphasized the importance of resource mobilization. Latent groups with access to financial resources, human capital (e.g., organizational experience), and strong affective bonds find it easier to pay the costs of organization (Hojnacki et al. 2012). Social movement theory has also focused on the role that political institutions play in shaping the costs of organization, a point which has also not received significant coverage in accounts of groups within IPE.

Equally fundamental as these questions of start-up costs, operations, and surrounding institutions, is whether potential group members agree on policy concerns in the first place. On issues of any complexity, group members must consider the compromises in their own ideal position that come with delegating representation to a larger collective. These concerns about positional autonomy have contributed to a significant line of literature in the political economy of trade which investigates whether industries will work together (through an association) or individually (as firms) on trade policy. One strand of the literature emphasizes that firms in an industry often don't agree on trade policy, whether because some firms export while others do not (Madeira 2016; Osgood 2017a), or because some firms source or produce abroad while others do not (Heron and Siles-Brügge 2012; Osgood 2017b; Yildirim et al. 2018). A separate line of argument holds that where products are differentiated, tariffs become targetable to individual firms (Gilligan 1997; Bombardini and Trebbi 2012; Kim 2017). In this context, collective action doesn't make sense because firms are fighting for their own private protection.

Similar arguments are employed in other issue-areas in IPE. Peters (2017) argues that the intensity of pro-immigration preferences among firms depends on their ability to offshore production. Preferences over the exchange rate also depend on firm-level features, including the mix of onshore and offshore activities in both production, financing, and asset ownership (Broz, Frieden and Weymouth 2008; Walter 2008). Firm-level targeting of policies also occurs with subsidies (Rickard 2018), trade disputes (Davis and Shirato 2007; Poletti and De Bièvre 2014; Brutger 2017), trade remedies (Jensen, Quinn, and Weymouth 2015), and much else, making these areas where collective action may be less valuable or take concentrated forms (De Bièvre and Dür 2005). A key intermediate question in understanding policy outcomes across IPE is therefore understanding whether industry-based groups are encouraged or discouraged by the structure of preferences or targetability of policies, and then whether individual firms or cross-industry coalitions emerge to fight policy battles.

The Challenges of Collective Action

Even where the benefits of collective action are high and the costs are low, successful organization is not guaranteed (Olson 1965). This is because those who don't contribute to a group's efforts often cannot be excluded from the benefits the group's efforts secure. For example, the benefits of trade protection or higher immigration might accrue to all of the firms in an industry whether they contribute to the industry association or not. Likewise, groups working on environmental issues might be tempted to skip paying the dues to a new coalition—though they support its mission—and devote that money instead to fundraising

or increasing membership. Where the benefits of some good are non-excludable and contributions are costly, potential group members will be sorely tempted to not contribute while hoping that others will, thereby securing benefits at no cost.

Olson (1965) discusses three conditions that are likely to affect the severity of this collective action problem. First, it argues that groups with fewer members find it easier to overcome free-riding. One possible explanation for this is that smaller groups lead to improved monitoring of contributions. In IPE, this idea led to investigation of the role of geographic concentration as a driver of monitoring costs (Schonhardt-Bailey 1991; Busch and Reinhardt 1999). Smaller numbers also means that each individual contribution has a higher marginal impact. A small number of large members may also be preferable to a large number of small members where individual costs of political action are fixed and political experience lowers costs of contribution. This idea contributes to a classic explanation for the ubiquity of trade protection—producers are few, consumers are many (Schattschneider 1935; Pareto 1971).

The second argument is that heterogeneously sized groups might be better able to organize because the few large members (grudgingly) accept the burdens of collective organization. In groups where members are equally sized, no such focal members exist to bear these costs. This idea has received investigation across the long-running literature on endogenous trade protection (e.g., Pincus 1975; Treffer 1993). Bombardini (2008) provides a recent formal and empirical investigation of industrial concentration's positive links with trade protection.

The final argument is on “selective benefits” which occur when groups provide not only public goods (which benefit members and non-members alike), but also excludable private benefits (which are limited to group members). Olson argued that groups that generate these “by-products” are able to cut down on free-riding. As an example, many trade associations generate a magazine and host conferences which serve to disseminate market intelligence and technical innovations. Coalitions of non-producer groups may also be sites for sharing information on organizational or political tactics. In both cases, potential members who don't join the group don't get these benefits. The ability to provide these selective benefits must vary across industries or issues, for example, and scholars ought to consider more systematically how such variation explains patterns in group politics across IPE.

So far I have examined the benefits, costs, and challenges of forming political interest groups. Elements in this literature have been widely investigated in IPE, for example, how institutions affect the benefits of forming groups; where disparate policy positions defeat group formation; and how the effects of group size and size distribution impact the ability to overcome the collective action problem. Other elements—on start-up costs; mobilizational assets; and selective benefits—merit more attention. I now turn to an investigation of special interests broadly defined: any non-voters and non-politicians that take an interest in politics. As described above, this broader definition is usually how the literature thinks of special interests. More importantly, it allows us to identify two fundamental questions that divide scholars of IPE into competing traditions.

INTEREST GROUPS AND GLOBAL ECONOMIC ORDER

The fundamental concern of IPE is to explore which actors, institutions, and forces contribute to order—openness, predictability, lawfulness—in global economic relations. This

motivating concern drives two fundamental questions which divide scholars of IPE into competing traditions. First, how important are interest groups as drivers of global economic (dis)order in comparison with voters, politicians, international institutions, and systemic factors? Competing answers to this question represent a fundamental dividing line across research programs in the field of IPE. Second, through what channels do special interest groups contribute to global order or disorder? For example, should we focus on interest groups' size and preference intensity, or their level of organization and political activity, or their interaction with domestic and international institutions? Describing the answers to this question provides a useful structure to survey the vast scholarship that places interest groups at the center of explanations for global economic order.

How Important Are Interest Groups for Global Order?

All scholars of IPE acknowledge that a vast array of actors (voters, politicians, bureaucrats, interest groups, identity groups, societies) and institutions (domestic, international, formal and informal, cultures) affect the quality and quantity of economic order. Different research traditions and literatures, however, place different weights on these forces and, even within narrowly defined topics, relative emphasis on special interests over other factors varies. Scholarship which emphasizes interest group politics therefore represents a particular view, and is part of a tradition within IPE that gives a central role to interest groups or, at the extreme, views interest groups as the prime movers in the study of international political economy. "How important are interest groups in IPE?" is not feasibly answered in a single research project. Instead, it's a question to which scholars develop a hunch after long exposure to the field. That being said, some ambitious work outside of IPE has tested competing claims on the importance of interest groups. Gilens and Page (2014) use a combination of data from surveys, interest group positions, and Congressional voting to test majoritarian and pluralist interpretations of American politics. They conclude that economic elites and elite interest groups drive public policy in the US. Work like this covering the issues of interest to scholars of IPE—and considering alternative theories arising from the study of economic and foreign policy—would be a valuable contribution to the literature.

The importance of interest groups is implicitly highlighted by work investigating the disinterest or inefficacy of the voting public. Guisinger (2009, 2017) argues that American voters do not prioritize trade issues in surveys or in voting behavior. Betz and Pond (2019) find no trace of consumer preferences on tariff rates, even in democratic countries. Indeed, the goods most heavily consumed by final consumers are more heavily taxed at the border, suggesting that consumers' preferences for free trade aren't a driving force, and that import-consuming producers' preferences are. These empirical findings on voters are complemented by a long-running body of theory which emphasizes interest groups' advantages—in terms of information, organization, and preference intensity—over ordinary voters or consumers (Stigler 1971). The primacy of special interests is also suspected *a priori* given the technical or distributionally complex nature of the policy choices across many issues in IPE.

Two points are key. First, complex social phenomena—like foreign economic policy or global economic institutions—have complex, multi-faceted causes. Scholars don't need to choose one actor or force to the neglect of others. However, it is useful to critically examine which actors or forces one sees as more or less impactful, and most scholars develop tendencies or biases around this question. Second, understanding the extent to

which particular bodies of work emphasize interest groups (or some other actor, institution, or force) is a valuable method to understand contending viewpoints within the field. It also provides a useful way to divide up debates in the discipline using root causes (public opinion, interest group activity, international institutions, international dynamics, systemic factors) rather than around substantive issues (trade, exchange rates, immigration, and capital flows).

Varieties of Interest Group Explanations for Economic Order

In this section, I provide a schema to categorize explanations for international order which feature interest groups. I break up these explanations into four broad categories on: the preferences of interest groups; the organization of interest groups; domestic political institutions; and international institutions. Each of these factors is treated as more or less fixed (i.e., exogenous) in some parts of the literature and as changeable and subject to interest group activity (endogenous) in other parts of the literature. For example, some work treats domestic political institutions as determined either at a prior point in time or for reasons unrelated to interest group influence. Other work argues that domestic political institutions are chosen due to the activity of interest groups or in order to favor some interests over others. Table 34.1 breaks down interest group explanations for order according

Table 34.1. Schema of Interest Group Explanations for Order

Factor affecting group influence	Factor is treated as: Exogenous	Endogenous
Group size, preferences, and preference intensity	Exogenous features of domestic or global economy shape size (and so influence) of interest groups. Exogenous features of industries or issues shape preference intensity of groups.	History, especially prior policy choices, drive current size and influence of interest groups. Preferences and beliefs are learned in a process of identity or cultural creation.
Group organization	Better-organized and more politically active groups are more likely to get the policies they want. Groups have natural homes in pre-existing parties, and party success means more power.	Group features drive ability to organize which contributes to political influence. The structure of group interests and organization influence party formation and success.
Domestic institutions	Pre-existing or exogenously determined institutions favor some groups over others.	Institutions are chosen to favor some groups over others, and groups influence institutional creation.
International institutions	Pre-determined institutional configurations favor some groups over others.	Both the choice and operation of international institutions is importantly driven by interest groups.

to these two dimensions, and then provides characteristic arguments. The literature which employs interest groups is so large that I endeavor only to illustrate this schema with an idiosyncratic set of work from across IPE.

Group Size, Preferences, and Preference Intensity

A vast scholarship in IPE describes and tests theories of the preferences of interest groups over foreign economic policy. In this tradition, we begin to understand policy outcomes by understanding the distributive consequences of policy changes which then map to preferences over policy. Of course, describing cleavages in society cannot in and of itself explain policy outcomes because knowing there are winners or losers from any policy change can't explain which side wins. Scholars in this tradition therefore have adopted two strategies to connect preferences to policy outcomes. First, comparative statics predictions about policy changes can be made by understanding how particular sides of a debate differ in size across time and space. Groups that are larger are presumably more likely to win policy debates. Second, scholars have assumed that policymakers weight both sides of a policy conflict in some proportion, and then consider how features of group preferences (especially preference intensity) contribute to changes in policy.

As an illustration of the former, the literature on exchange rate policy has examined the preferences of sectors and firms in detail. Frieden (2014, Ch. 5), for example, finds that economies with larger tradable sectors are more likely to have floating exchange rates, which can depreciate to improve the competitive position of both exporters and importers. Walter (2008) shows that firms' negative exposure to depreciation through foreign borrowing contributed to policy responses to the Asian financial crisis. This work shares in common a comparative static analysis across countries and time: as groups supporting one policy grow in size, that policy becomes more likely. A related approach in the literature is to focus on how essentially exogenous changes in the structure of the world economy have grown or shrunk demanders for particular policies. The large literature on global supply chains and interest group support for trade argues that exogenous increases in the feasibility of global production networks have contributed to freer trade by growing a constituency of pro-trade producers (Heron and Siles-Brugge 2012; Zeng, Sebold, and Lu 2020). In a similar way, Peters (2014) finds that growth in trade and FDI have shrunk the intensity of preference for liberal immigration policies.

The relative weight of interest groups is shaped over time by endogenous forces as well as exogenous ones which has led to a strand of literature which focuses on the historical legacies of policies as well as the dynamics of interest group strength over time. Brooks and Kurtz (2012) argues that Latin American countries with a strong Import Substitution Industrialization (ISI) tradition tended to create export-competitive manufacturing sectors supportive of (at least partial) capital account liberalization, owing to their greater needs for trade credit and capital investment. Hollyer (2010) argues that conditions associated with entering the EU are designed to unleash interest group dynamics, by weakening elite economic groups wedded to state intervention and strengthening new interest groups in favor of integration. Hathaway (1998) and Davis (2019) argue that trade liberalization weakens the long-run power of anti-trade interests by shrinking their economic (and so political) resource base. While this process may lock in liberalizations successfully achieved, it also increases resistance to liberalization in the first place.

Constructivist accounts of IPE also endogenize preferences and preference intensity by examining social dynamics (Abdelal, Blyth, and Parsons 2015). Kirshner (2003), for example, examines how social processes define the boundaries of legitimate economic policy by states, and so shape the decisions of interest groups on how to react to policy changes in the area of capital controls. Self-conscious processes of community building, for example, to resist global order in the area of trade, also exemplify sites where organizations build collective power but also learn and re-define collective preferences and beliefs (Von Bülow 2010).

The literature examined so far draws on comparative statics: variation in the preferences or size of interest groups over space or time explains variation in foreign economic policy. An alternative approach in the literature on special interests and trade argues that governments trade off the preferences of interest groups (usually producers) with those of other parties (usually consumers) according to some political weight. Governments then set policy to maximize the weighted utility of the two groups, so as to maximize the chance of re-election or some other political objective (Stigler 1971; Peltzman 1976). As an example, a government might raise a tariff to benefit an import-competing industry until the marginal political benefits of doing so are exceeded by the marginal political harms owing to higher prices for consumers (Hillman 1982).

This political weighting can appear *ad hoc*, and so a critical contribution of the canonical Grossman and Helpman (1994) model is to provide microfoundations for these weights. Governments seek to maximize a weighted contribution of consumer welfare and monetary campaign contributions by producer interests which are contingent on particular policy outcomes. It is shown that contributions that are truthful—that is proportional to gains earned by producers—represent an equilibrium outcome. The weighted maximization of consumer and producer surplus assumed in earlier literature can therefore be derived from the strategic interaction of those groups when campaign contributions are permitted.

With these weights explained, Grossman and Helpman (1994) make several predictions about trade policy, including that tariffs will be lower where consumers are more sensitive to price increases, and that contributing import-competing industries get positive tariffs while export-competing industries get export subsidies. In this way, comparative statics predictions about trade protection across industries are generated by examining how the direction and intensity of preferences of groups varies across those industries. This model has been tested and extended in a rich subsequent literature (e.g., Gawande and Bandyopadhyay 2000; Gawande, Krishna, and Olarreaga 2012), and the weighting of competing groups appears in much subsequent work as a foundation on which to build arguments about group interests and preference intensity.

Organization

Rather than focusing on the preference intensity or size of interest groups, a second branch of the literature explains policy outcomes by exploring which interest groups are better organized than their political competitors. A simple version of this emerges in the predictions of Grossman and Helpman (1994), wherein organized (i.e., contributing) import-competing industries earn a tariff as protection, while unorganized (non-contributing) industries face an import subsidy in equilibrium. A range of other literature similarly brackets the determinants of organization, and instead focuses on whether organized influence is being

practiced to understand policy outcomes. Broz (2008), for example, finds that the campaign contributions of interest groups (particularly banks) are among the key drivers of US Congressional voting to fund international financial institutions. Woll (2009) examines the trade-offs faced by firms as they consider where to exert their efforts within Europe's multi-level institutional structure.

Some literature focuses on the determinants of collective organization, and uses differences among groups or between interest groups and voters to understand policy outcomes. Most emblematic of this is the long-running literature on industrial political organization and trade protection described above. More recently, Osgood (2020) argues that the larger, pro-trade firms have advantages in collective organization owing to their small numbers and intense preferences. In comparison, anti-trade firms (who tend to be smaller and have less intense preferences) struggle with collective organization. Dür and De Bièvre (2007) highlight the challenges faced by issue groups (e.g., on the environment or human rights) in mobilizing to strengthen the environmental and human rights provisions of trade agreements. In comparison with producers, these groups face an enormous challenge in generating mass collective interest in trade agreements given the relatively diffuse set of interested parties and the small per-person effects of the agreements.

A final large branch of the literature examines political parties as vessels for interest group mobilization and influence. On this view, elections and the process of government formation determine which interest groups gain relative power over others. As an example, a simple model of the partisan politics of FDI suggests that parties of the right (representing capital) oppose inward foreign investment, while parties of the left (representing labor) support it. Pinto and Pinto (2008) complicate this, arguing that some capital flows might be complementary to labor and so supported by left-wing parties, for example, while others might substitute for labor, and so will be opposed. Similar patterns can hold for capital, too. A rich distributive politics in interaction with partisan success can then explain variation in openness to foreign investment across countries and over time (Pinto 2013). The monetary policy literature also emphasizes party-centered explanations of interest group success and failure (Bearce 2003; Mukherjee and Singer 2008).

The preferences and activities of interest groups have also been explored as drivers of party formation. Rogowski (1989) argues that societal cleavages over trade contributed importantly to defining the party maps of nineteenth-century Europe and the US, a point also examined in Hiscox (2002). The rise of new left- and right-wing parties with extreme attitudes on economic integration and immigration provides a useful opportunity for scholars to re-examine how interest groups and IPE issues shape the evolution of contemporary party systems.

Domestic Institutions

A third explanation for how interest groups contribute to order is that domestic political institutions determine whether pro-order or anti-order special interests win politically. Oftentimes, these institutions are assumed to be essentially exogenous to foreign economy policy. Ehrlich (2007), for example, argues that more access points for lobbying special interests—parties in government or the number of electoral districts—make it easier to lobby, and so conduce toward trade protectionism. A lively debate has examined whether

the structures of EU policymaking have tended to defeat or promote special interest influence, particularly in the area of trade (Woll 2009; Dür 2008).

The literature also examines endogenous institutions, that is, institutions which are chosen by politicians and their constituents in order to pursue particular ends. Bailey, Goldstein, and Weingast (1997) argue that the Reciprocal Trade Agreements Act of 1934—the progenitor of today’s “trade promotion authority” rules for passing trade treaties through the US Congress via simple majority and without amendment—was designed to serve the pro-trade interests of key Democratic constituencies. Subsequent literature has both rounded out and contested this claim. Goldstein and Gulotty (2014) argue that the delegation of authority to the executive branch facilitated the strategic targeting of protectionism to buy off special interests (and their Congressional representatives) and so avoid organized protectionist revolt. Examining the EU, De Bièvre and Dür (2005) similarly argue that delegation to specialized agencies serves to manage the complex demands of producer groups for both liberalization and protection.

International Institutions

A final set of interest group literature examines how these actors relate to international economic institutions. One branch of the literature looks at how international institutions reshape the activities and organization of interest groups. As an example, Davis (2004) argues that the “package” negotiation structure of most international trade negotiations activates export-oriented (or other pro-trade interests) to fight against protectionists clinging to trade protection. Poletti, De Bièvre, and Hanegraaff (2016) argue that a combination of EU and WTO institutions have led apparently broad peak associations in Europe to operate, in fact, as agents for the narrow interests of a few very large corporations. It is important to note that this work, and others in the tradition, don’t really view international institutions as exogenous, but rather as institutionalized through long-time practice or determined ahead of time in an initial stage of the negotiations. In this sense, IPE scholars rarely view international institutions as “exogenous,” but rather as (partly) fixed by prior arrangement.

Another branch of the literature examines more directly how interest groups have shaped the creation and expansion of international institutions. Baccini and Dür (2012) argue that the spread of preferential trade agreements is driven by competition for market share among export-oriented interests. Pelc (2011) finds that those same exporters drive the accession process to the WTO, and demand larger concessions from entrants with more tempting export markets. Moving beyond trade, Johns and Wellhausen (2016) argue that foreign firms may not push as hard for treaty protections through a Bilateral Investment Treaty (BIT) if they can fall back on links to politically-connected suppliers in host markets.

Interest groups also critically impact the operation of international institutions, not just their creation. Chaudoin (2014) highlights that the WTO’s decentralized system of enforcement relies on countries harmed by illegal barriers lodging a complaint, and pro-trade interests in defendant countries getting mobilized. Complainants may therefore strategically delay their complaints when elections are unfolding and unemployment is high, because pro-trade interests in defendant countries are likely to be less persuasive under those conditions. Yildirim et al. (2018) also highlights the role of interest groups in WTO enforcement, emphasizing that global production networks create new domestic constituencies

for compliance in defendant countries (though see also Ryu and Stone 2018). Other work has examined how interest groups impact the functioning of IMF lending (Mayer and Mourmouras 2008) and World Bank lending (Malik and Stone 2018).

CONCLUSION

So far I have described: the widespread activity of interest groups across issues in international economic relations; a set of factors which might explain variation across issues in the ability of groups to organize collectively; and, a way of organizing the interest group literature focused on the factors that explain which interest groups win or lose in the fight for global order. I conclude by briefly outlining one final question about interest groups and global economic order: Have the activities of interest groups mainly been constructive or destructive in establishing stable and open international commercial relations?

The early trade literature, exemplified by Schattschneider (1935), sees special interests as contributing primarily to disorder in global economic relations due to their organizational advantages over mass consumer interests and their tendency to compromise together to defeat foreign competition. This view leads to the conclusion that domestic institutions (like democracy) or international institutions (like the WTO) are needed to defeat parochial interest groups. As trade and foreign investment flourished in the second half of the twentieth century, scholars of special interest politics began to emphasize the positive contributions (at least on net) that interest groups make to openness and order (Milner 1989; Frieden 1991). The positive contribution of interest groups to order is especially vivid in times of economic or political turmoil, when mass interests might be tempted to turn away from the costs and challenges of global cooperation.

A third view is that some interest groups favor economic order and some oppose it, and that the net effects of interest group activity on global economic openness are roughly as positive as negative. Structural features of particular countries, industries, or issues might empower pro- or anti-order interest groups at different times but, overall, interest groups have contributed to as much conflict as cooperation. Another way of expressing this is that the effect of interest groups on order at any point in time is contingent on whether organization and institutions favor pro- or anti-order interests. Many contemporary scholars provide a fourth answer: interest groups don't push for order or disorder, but rather particular kinds of order and disorder that subtly suit their intentions (Gulotty 2020). Scholarship in this tradition sets aside generalizations, and focuses on the gritty detail of how global commercial relations are ordered and institutionalized. Interest groups have no general agenda vis-à-vis order, but rather hold electric and evolving mixes of policy wants that constantly remake the richly textured fabric of global economic relations.

As with other big debates in the field, no easy answer exists to the question of whether (and how) interest groups are good or bad for global economic order. It is, however, possible to confidently predict that interest groups will remain at the center of the continuing effort to understand international economic cooperation. One indicator of the present and future health of interest group scholarship in IPE is a continuing tradition of transatlantic engagement. This review piece has made reference to scholars at both North American and European universities (and beyond). Top journals and major conferences in the study

of IPE and interest groups likewise feature research on special interest politics from the US, EU, and other major economies. Scholars of the EU have been influenced by scholarship on American politics; scholarship on interest groups in the EU and the rest of Europe has contributed original perspectives, theories, and findings, in turn. One fact which has encouraged this exchange is relative agreement among special interests scholars on research questions, and appropriate methods and data. Continuing this fruitful exchange on the importance of interest groups across countries and political systems will contribute to the vitality of this long-running research program.

NOTE

1. Group formation may occur for reasons that are orthogonal to politics. Industries may form groups if there is a large need to share market intelligence or technological developments. Issue groups—e.g. on the environment—may form to share non-political information among members. Once created, however, these groups may find it easier to invest in political activities.

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CHAPTER 35

HOUSEHOLDS

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INTRODUCTION

If you study the interrelationship between public and private power in the allocation of resources in an increasingly globalized economy, then households surely matter. For many of those within the Organisation for Economic Co-operation and Development (OECD) region, residential property is the largest asset they own. However, homeownership now represents more than just a roof over one's head. Due in part to welfare retrenchment and reform, and in part due to state-stoked asset price inflation and financial innovation, homeownership can now also be a pension, a safety net, a way to store wealth, and is otherwise crucial to maximizing one's life chances. This shift, in turn, has demonstrably impacted political preferences, and housing now plays a deepening role in shaping state strategy in a global economy.

Through the associated mortgage debt, households also represent one of the major assets for many banks. Thanks to mortgage securitization and other related techniques, banks can repackage mortgages for secondary markets, which, among other outcomes, allows banks to make more loans and use households as a vehicle for financial speculation. In the US, these financial techniques were coupled with the exceptional borrowing capacity of sub-prime mortgage originators thanks to China's willingness to recycle US trade deficits. These were all crucial factors in the resulting asset bubble that triggered the 2008 global financial crash. More generally, however, this wider process connected people and their everyday lives to global capital flows in a way that is largely unprecedented, and put the ordinary pursuit of life chances and financial decisions at the center of the most serious financial crash in this generation.

Households therefore represent both assets and vehicles within the global economy. They can also be seen in similarly abstract terms, either as a unit of analysis in various types of surveys, or as a national accounting category. Households are however more than just an asset, vehicle, or unit. They are also institutions in and of themselves. The household is a site in which broader state and market relations are constituted. For the majority of individuals, access to income and wealth—and therefore wider decisions about consumption more broadly—are mediated through relations within households.

The household is also the key site of biological reproduction, reproduction of labor power, and the social practices connected to caring and provisioning of human needs—all of which are central to the constitution of global capitalism. As a wide body of literature pioneered by feminist streams of IPE has demonstrated, households both facilitate and are shaped by broader state and market relations, including the relations of production. Over recent decades, households—and especially women's unpaid labor within them—have become increasingly vital to the functioning of the global economy and accumulation of wealth, given their role in mediating the crises associated with the scaling back of welfare state provisioning and entitlements across many countries (cf. Bakker 2008; Bakker and Gill, 2003; Rai and Waylen 2014).

While we acknowledge that the household is also an important symbol within the intellectual heritage of economic thinking (Booth 1991; Watson 2005, 33–41) and a legitimating analogy in contemporary economic life (Stanley 2014; Brassett and Rethel 2015), this chapter focuses on the household as an actor within the global economy. We conceptualize the household in two ways. First of all, we look at the *external* relations of the household within the global economy, primarily focusing on residential property and the associated mortgages as a set of key assets for households and banks respectively. We review and evaluate the IPE literature on the housing-welfare trade-off, varieties of residential capitalism, and the role of mortgage securitization in global finance. Second, we look at the *internal* relations of the household within the global economy, primarily focusing on the household as a site of production and reproduction. We review and evaluate the IPE literature on unpaid domestic labor, social reproduction, and the character of capitalism.

In reviewing and evaluating this relevant literature, our argument is that households are not simply passive victims or a site that is merely subject to the vagaries of the global economy. Rather, households are a constitutive element of the global economy. To be sure, households do not possess the formal authority of the traditionally powerful actors within the global economy. Yet, in the absence of the aggregated individual-level financial mortgage transactions there would have been no global financial crisis in 2008, and the global labor force is to an extent dependent upon unpaid domestic labor inside the household for basic and continuing reproductive means. Although the household has traditionally been sidelined as an actor within the discipline, IPE scholars have been at the forefront of interdisciplinary debates about household in the global economy. Those approaches that explore the *interrelations* of the external and internal relations of the household in the allocation of public and private resources in the global economy are the most fruitful future direction for this literature.

HOUSEHOLD AS ASSET, HOUSEHOLD AS VEHICLE

The relationship between housing, welfare, and global finance is of increasing interest to IPE scholars. Within this literature, the household is typically conceptualized through its *external* relations as an asset for ordinary people to enhance their life chances, and as a vehicle for financial speculation and political and economic change. This section of the chapter reviews and briefly analyzes the core debates and claims from within this literature. In particular, it focuses on the “really big trade off” between housing and welfare and the

relationship between domestic housing systems and global capital flows. It then moves on to analyze the contributions of “Everyday IPE” as an approach that foregrounds households as actors in the global economy, attempts to analyze the interrelations between housing markets and the household as a site of social reproduction, and other innovations in the literature.

The debate within social policy between Jim Kemeny (2005) and Frank Castles (1998) on the “really big trade-off” between homeownership and welfare preferences has influenced the nascent IPE literature on the role of the household. At the core of this debate was an observation that high homeownership rates are usually found in countries with under-developed welfare states and smaller publicly funded pension systems. Although the causal accounts were and still are disputed, this initial observation has been refined into a resulting claim that the expansion of homeownership is normally accompanied by declining support for welfare states and for public pension systems. There is a growing literature that demonstrates how residential property in general, and home equity in particular, have helped fill this gap left by the erosion of tradition welfare services (Schwartz 2012; Montgomerie 2013). Homeownership may be fueled by the basic need for a secure and permanent roof over one’s head, but ownership transforms bricks and mortar into both a store of wealth and a potential reserve of cash. Homeownership therefore provides a way of redistributing income over the life-cycle in a way that mirrors welfare state provision—in particular, the public pension system (Kemeny 2005, 62).

Herman Schwartz (2008) and Matthew Watson (2008) have built on these initial insights to argue that rising importance of residential property in securing life chances is, respectively, “building conservative politics one brick at a time” in the US and “constituting monetary conservatives” in the UK. Homeownership provides an incentive to prioritize certain policies. As Schwartz argues, “[p]eople’s rational (logic of consequences) orientation toward homeownership, low inflation, and low taxes thus inevitably shifts into a more value-rational (logic of appropriateness) attachment to those things” (2008, 281). These claims have been strengthened through Ben Ansell’s (2014) large-scale quantitative analysis of the relationship between asset ownership and support for social spending in which he discovered similar dynamics.

Herman Schwartz and Leonard Seabrooke’s (2008) *Varieties of Residential Capitalism* (VoRC) framework and related special issue of *Comparative European Politics* is the most advanced and rigorous analysis of the role homeownership in IPE. Building on the insights from the “really big trade-off” debate, VoRC demonstrates the relationship between homeownership, national welfare systems, and global finance. Schwartz and Seabrooke base VoRC on the variables of owner-occupation rate (as a proxy for market exposure) and the average level of mortgage debt in relation to GDP (as a proxy for financial liberalization). By plotting how countries deviate from OECD averages on these two variables, VoRC develops four coherent and distinctive ideal types of residential capitalism: corporatist market, liberal market, statist-developmental, and familial. Despite the coherence and rigor of the framework, VoRC has its critics. In a recent intervention, Rodrigo Fernandez and Manuel B. Aalbers (2016) suggest that OECD housing systems are best characterized as following a common trajectory of increasing private debt levels rather than as diverging into different types.

Whether housing finance systems are diverging or converging, however, most scholars would nevertheless position mortgage securitization as one of the most important

connections between households and global finance. Securitization is a common and relatively simple method for repackaging a pool of collateral into a structured product for secondary markets. Mortgage securitization provides banks with a way to sell the stakes in the interest payment streams and therefore pass on risk to investors around the world, while simultaneously refreshing capital for more loans and facilitating a credit-fueled expansion of the housing markets (Schwartz and Seabrooke 2008, 249). Within the VoRC framework, the degree of mortgage securitization is the most important indicator of, and, to a lesser extent, causal mechanism underpinning, a financially liberal system, and is therefore apparent in both corporatist market types such as Denmark and liberal market types such as the US. In financially repressed housing systems without securitization, mortgages tend to be smaller because banks must carry the credit and interest rate risk (Schwartz and Seabrooke 2008, 254). In more liberal systems with securitization, there is more credit available and thus a larger housing market.

Mortgage securitization and associated complex financial techniques were at the center of the US sub-prime mortgage crash that triggered the 2008 global financial crisis (Aalbers 2008; Langley 2008; Schwartz 2009; MacKenzie 2011; Nelson and Katzenstein 2014). The creation of the US state-sponsored “sibling” institutions Fannie Mae, Freddie Mac, and Ginnie Mae provided large-scale support for sub-prime mortgage lending and securitization (Seabrooke 2006, 125–9; Thompson 2012). The “siblings” are the institution, and mortgage-backed securitization the mechanism, that link international credit markets to US households. In 2001, foreign holdings of their mortgage-backed securities amounted to \$133 billion, but by 2007 had exceeded \$1 trillion—with Japan and China accounting for the majority (Schwartz 2009, 104). Since the “securitization machine” hinged on the belief in continuously increasing home prices (Nelson and Katzenstein 2014, 374), the swathe of sub-prime defaults in 2006 burst the bubble. Given the scale of borrowing required to fund the securitization machine, the sub-prime panic was severe enough to cause a systemic crisis in global finance. As Schwartz (2012) demonstrates, it was only the liabilities and assets created via housing finance that were large enough to do so.

The sub-prime crash and 2008 global financial crisis have therefore brought the role of households in the global economy to the fore. For sure, the sub-prime crash was dependent on the aggregation of individual mortgage transactions. But were households just a passive actor in all this, swept along in a tide of irresponsible or even immoral lending practices, with sub-prime borrowers acting in a way that contradicted their true interests? While that particular argument was a key populist narrative in the fallout from the crash, some IPE scholars have taken this question seriously. In particular, the rise of “Everyday IPE,” pioneered by Hobson and Seabrooke (2007), is pivotal in unpacking the processes and subjectivities that constitute these sorts of borrowing practices. Both the “everyday life” and “everyday politics” strands of the approach have made important contributions regarding the role of the household in the global economy. We shall briefly review both.

Everyday politics places emphasis on the way in which the aggregated economic activities of day-to-day life can shape and transform the global economy (Hobson and Seabrooke 2009, 299–305). For instance, Seabrooke (2010) argues that Americans who took out sub-prime mortgages were not “stupid,” that is, acting irrationally or without intention. Echoing the insights of the VoRC literature, Seabrooke instead argues that those types of transactions make sense within the US system whereby there is a welfare trade-off steeped in social norms about maximizing life-chances and expectations about homeownership. These expectations,

in turn, help explain why the US state was so willing to intervene in enabling access to credit through, for example, the “siblings” institutions (see also Seabrooke 2006). Johnna Montgomerie (2009) echoes this argument in her analysis of non-mortgage household debt. She argues that the rise in US household unsecured debt does not resemble a long-term and secure replacement for real wage growth and state welfare, despite it being used as such. For both Seabrooke and Montgomerie, the rise of sub-prime borrowing was dependent upon this type of contextual and belief-driven action.

Everyday life, meanwhile, places emphasis on the way in which transnational financial networks are produced and reproduced in the practices and subjectivities of day-to-day economic activities—and vice versa (Hobson and Seabrooke 2009, 295–9). For instance, Paul Langley (2008) has argued that the growth in Anglo-American household borrowing was only possible through “qualitative transformations” in credit and mortgage networks. In particular, the boom in household borrowing is constituted through the re-assembly and re-making of everyday financial subjectivities in which “entrepreneurial subjects” routinely perform types of financial self-discipline. Global finance is then partly dependent upon “neo-liberal entrepreneurial financial subjectivities” (2008, 186). On the back of this, Langley argues that we can only make sense of contemporary global finance by exploring the “intersection between changes in the capital markets, on the one hand, and transformations in everyday spaces, practices and identities, on the other” (2008, 7). Contemporary finance is therefore performed in the routine practices of saving and borrowing of which households are central to. Taking both the “life” and “politics” strands of this literature together, the way in which everyday IPE encourages scholars to rethink the conventional top-down character of the discipline provided this approach with a considerable comparative advantage in explaining certain aspects of the sub-prime crash and 2008 financial crisis.

This literature on the relationship between households and global finance has been extended in a number of significant ways. For instance, Aalbers and Brett Christophers (2014) have critiqued the IPE literature on housing for an underdeveloped conception of capital. Acknowledging the contribution of VoRC, Aalbers and Christophers argue that there is no “coherent and relatively comprehensive conceptualization of the place of housing in the contemporary capitalist political economy, more generally” (2014, 374). Susanne Soederberg (2012, 217–40), meanwhile, has extended the focus on the link between households and global finance to cases outside of the Global North, specifically with a study on the links between debt and housing in Mexico.

The focus on residential property as an asset or as a vehicle configures the “household” in a particular way. By foregrounding the household’s external relations to global finance or as a way of offsetting relatively low welfare provision, the household itself can sometimes be black boxed. Adrienne Roberts’ (2013) work on the link between sub-prime mortgages and social reproduction is therefore a crucial innovation. Roberts argues that the IPE literature on sub-prime mortgages focuses on the global and macroeconomic level to the extent that it possesses two serious blind spots in its analysis of the household in the global economy. First, the rise in sub-prime mortgage debt is a result of the “reprivatisation of social reproduction” in the US. Rather than being used to maximize life chances per se, homeownership via a sub-prime mortgage is rather a necessary step for lower classes to meet their daily and long-term needs in an era of welfare retrenchment, declining social housing, privatization of healthcare, and other similar trends (see also LeBaron 2010, 905–6). Second, by foregrounding the relationship between sub-prime mortgage debt and social reproduction,

Roberts shows how increasing indebtedness is not gender neutral. Wealth and asset ownership already perpetuates gendered divisions and inequalities, and so the use of mortgage debt as a form of social provisioning has only intensified this state of affairs. By exploring the way in which the exterior relations of residential property markets interact with the interior relations of socially reproductive households, Roberts significantly advances the literature.

Montgomerie and Mirjam Büdenbender (2015) seek to further destabilize the conceptual category of the “household” that is frequently deployed within the IPE literature on housing finance. As they rightly point out, “households” are often equated with the “household sector” category that is used in national accounting and macroeconomics. As a result, terms such as “household debt” become drained of meaning. Montgomerie and Büdenbender therefore argue that the literature should focus on the interaction between the interior and exterior relations of the household. By doing so, they demonstrate that, if residential property is supposed to be used as a form of privatized social protection, then inequities and differences in wealth and income and the problematic assumption that residential housing is a static and unchanging asset-class, mean that so-called “asset-based welfare” policies are likely to fail.

HOUSEHOLD AS SITE, HOUSEHOLD AS MEDIATOR

Another literature within IPE has focused on the significance and value of the relationships, activities, and labor that occur *inside* of households. This body of work has been pioneered by feminist strands of political economy scholarship, which have long been centrally concerned with the role of households in constituting broader state and market relations, and in mediating the tensions between production and reproduction throughout various eras of capitalism (see, for instance, Mies 1988; Dalla Costa and James 1975; Rai and Waylen 2014; Bakker and Gill 2003; Bakker 2008; Elson 2012; Rai, Hoskyns, and Thomas 2013; Peterson 2005). This section of the chapter sets out the major insights and avenues of inquiry of the feminist literature on households, and in particular, highlights the importance of its observation that households operate as an actor, one which shapes and is shaped by wider relations of production and exchange.

Recent debates within the feminist IPE literature on households have focused on topics including: the role of households in mediating social and economic tensions linked to declining welfare state provision and the privatization of state assets, especially for women and low-income families (Bezanson and Luxton 2006; Bakker and Gill 2003); the impacts of labor market restructuring on women’s caring responsibilities (LeBaron 2015; Strauss 2013); the transnationalization of families (Safri and Graham 2010); and experiences of migrant domestic workers (Elias 2013; Farris 2014). These contemporary discussions build on several decades of feminist political economy scholarship on households, with roots in classical political economy texts on the family, households, and daily life, such as the writings of Friedrich Engels (1884).

Within feminist political economy, early avenues of inquiry revolved around the question of why and how women experience a lower social and economic position than men in capitalist society, including through unremunerated reproductive labor within households. The most significant contribution of the first wave of literature, beginning in the early 1970s,

was to analyze the role and value of women's unpaid labor within broader relations of production, and to challenge the gender-blindness of non-feminist political economy scholarship. Scholars like Margaret Benston, Silvia Federici, Lise Vogel, and Maria Mies criticized non-feminist political economy—and especially Marxist streams of thought—for omitting gender and households from its scope. In particular, they challenged non-feminist political economy's narrow focus on exchange and the exclusion of reproductive activities, their abstraction of the market from broader patterns of social determination, and depiction of the household and family as harmonious institutions (Benston 1969; Federici 1975; Vogel 1983; Mies 1988). A key point of contention in these early debates was whether capitalism should be understood as a single system, encompassing both production and reproduction (cf. Young 1981; Seccombe 1974), or whether patriarchy (as a mode of reproduction) and capitalism (as a mode of production) comprised two separate systems of domination that operate in relation to each other (cf. Hartmann 1979). In other words, whether capitalism is inherently dependent on gender-based forms of oppression and reproduction, especially within the household, or could exist—at least in theory—without it. These early debates were highly theoretical and characterized by formal abstraction.

Subsequent waves of scholarship delved deeper into the role of households in mediating capitalism's tension between market imperatives and progressive forms of reproduction for the majority of the population. Throughout the 1980s and 1990s, as states like Canada, the US, and the UK rolled back state-based entitlements and provisioning typical of the Keynesian era, scholars documented the disproportionate impact that these social policy shifts had on women and households (Bakker 1994, 1996; Corman and Luxton 2001). They argued that government cuts to spending on education, healthcare, childcare, and welfare programs (such as food assistance) were re-privatizing social and community-based forms of reproductive labor, and necessitating that individuals (who were usually and disproportionately women) perform greater amounts of unpaid labor. At the same time, the “erasure” of gender from social and fiscal policy was eroding further sources of financial and social support for women, and especially those with lower incomes, reinforcing and deepening their inequality vis-à-vis men (cf. Brodie and Bakker 2007).

IPE scholars like Isabella Bakker and Stephen Gill observed that such shifts ultimately deepened and reinforced gender-based inequality in and outside of households (2003; see also, Bakker and Silvey 2008). With the decline of the post-Second World War “male breadwinner model,” women's workforce participation increased in many countries as they sought to keep themselves and their families afloat and manage the contradictions of neo-liberal restructuring (Vosko 2005). Yet, women did not experience the same labor market opportunities as men. Rather, as several national and cross-national studies by scholars in IPE and overlapping disciplines like gender studies, sociology, and economics have demonstrated, women have tended to be concentrated in precarious and part-time work, in part because of the need to juggle paid labor with unpaid responsibilities for children, the elderly, and the household (cf. Fudge and Owens 2006; US Department of Labor 2011; Kalleberg 2009; Pearson, Danloy, and Razavi 2004). Simply put, the key insight of this body of scholarship has been that state restructuring has increased women's responsibilities within households and as well as their need to take on paid employment, and at the same time, has integrated women into the labor market on inequitable terms since “the gender gap in unpaid care work has [had] significant implications for women's ability to actively take part in the labour market and the type/quality of employment opportunities available to

them” (Ferrant et al. 2014). While still highlighting several theoretical concerns that surfaced in the early feminist debates on households, this wave of scholarship was much more empirical as well as policy-oriented, and has been formative in national public policy debates about gender mainstreaming, national gender budgeting, and other issues.

Scholarship analyzing women’s unpaid household labor, and investigating the impact of such responsibilities on their labor market participation, have informed and overlapped with more theoretically oriented debates about the nature and character of “social reproduction” in IPE. Drawing on and synthesizing across earlier scholarship, including the work of Antonella Picchio, Diane Elson, Isabella Bakker, and Catherine Hoskyns, the concept of social reproduction was notably defined by Isabella Bakker and Stephen Gill (2003) as encompassing biological reproduction, the reproduction of labor power, and the social practices connected to caring and provisioning of human needs. Articulated slightly differently by Shirin Rai and Kate Bedford, the concept has been used by feminist political economists to describe, “first, biological reproduction, the production of future labor, and the provision of sexual, emotional, and affective services (such as are required to maintain family and intimate relationships); second, unpaid production of both goods and services in the home, particularly goods and services of care, as well as social provisioning . . . ; and third, the reproduction of culture and ideology, which stabilizes dominant social relations” (2010, 7). The publication of key works such as Bakker and Gill’s *Power, Production, and Social Reproduction: Human In/Security in the Global Political Economy* (2003) sparked and catalyzed debates amongst feminist political economists regarding the role and value of social reproduction to broader processes, institutions, and relations associated with changing relations of production, and the role of states in mediating these relations (see also Kate and Rai 2010; Steans and Tepe 2010).

This new body of scholarship on social reproduction that has arisen over the past decade is pathbreaking for a number of reasons. First, the literature consistently stresses the mutually constitutive relationship between social reproduction and production, arguing that households are not simply passive agents *responding* to market and state restructuring, but rather, that households shape and condition the possibilities for that restructuring to occur in the first place. To date, authors have developed this claim through persuasive analysis of changing household consumption and ownership patterns, and reconfigurations of household members’ divisions of labor and labor market participation (Kate and Rai 2010; Steans and Tepe 2010; Bakker and Gill 2003; Roberts 2013; Elias and Roberts 2016). Although the claim that there is a mutually constitutive relationship between relations of production and social reproduction is not wholly new, previous streams of feminist political economy have tended to prioritize and emphasize the analysis of how productive relations structure (in the sense of defining what is possible) social reproduction, rather than vice versa. The burgeoning literature on social reproduction departs from this emphasis, demonstrating the centrality and importance of households as actors that actually facilitate prevailing models of capital accumulation rather than merely responding to them.

Two recent special journal issues on social reproduction, both published in 2010—*Signs*, edited by Shirin Rai and Kate Bedford, and *Review of International Political Economy*, edited by Jill Steans and Daniela Tepe—have been important in capturing and analyzing such links. Furthermore, these two special issues are also emblematic of a second key contribution of this emerging body of work in feminist IPE—a new and innovative combination of theoretical and empirical inquiry. As noted earlier in this section, early scholarship on the role

of households and gender inequality in shaping and mediating the relations of production was highly abstract and theoretical. Subsequent literatures were geared toward public policy formation, and turned away from the macro theoretical questions that animated early debates, and toward empirical investigation of the impact of welfare restructuring on women and households, women's labor market participation, and the social and economic value of women's unpaid labor. While this body of work fed into and shaped national policy of countries like Canada, the UK, and the US, in part facilitated by close links between feminist economists and advocacy groups like the Women's Budget Group, it was less attentive to advancing IPE debates and scholarship. The burgeoning literature on social reproduction in the global economy is harnessing the insights of both theoretical work investigating the relationship between contemporary capitalism and gendered relations of social reproduction, and more empirical scholarship, which was previously visible in policy debates but less integrated into IPE scholarship (see, for instance, Elias and Roberts 2016; Roberts 2013; Rai and Waylen 2014; Safri and Graham 2010). Combining macroeconomic studies and micro-level research (conducted through techniques including ethnography and interviews) to investigate the shifting relations of production and social reproduction in national economies and the global economy, this literature more broadly is making in-roads toward theoretically informed, empirically grounded, political economy analysis of the household as a key actor within international political economy.

CONCLUSION

Writing in 2009, Wesley Widmaier (2009, 947) suggested the following: "Fifteen years ago, an article or book suggesting that US subprime mortgage standards might have implications for the health of the global economy would have likely to have encountered considerable difficulty in getting published in international relations (IR) or IPE journals." Five years later, Stephen C. Nelson and Peter Katzenstein's (2014) article in *International Organization*, in which the sub-prime crash is meticulously unpacked through the frame of uncertainty and risk, has vindicated this claim. No doubt, the sub-prime crash and 2008 global financial crisis have brought the role of households in the global economy to the fore, and have sparked deeper interest amongst IPE scholars in the pivotal role of households within the global economy. There is now a well-developed IPE literature on the interaction between housing finance and international credit.

But, as we have argued in this chapter, the discovery of households as an actor in IPE is not a wholly new one, nor is the role of households restricted to their involvement in the sub-prime crash. As our overview of the literature makes clear, households have featured within IPE scholarship for several decades. Two bodies of work provide especially important insights, with one analyzing contemporary households' *external* relations as an asset and vehicle for wealth accumulation, and the other focused on households' *internal* relations as the site in which social reproduction occurs and mediates the contradictions and tensions of production. Scholarship investigating the links between households and broader national and global dynamics of political economy appears to be gaining traction in IPE, and articles focused around household assets, dynamics, and labor are becoming less rare in key journals like *Review of International Political Economy* and *International Organization* than

they used to be. As such, there is reason to be optimistic that IPE is overcoming its historical tendency to sideline households in favor of prioritizing powerful actors like corporations, international organizations, and states.

As we have argued throughout this chapter, the literature on households is progressing, with several promising avenues of inquiry. Meaningful progress will hinge on our ability to connect households' internal and external relations (see also Steans and Tepe 2010). A cutting edge literature is beginning to make significant progress in addressing this shortcoming by linking social reproduction to financial and labor markets and transnational governance. Recent studies have investigated the role of debt and credit in enabling the privatization and individualization of social reproduction (Roberts 2013; Montgomerie 2009), the power of international organizations to regulate social reproduction and households (Ruckert 2010; Elias 2010), and the impact of inequitable divisions of household labor on women's paid labor market participation (Fudge and Strauss 2013). In our opinion, the strongest work in the field lies in this burgeoning literature, and more broadly, in approaches that explore the *interrelations* of the external and internal relations of the household in the allocation of public and private resources in the global economy. Both theoretically driven and empirically grounded, this burgeoning literature comprises the most fruitful future direction for IPE scholarship on households.

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CHAPTER 36

NON-GOVERNMENTAL ORGANIZATIONS

ELIZABETH BLOODGOOD

THE study of non-governmental organizations (NGOs, often international NGOs, or INGOs) has largely escaped the pitfalls of potentially unproductive debates between problem-solving and paradigm-centric approaches in International Relations (IR) and International Political Economy (IPE). The main paradigms generally rejected NGOs as significant actors, dismissing them as epiphenomenal (Mearsheimer 1994) or relegating them to subservient positions following states' leads (Feld 1972; Skjelsbaek 1971; Charnovitz 1996; Raustiala 1997). NGO scholars themselves are divided on the *modus operandi* of NGOs. Some scholars view NGOs as organizations created to solve problems, often classified as market failures, caused by the lack of economic incentives to provide a service to some population, including political representation of minority political positions (Johnson and Prakash 2007; Weisbrod 2009; Weiss and Gordenker 1996). Alternatively, NGOs were seen to transcend political boundaries and market forces, providing the "moral consciousness of the world" as global civil society and principled transnational activist networks (Willets 1996; Keck and Sikkink 1998). While public administration, economics, and management worked on the market failure angle, early IR scholarship concentrated on the transnational activist network approach. Academic work on NGOs as global civil society tended to take a more critical or sociological bent (Boli and Thomas 1997; Clark et al. 1998; Scholte 2000). This has expanded into work that examines the role of NGOs in expanding participation and democratic representation within global governance (Held 1995; Keohane and Nye 1998; Nanz and Steffek 2004; Bäckstrand 2006) and has raised issues of legitimacy and representation in the face of globalization (Lipschutz 1992; Wapner 1996; Finger and Princen 2013). A second group of IR scholars focused on NGOs as principled actors challenging the self-interested behavior of strategic states (Keck and Sikkink 1998; Brysk 1993; Clark 1995; Raustiala 1997). This strand of literature is closer to a problem-solving approach, as these authors sought an explanation for why states act counter to their self-interest (and the expectations of realism and liberalism). NGO scholars found explanations for this puzzle in the creation and institutionalization of norms. NGOs are argued to have a central role in propagating norms and pressuring states from above and below to act as if they were norm-abiding (Risse, Ropp, and Sikkink 1999; Price 1998; Cortell and Davis 1996).

THE STATE OF NGO RESEARCH IN IPE AND BEYOND

The literature on NGOs in IR has shifted from showing that NGOs matter to examining how, when, and why they matter. Key factors under consideration include where NGOs are located, how they are organized, and the effects of their internal and external structures on their activities and influence. Growing concerns about the rising numbers and economic power of NGOs, as well as public scandals, have also brought scholars to examine the dark side of NGOs.

Initially scholars invested large amounts of time and effort into establishing that NGOs had played important roles in producing policy outcomes contrary to what would have been expected according to states' interests. A combination of edited volumes, which presented overviews of the range of NGOs active in international politics, and their different roles in international events, were published in combination with single case studies that meticulously demonstrated the impact of individual NGOs on international outcomes across a range of policy fields (Florini 2000; Risse-Kappen 1995; Charnovitz 1996; Wapner 1996; Evangelista 2002; Josselin and Wallace 2001; Arts et al. 2001; Hall and Biersteker 2002). These generally descriptive works helped to establish the importance of NGOs, but lacked a systematic theoretical explanation of how and when these organizations could and would be influential.

The continued growth of NGOs throughout the 1980s and 1990s (Reimann 2006; Büthe 2004) led to a newer wave of literature that sought to answer important theoretical questions about NGOs' activities and influence. The boomerang model (Keck and Sikkink 1998) posited that NGOs exert influence by putting external pressure on resistant states for normative change using a combination of norms, information, and alliances with other powerful actors. Brysk (1993), Clark (1995), and Meyer (1997) each elaborated on aspects of this pincer-like strategy for applying normative pressure (rooted in NGO information) for changes in international norms, domestic policy, and state behavior. The spiral model of norm adoption, used to explain how states move from norm-resistant to norm-acceptant, and even advocates, provided a more detailed account of the interplay between states and NGOs as norm entrepreneurs (Risse, Ropp, and Sikkink 1999; Finnemore and Sikkink 1998). While the boomerang and spiral models were most commonly applied to human rights cases, the idea of private actors establishing standards for state behavior quickly spread to economic and environmental realms as well. A third set of literature examined private (non-state) actors ability to set standards for their own behavior, including NGO and firm self-regulation, in an effort to pre-empt state control and with the result of potentially reducing or circumventing state authority (Cutler et al. 1999; Hall and Biersteker 2002; Cashore 2002; Abbott and Snidal 2009; Büthe and Mattli 2011; Potoski and Prakash 2005; Avant et al. 2010; Prakash and Gugerty 2010b). More recent work on public-private partnerships (PPP) between governments and private actors, both corporations and NGOs, have provided new impetus to understanding the mechanisms by which NGOs can not only change standards (both legal and normative) for other actors' behaviors, but also how they can help to implement these standards. PPP (alternatively called multi-stakeholder initiatives and transnational partnerships (Hale and Held 2011; Dingwerth 2008) enable

NGOs to produce concrete effects on outcomes for climate change, marine management, finance, and mining, to name just a few (Green 2013; Petersson 2019; Buthe and Mattli 2011; Dellmuth and Bloodgood 2019). Scholars have also applied the ideas of the spiral model to NGOs' efforts to target firm behavior to produce *de facto* policy change when states and international organizations were unable or unwilling to act, particularly on issues of labor rights and environmental protections (Spar and La Mure 2003; Sasser et al. 2006; Pulver and McAteer 2009).

The most recent waves of scholarship have shifted from examining the influence of NGOs as a category, to examining NGOs as organizations, in particular the internal and external structures that shape their strategic calculations, efficacy, accountability, and legitimacy. The growth of some NGOs into large bureaucracies with sprawling networks of national chapters raises questions of which structures for decision-making, agenda-setting, and campaigning are the most effective (Wong 2012; Stroup and Wong 2013; Tarrow 2001, 2005; Della Porta and Tarrow 2005). The global spread of international NGOs, akin to the growth of multinational corporations (MNCs), raises questions of whether the national origins effects seen within MNCs apply to international NGOs (Stroup 2012; Stroup and Murdie 2012). The political and economic climate of the states within which NGOs are located affect not only their tactics, but also their likelihood of success. NGOs tend to locate in more open, democratic, wealthy countries, because of the political and economic opportunities that these countries provide. Ironically, while democratic institutions supply NGOs with more access, they are less likely to provide influence for any particular organization (Risse-Kappen 1995; Evangelista 2002; Smith and Wiest 2005; Pallas and Uperlain 2013; Pallas and Uhlin 2014; Bloodgood et al. 2014; Dupuy, Ron, and Prakash 2016). The study of this access-influence interplay has been extended from national states to international organizations (Beyers 2002, 2004; Dür and De Bièvre 2007; Dür 2008; Steffek and Hahn 2010; Tallberg et al. 2013).

The shift from examining the effects of NGOs on states, firms, and IOs toward studying them as organizations was driven, in large part, by the need to explain patterns of bad behavior by NGOs. Once their status as principled actors was no longer assumed, the organizational imperatives of NGOs became clear (and problematic). NGOs need to behave strategically enough to survive, which creates incentives for them to put their own interests above those they are seeking to help (Fisher 1997; Cooley and Ron 2002; Bob 2005, 2012; Gibelman and Gelman 2004; Mitchell and Schmitz 2014). NGOs compete for funding, members, projects, and attention in ways that have produced conflict among even organizations seeking the same principled goals (Edwards and Hulme 1996; Bob 2005). Despite their presumed decentralized, participatory nature, competition has also led to the rise of power imbalances and even hierarchies among NGOs (Hertel 2006a; Carpenter 2011). Tensions between NGOs' need to respond to donor demands to maintain their funding and NGOs' principled obligations to pursue their missions produce accountability problems and result in sub-optimal performance (Avant 2007; Cooley and Ron 2002; Jordan and van Tuijl 2006). NGOs' reluctance to bite the hand that feeds them has reduced their legitimacy and left them vulnerable to co-optation by others (states and firms) with political agendas (Baur and Schmitz 2012; Ron, Ramos, and Rodgers 2005; Nelson and Dorsey 2003). The extent to which the legitimacy, and thus influence of NGOs, has been eroded is up for debate, but at least some recent scholars forecast the retrenchment of large international NGOs' power (Mitchell et al. 2020; Stroup and Wong 2017).

The question of where NGOs belong within IR is unclear. The study of NGOs could belong within International Organizations (IO) (global governance) or within International Political Economy (IPE) (as economic associations, interest groups, and non-state actors). Panel presentations on NGOs at the International Studies Association (ISA) and American Political Science Association (APSA) annual meetings appear within both sections. The major journals commonly associated with IO and IPE (*International Organization* and *Global Governance* versus *Review of International Political Economy* and *International Studies Quarterly*) all publish NGO research. The major NGO research themes within the IPE literature “proper” concern the role of NGOs pressuring for the inclusion of labor rights and environmental protections within the activities of economic IOs such as the WTO, IMF, and World Bank (Nelson 1995; Clarke 1998; Charnovitz 2004; Dunoff 1998; He and Murphy 2007; Bernstein and Hannah 2008). This literature includes NGOs’ promotion of increased accountability by governments and firms for social and economic rights (or rights-based approaches to development, including access to water and medicines) (Nelson and Dorsey 2003; Hertel 2006b; Kindornay et al. 2012; Schmitz 2012). The second literature regarding NGOs in IPE centers more directly on economic development, in particular the distribution of foreign aid, as increasingly states have delegated foreign aid delivery to NGOs (Edwards and Hulme 1996; Cooley and Ron 2002; Lancaster 2008). The rise of private foreign aid (Büthe et al. 2012; Smith 2014), and the increased importance of human rights in aid allocations (Cingranelli and Pasquarello 1985; Poe 1990; Neumayer 2003; Lebovic and Voeten 2009), has increased NGOs’ influence on economic development.

HELPFUL TURNS IN THE LITERATURE

While NGO scholars may initially have faced an uphill battle to win recognition within IR, the prevalence of NGOs in everyday life, and their importance as actors, has become an accepted fact. The academic NGO literature, however, risked stagnation if not for several recent advances in the research program. Traditionally NGO literature has been very descriptive, to counter the initial rejection of NGOs as significant, independent actors, and their general lack of fit within traditional IR paradigms. In order to progress, however, NGO research needed theoretical explanations for how and when NGOs matter. Scholars have had success borrowing theoretical insights from related disciplines in order to overcome the lack of applicable theories within IR. Insights from the economics of organization literature (Moe 1984; Yarbrough and Yarbrough 1988, 1990) have helped to explain rising competition among NGOs, centralization, and professionalization, as well as their adaption of techniques for advocacy from other types of actors (Cooley and Ron 2002; Sell and Prakash 2004; Klüver and Saurugger 2013; Beyers 2008; Prakash and Gugerty 2010a; Wong 2012; Murdie 2014a; Mitchell 2014). The application of new economics of organization and organization theory to NGOs also opened the study of the economic aspects of NGO activity. More NGOs are active in economic issues than any other issue area, but these organizations have traditionally received less attention than environment, human rights, and security NGOs, with the exception of economic development (Skjelsbaek 1971; Boli and Thomas 1997).

This theoretical development is complementary with moves to include insights from the varieties of capitalism literature (Hall and Soskice 2001; Hall 2007) as well as social movement

theory on political opportunity structures (Kitschelt 1986; McAdam et al. 1996; Meyer and Staggenborg 1996) in order to account for the impact of external environment on NGOs' structures, activities, and influence (Stroup 2012; Murdie and Davis 2012b; Bell et al. 2012; Stroup and Murdie 2012; Bloodgood et al. 2014; Dellmuth and Bloodgood 2019). Finally, a growing literature on NGO advocacy activities and influence (Beyers 2004; Stoddard 2006; Tallberg et al. 2018; Bloodgood 2011; Böhmelt 2013; Klüver 2012; Dellmuth and Tallberg 2017) building on interest group theories from American politics focused attention on NGOs' use of information (Austen-Smith 1993; Lohmann 1998). Lastly, analysis of NGOs' issue campaigns (Carpenter 2007; Ohanyan 2009; Murdie 2014b), and the development of power relationships among NGOs (Carpenter 2011; Stroup and Wong 2016), has advanced by adopting more formal forms of social network analysis (Ward et al. 2011; Hafner-Burton et al. 2009).

The movement from overgeneralized models of NGO behavior and influence based upon NGOs as a collective (global civil society or the third sector) or on a certain, rare subset of NGOs (large, established, successful, and global) has also helped research on NGOs progress. While large, established organizations are the best known, they are numerically a very small subset of the NGOs operating in IR (Uvin et al. 2000; Young and Lecy 2014). The population of NGOs active in national and international politics is much larger and more diverse and dynamic (i.e., higher rates of birth and death) than would be expected from a focus on just the largest and best-known organizations (Lecy and Van Slyke 2012; Nelson and Dorsey 2003).

Traditionally, NGOs as non-governmental, non-profit organizations were viewed as altruistically motivated, principled actors lacking motivation for political or self-seeking behavior (Clark 1995; Lipschutz 1992; Keck and Sikkink 1998). More recent work has rejected these normative assumptions about what NGOs do, and their inherent desirability or superiority, in favor of an empirical understanding of individual organizations' successes and failures (Bush 2015; Koch et al. 2009; Nunnenkamp, Weingarth and Weisser 2009; Lecy et al. 2012). This has enabled theoretical development regarding NGOs' strategic logic and organizational incentives (which might lead to misbehavior and unintended consequences) (Sell and Prakash 2004; Mitchell and Schmitz 2014) and helps to account for the diversity and dynamism observed within NGOs as a population.

TOP RESEARCH PROJECTS IN THE LAST DECADE

While it is difficult to select the absolute best of the recent research on NGOs, there are several current clusters of researchers that are producing innovative, theoretical research drawing on solid empirical methods that are likely to make substantive contributions to the development of NGO research.

Charli Carpenter (2007) led the way by using a new technological tool (IssueCrawler) which provides a rigorous methodology to map out NGOs networks via their website links. Using this network data, she has convincingly demonstrated that some NGOs play politics very effectively to shape the definition of issues for advocacy (agenda setting). NGOs in certain network positions can serve as gatekeepers within campaigns, as even when networks are informal centralization still occurs (Carpenter 2007, 2011).

Amanda Murdie, in collaboration with David Davis, Chad Clay, Sam Bell, and Tavishi Bhasin, has leveraged both sophisticated statistical methods and innovative data sources

(IDEAS) in order to test important claims from the first wave of NGO literature more completely and convincingly. Their research has shown the substantive effect of naming and shaming on state behavior (Murdie and Davis 2012a) as well as the impact of domestic NGOs when networked to foreign NGOs on domestic policies and protest (Murdie and Bhasin 2011; Bell et al. 2012, 2014). This work confirms core insights of the boomerang model (Keck and Sikkink 1998). Murdie's research group has also advanced the application of formal social network analysis to NGO networks (Murdie and Davis 2012b; Murdie 2014b) and they have demonstrated the importance of NGO location via spatial analysis of NGOs' actions and influence (Bell et al. 2012, 2014).

Jonas Tallberg and Christer Jönsson, in a joint Transdemos/Transaccess project housed at Lund and Stockholm Universities, have collected incredible new data on NGOs access to and influence within IOs (Jönsson and Tallberg 2010; Tallberg et al. 2013). This new data has allowed them to test systematically theories on when and why NGOs can change national and international policies (Tallberg et al. 2018; Dellmuth and Tallberg 2017) and the ability of NGOs to provide democratic legitimacy for IOs (Bexell et al. 2010; Tallberg and Uhlin 2011).

A second large data collection initiative, the TNGO Initiative, at Syracuse University has produced another trove of data built on extensive interviews with the leadership of the largest INGOs located in the US. These elite level interviews have enabled Hans Peter Schmitz and his co-authors, including George Mitchell, Jesse Lecy, Haley Swedlund, and Paloma Raggo, to gain new insight into the strategies employed by NGOs to cope with funding challenges, manage reputation concerns, evaluate success, and maintain accountability (Mitchell et al. 2020; Mitchell and Schmitz 2014; Lecy et al. 2012; Mitchell 2014; Schmitz et al. 2012).

Sarah Stroup and Wendy Wong have merged individual research projects with considerable overlap into a larger, joint project to study NGOs' authority in IR. Their research differs from the other groups mentioned here in that it returns to IR theory to explain NGO behavior. Now that NGOs are accepted as full participants in international politics, Stroup and Wong examine variations in individual organizations' authority. They apply classic IR theories to INGOs in a rigorous and systematic fashion that takes INGOs seriously as actors with their own power and influence, but without assumptions about what NGOs do and why (Stroup and Wong 2013; Stroup and Wong 2016; Stroup and Wong 2017).

A third large project leverages the benefits of official government data on international NGOs from US tax filings to conduct time series analysis on this population of NGOs. Using population ecology theories, Sarah Bush and Jennifer Hadden (2019) find that the establishment of new INGOs in the US has "stagnated," likely due to the density of the population. They have tested their quantitative findings in two sectors, conservation and democracy promotion, but the larger project seeks to extend their explanation for why the era of phenomenal INGO growth has likely passed. Their finding of INGO stabilization, and possible retrenchment, echoes across several other recent projects as well (Mitchell et al. 2020; Stroup and Wong 2017; Balboa 2018).

FUTURE DIRECTIONS IN NGO RESEARCH

While NGO research has progressed markedly in the last decades, there are three notable areas of weakness. These gaps in NGO research provide fertile areas for future research.

First, there is a need to bridge the gulf that exists between American and European NGO research. Parallel theoretical development wastes resources and misses important areas for collaboration. Conversations between American and European scholars have been slowed (or prevented) by the use of different concepts—advocacy NGO (e.g., Mitchell and Schmitz 2014; Stroup and Wong 2016; Murdie and Davis 2012a, 2012b) versus interest group (Beyers 2004; Dür 2008; Klüver and Saurugger 2013). Slowly growing recognition between the communities provides increasing opportunities for more transnational research partnerships. Scholars have now given deliberate attention to the theoretical benefits of explicitly combining ideas from the INGO literature in international relations and the interest group literature from European studies (Dellmuth and Bloodgood 2019). While INGO researchers provide insights into the power of networks and normative framing, interest group theories provide insights into forum shopping and the creative use of political opportunity structures.

Second, NGO researchers are desperately in need of better data. There is currently no dataset that provides researchers with the ability to study the activities of NGOs across issues, countries, and time simultaneously. It is not surprising that four of the most productive research projects in the last 10 years involve new means of collecting better data on NGOs (Carpenter 2007; Murdie and Davis 2012a; Tallberg et al. 2013). Without sufficient quantities of quality data to test theories of NGOs' locations, structures, actions and influence, NGO research programs cannot advance.

Third, while there has been much progress in this area, NGO research still needs more explicit theory building and testing to explain what NGOs do, when, and why. Research has moved beyond demonstrating that NGOs matter, but we are less certain about the conditions under which NGOs matter and why not all NGOs matter (or even survive). In addition, theory building needs to shift from a focus on NGO access and influence to the nature of NGO power and accountability (augmenting work such as Balboa 2018; Deloffre 2016; Raggo 2020).

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PART VII

OUTCOMES

CHAPTER 37

LIBERALIZATION

TIMM BETZ AND AMY POND

THE cross-border movement of capital, goods, and people is central to International Political Economy (IPE). Government policies play a key role in this regard: governments can facilitate or restrict cross-border transactions. Market liberalization—which we understand here as the removal of barriers to cross-border transactions—sets the stage for globalization, but it also shapes the form that globalization takes.

Consequently, studying the sources of liberalization takes a prominent place in IPE. And it offers lessons for political science more broadly. Ultimately, because liberalization is a policy choice by governments, questions about liberalization are questions about government behavior in domestic and international politics and about the relationship between governments and other actors in politics. Research about liberalization contributes to our understanding of distributive politics; the role of power, norms, and ideas; mass political behavior; and institutional theories of politics. And because it addresses cross-border transactions, liberalization touches on questions at the core of our understanding of the state, including control over territorial borders, the domestic economy, and policy autonomy.

A major distinction across work in this research area is what is perceived as the puzzling aspect of liberalization. One perspective views restrictions as puzzling: if liberalization is a public good with broad-based benefits, why do we observe so many market restrictions? Viewing liberalization as a public good is a natural consequence of economic theories, which emphasize the aggregate gains from trade and capital account liberalization, such as lower prices, lower borrowing costs, productivity gains, and technological spillovers. From this perspective, politics explains the maintenance of (inefficient) market restrictions.

A second perspective views liberalization as puzzling and, in more critical approaches, problematic. Liberalization—and especially liberalization in the context of a broader neo-liberal reform agenda—is a private good. Liberalization from this perspective benefits a select few and frequently carries significant costs for domestic societies. These two different perspectives are united by an emphasis on collective action problems, corporate influence in politics, and bargaining between states; and by questions over who supports liberalization, when liberalization occurs, and how governments liberalize markets.¹

LIBERALIZATION AS A PUBLIC GOOD

Viewing liberalization as a public good offers a powerful analytical framework in IPE. The liberalization of trade and capital accounts benefits societies overall, reducing the cost of goods and the cost of borrowing, reducing opportunities for rent-seeking by interest groups and policymakers, and increasing the efficiency of markets. And while liberalization creates winners and losers, in principle the gains could be redistributed to make everyone better off. Yet, because the gains from liberalization are dispersed across voters as consumers, who are a large and diffuse group, public demand for liberalization is weak (Pareto 1927; Olson 1965). In contrast, the costs from liberalization are concentrated: with both trade and capital account liberalization, market incumbents lose market shares and may go out of business, providing an incentive to oppose liberalization.

This framework provides a parsimonious model for understanding central cleavages over government policy. Because the framework emphasizes the public goods character of liberalization, and contrasts it with the private goods delivered by market restrictions, it connects readily to other subfields in political science. If trade and financial market liberalization are public goods and hampered by firm influence, for example, political institutions that circumscribe the influence of firms—and special interests more broadly—should lead to liberalization. This argument has implications for our understanding of several dimensions of political institutions. Differences in electoral rules (Rogowski 1987b; Nielson 2003; Gawande, Krishna, and Olarreaga 2009; Mukherjee, Yadav, and Béjar 2014), checks and balances (Mansfield, Milner, and Pevehouse 2007), interest group access (Ehrlich 2007), and delegation to politically insulated policymakers (Lohmann and O'Halloran 1994; Gilligan 1997a) explain patterns of trade and capital account liberalization across countries. Notably, these patterns are reversed for the liberalization of migration policy, where firms tend to benefit from open markets and voters tend to be opposed (Bearce and Hart 2017).

This framework also lends itself to a normative interpretation: processes and institutions that are associated with liberalization can be justified on the basis that they produce public goods, suppress the influence of special interests in politics, and are more representative of (diffuse) voter interests. In particular, liberal political systems and liberal economic policy go hand in hand (Mansfield, Milner, and Rosendorff 2000, 2002; Lake and Baum 2001; Bueno de Mesquita et al. 2003; Phelan 2011)—offering a powerful narrative behind the simultaneous waves of democratization and liberalization throughout the twentieth century (Milner and Mukherjee 2009).

Moreover, if liberalization is driven by public-goods considerations, government capacity becomes an important variable in explaining why some governments refrain from liberalization despite the perceived benefits: governments may struggle to replace the lost revenue from liberalization because they lack the fiscal capacity to do so (Richter 2013; Queralto 2015; Bastiaens and Rudra 2016); governments may lack the capacity to implement and enforce liberalization (Hamilton-Hart 2003; Mosley 2010; Gray 2014; Betz 2019); and frictions in the redistribution of the gains from liberalization across groups can undermine the ability of governments to open their markets (Davis 2020).

The public goods framework has additional implications for how governments liberalize their markets. If liberalization is a public good, some governments may have incentives

to appear as if they put liberalization in place, while undermining the effects of liberalization on less transparent dimensions. Governments can substitute regulatory and non-tariff barriers, as relatively obscure and complex measures, for tariffs, which are a more easily observable form of trade protection (Magee, Brock and Young 1989; Mansfield and Busch 1995; Kono 2006). A similar argument explains patterns of capital account liberalization, where *de jure* liberalization efforts are not accompanied by *de facto* liberalization (Quinn, Schindler, and Toyoda 2011; Henisz and Mansfield 2019). For example, governments can open capital accounts to facilitate capital inflows while forcing foreign entrants to channel credit through domestic banks (Pepinsky 2013) or liberalize capital inflows while restricting outflows (Pond 2018c). While governments may have incentives to provide liberalization as a public good, they systematically offset these effects by reaching for compensatory market restrictions that are even costlier to societies than the measures that were removed (Kono 2006).

Liberalization as a public good, but across countries, is also the basis for theories that emphasize the international system (Lake 1993). In Hegemonic Stability Theory, because of collective action challenges, a global hegemon is necessary to provide the public good of maintaining open markets, for example by punishing defectors that would undermine the liberal order (Krasner 1976; Kindleberger 1986; Eichengreen 1987; Gilpin 2011). If open markets increase the wealth of individual states, they also increase the wealth of adversaries. This rationale underpins a large literature on the security externalities of liberalization. Governments should be more open to economic interactions with allies (Gowa and Mansfield 1993; Gowa 1994) and with countries with similar interests (Morrow, Siverson, and Tabares 1998), and they may open markets in exchange for security benefits (Poast 2012).

International institutions can sustain international openness even in the absence of a hegemon, thus providing public goods at the international level. The literature offers two broader interpretations of the role of international institutions in liberalization. As a set of rules, international institutions align expectations, coordinate behavior, and empower some actors over others; but international institutions, and bureaucrats associated with them, also have agency of their own, driving policy change, reorienting the political discourse, and exploiting their agenda-setting power.

The seminal work on institutions as a set of rules has shaped the literature in both international political economy and international institutions, and the two fields remain closely aligned (Keohane 1984). By aligning expectations and providing standards for evaluating government behavior, international institutions sustain liberalization once in place (Simmons 2000; Rosendorff 2005; Chaudoin 2014). By structuring negotiations, international institutions mobilize domestic constituencies and shape bargaining between states (Davis 2004). By providing legitimacy, international institutions also shape the responses of other governments and thus the evolution of liberalization (Pelc 2010; Brewster 2013). And as commitment devices, international institutions reduce policy uncertainty, sustaining beliefs about continued market openness and its benefits (Mansfield and Reinhardt 2008; Mansfield and Milner 2018).

A second strand of literature ascribes international institutions more agency in driving liberalization. The International Monetary Fund (IMF) is an early example in this literature, because international bureaucrats directly negotiate with governments over trade and capital account liberalization in exchange for loans. In terms of who is driving liberalization, this led to a vibrant literature weighing the relative importance of powerful donor countries (Stone 2002; Dreher and Jensen 2007; Dreher, Sturm, and Vreeland 2009), private

creditors (Gould 2003; Broz and Hawes 2006), the Fund staff (Momani 2005; Copelovitch 2010), and recipient countries (Vreeland 2003). This research agenda presents new questions for those interested in the legitimacy of the international order, and new opportunities for understanding—and questioning—the role of powerful countries and emerging powers in international politics (Moschella 2009; Fioretos 2019; Fioretos and Heldt 2019).

Increasingly, the literature also turns to other international institutions as independent actors in driving liberalization. In this area, American- and British-style research traditions are starting to see more overlap, both in substance and approach, by emphasizing the ability of international institutions as actors, independent of state interests, to change the discourse over liberalization. Consistent with sociological approaches to institutionalism, liberalization is not (solely) driven by material considerations, but also by what is perceived as appropriate and legitimate.

For example, the UN confers legitimacy to its delegates; UN delegates, in turn, can use their perceived authority to shape rule-making at the World Trade Organization (WTO) (Margulis 2018). Similarly, the Organisation for Economic Co-operation and Development (OECD) played an active role in the liberalization of capital accounts by shifting the discourse over liberalization, moving beyond what OECD member states had initially envisioned (Howarth and Sadeh 2011). The increasing availability of tools such as text analysis facilitates quantitative studies as well, documenting the role of language and discourse in driving liberalization both through the IMF (Kaya and Reay 2019) and the WTO dispute settlement body (Busch and Pelc 2019). Experience within the rule-based environment of international institutions, such as the WTO, can also lead governments to invest in their institutional capacity, allowing for a more active role of the government in shaping liberalization domestically and at the international level (Shaffer, Nedumpara, and Sinha 2015; Sinha 2016).

The public goods framework is also amenable to the role of learning and ideas in explaining the timing and the forms of liberalization. Whether liberalization is perceived as a public good, and to what extent this belief is held and perpetuated, has changed over time. For example, waves of capital account liberalization and closure throughout the twentieth century align with waves of ideology (Quinn and Toyoda 2007). The prevalence of ideas can also explain liberalization at key moments in history. Morrison (2012) documents how Great Britain's move toward open trade was driven by a shift in the beliefs of key policymakers away from mercantilism and toward Adam Smith's ideas of liberalism. Thus, the emergence of a new idea was key in explaining this period of globalization. The repeal of the corn laws in Great Britain can likewise be traced to a shift in ideas among political elites (Schonhardt-Bailey 2006). Chwioroth (2007) offers empirical evidence for the role of ideas in explaining capital account liberalization: countries whose policymakers were socialized into neoliberal beliefs through their education tend to have more open capital accounts. Liberalization as a public good also featured prominently in the justification of the Washington Consensus and its underlying neoliberal ideas, which view market openness and deregulation as optimal and envision little role for the state (Florio 2002; Helleiner 2003, 2019).

Moreover, ideas shape the form of liberalization. What is viewed as a legitimate form of market restriction and as the "proper" role of the state changes over time. These waves of ideas result in different policy choices that then persist, explaining the simultaneous presence of seemingly contradictory policies—such as a more active role of government intervention in trade policies for some sectors and not others (Goldstein 1986, 1988). Thus,

ideas matter not only for whether governments liberalize their markets, but also how they do so. A prominent example is the pact of embedded liberalism, which emphasizes the role of the government in providing social safety nets to accompany open markets (Ruggie 1982; Hays 2009)—an idea that is distinct from the paradigms of free trade or fair trade (Goldstein 1988). This pact has faltered in the wake of the global financial crisis as citizens became disillusioned with government intervention and instead associated international integration with job loss and inequality (Frieden 2019 Ch. 2).

Prior experience—of their own and others—can be an important element in how firms, citizens, and policymakers perceive liberalization. Economic crises, in particular, provide an impetus to change the status quo and to challenge existing paradigms, which may result in a swing toward more or less liberalization (Florio 2002; Brooks and Kurtz 2007), even beyond what the change in economic circumstances would warrant (Widmaier, Blyth, and Seabrooke 2007).

The broad-scale liberalization in response to the Smoot-Hawley Tariff Act and the Great Depression, and the increasing legitimacy of capital controls in response to the Great Recession, are perhaps the most notable examples of changes brought about by crises (Chwieroth 2013; Grabel and Gallagher 2014). But similar patterns emerge for smaller crises, and economic difficulties more generally, as well. A tightening of global credit conditions may promote liberalization if governments seek access to capital from abroad (Haggard and Maxfield 1996; Betz and Kerner 2016b). Policymakers may also use liberalization during crises to reassure citizens and markets that they are implementing sound economic policies, reinforcing the perception that liberalization is a public good (Mansfield and Milner 2018).

If the experience of other countries informs the perception of economic policies, liberalization may diffuse across countries. Simmons and Elkins (2004) observe that policies of liberalization cluster in specific regions at specific times, spread by two mechanisms. First, the benefits of liberalization are larger when other countries also have open markets. Second, market openness in one country provides information about the costs and benefits of openness to that country's peers. Both processes are consistent with the policies of liberalization that we observe. The process may also be contingent on domestic political institutions (Steinberg, Nelson, and Nguyen 2018) and on the performance of the countries implementing the reforms (Brooks and Kurtz 2007).

Of course, ideas can be contested. Despite a growing overlap between American- and British-style IPE in their interest in ideas, only the latter approach emphasizes the role of ideas in perpetuating existing power relationships. In particular, and reminiscent of work by Gramsci, the very idea that liberalization is a public good might be perpetuated by elites that benefit from liberalization. Political elites have constructed a discourse over liberalization that tried to obscure the costs for societies (Siles-Brügge 2014). Some governments might want to give the appearance of adhering to established ideas like the Washington Consensus while in fact returning to market intervention (Ban and Blyth 2013). Ideas also matter for the choice among conflicting goals, which in turn may dictate policy choices. Blyth and Matthijs (2017) identify two broad macro-regimes throughout the twentieth century—one centered on full employment, one centered on price stability—and show how adherence to each of these regimes had consequences for the liberalization of individual policies.

The public goods framework thus speaks to several important debates and provides a clear analytical framework for understanding liberalization. At the same time, the public goods framework, which emphasizes the price effects of market restrictions, is at odds with other

empirical regularities, especially on trade politics.² While Baker (2003, 2005) documents that support for free trade in Latin America is driven by consumption effects, voters in other countries—and in particular in the most open countries—appear unaware of, and unmoved by, some of the benefits of market liberalization, especially when it comes to diffuse gains like the price effects of liberalization (Hiscox 2006; Naoi and Kume 2015; Bearce and Moya 2020). The low salience of trade issues in elections further casts doubt on some aspects of voter- and public goods-driven models (Guisinger 2009), as do findings that many voters evaluate liberalization on non-material dimensions, including nationalism and in-group favoritism, or are driven by social networks rather than economic self-interest (Tsygankov 2000; Mansfield and Mutz 2009; Ahlquist, Clayton, and Levi 2014; Pandya and Venkatesan 2016; Guisinger 2017; Mutz and Kim 2017; Mansfield, Mutz, and Brackbill 2019).

Moreover, there is little evidence that policymakers liberalize markets with an eye toward price effects. Tariffs are highest on those products where tariffs have the largest price effects. In democracies in particular, price considerations do not appear to drive trade liberalization: Tariffs are higher on consumption products than on other products, and this effect is strongest in democracies. Democratization, in turn, has smaller effects on tariff reductions for consumption products than for other goods, suggesting that price effects and consumer interests do not explain trade liberalization (Betz and Pond 2019). Moreover, early forms of democratization in India led to protectionism, not free trade (Gaikwad and Casler 2019).

On the one hand, these findings might be interpreted as evidence of collective action problems and informational asymmetries (Rho and Tomz 2017), which could be remedied if voters were more informed. Such an argument also explains different political dynamics across trade and capital account liberalization, based on differences in relative political salience (Brooks and Kurtz 2007).

On the other hand, these findings raise questions about the political forces that drive liberalization: if voters are unaware of the benefits of liberalization and frequently opposed to it, why would accountable and representative policymakers implement liberalization? The belief, based on economic models, that voters *should* value liberalization, and that any resistance to liberalization could be overcome with better information and more redistribution, led parts of this literature to underestimate the costs of liberalization for societies and the eventual resistance this might cause. The severity of the backlash to liberalization in advanced countries over the past decade, driven by the often persistent costs of liberalization (Autor, Dorn, and Hanson 2013; Colantone and Stanig 2018), has caught parts of this literature by surprise.

The contrast with more critical approaches to liberalization—which question, for example, the motives behind neoliberal reform agendas in the 1990s—is also pronounced. Focusing on the discourse over liberalization, Fairbrother (2010) observes that policymakers tend to focus on the benefits for producers, rather than for consumers and societies overall, which raises doubts over explanations that view liberalization as driven by public good considerations. And while liberalization can have substantial economic benefits, the distribution of those benefits is at times biased against voters as employees (Dean 2016) and toward high-income countries with high-quality market institutions (Wade 2010; Levchenko 2007).

Additionally, capital account liberalization can impose substantial constraints on governments, partially offsetting the gains (Lindblom 1977; Andrews 1994). While liberalization can have attributes of a public good, making societies better off overall, it may come at the cost of giving up other public goods, such as policy autonomy (Li and Smith 2002;

Arel-Bundock 2017) or environmental protection (Bechtel, Bernauer, and Meyer 2012). The public goods character of liberalization may be undermined if the policy constraints imposed by liberalization benefit political elites that fear redistribution in the future (Pond 2018a). Perhaps surprisingly, liberalization of migrant flows can reduce constraints on policymakers, by allowing potential political dissidents to leave (Hirschman 1970; Gehlbach 2006; Clark, Golder, and Golder 2017; Sellars 2018). Liberalization also may necessitate public spending to maintain support for openness, putting dual strains on governments (Flores and Nooruddin 2016; Bastiaens and Rudra 2018). Others have emphasized multinational corporations (MNCs), especially from the US and Western Europe, as the prime beneficiaries of liberalization, often in opposition to consumers and voter groups (Young 2016). As we note in the next section, the turn toward firm-level accounts of liberalization in recent years led to a new convergence between American- and British-style IPE.

LIBERALIZATION AS A PRIVATE GOOD

A second view of liberalization, reaching back at least to Schattschneider (1935), emphasizes the distributional conflict created by trade and capital account liberalization. Drawing on theories of comparative advantage, a large part of this literature focuses on whether the political cleavages over these policies fall along class lines, following the Heckscher-Ohlin model, or along industry lines, following the Ricardo-Viner model (Schattschneider 1935; Rogowski 1987a; Frieden 1991). Which of the two cleavages is more prevalent at any given point in time can be explained by differences in factor mobility (Hiscox 2002; Ladewig 2006): when labor is mobile across sectors, the Heckscher-Ohlin model provides a better explanation of political conflict. Immigration can also be viewed through this lens, where restrictions benefit unskilled labor in developed countries, but hurt unskilled labor in developing countries (Milanovic 2011).

These models explain several empirical patterns in the liberalization of trade and capital accounts. For example, whether industries are able to block liberalization depends on their geographic features. Political influence is largest for industries that are dispersed across electoral districts but concentrated geographically (Busch and Reinhardt 1999, 2005), and this effect in turn depends on a country's electoral rules (McGillivray 2004; Rickard 2012). In a class-based model, partisanship moves to the forefront: if political parties align with economic classes, the support of left-wing parties for liberalization depends on whether labor is a relatively abundant or scarce factor (Quinn and Inclan 1997; Li and Smith 2002; Milner and Judkins 2004; Pinto 2013). Similar arguments apply to democratic institutions, which should reduce the political influence of capital owners, relative to labor, in driving trade and capital account liberalization (Milner and Kubota 2005; Kono 2006; Pandya 2014). Some work emphasizes how these theories interact with the global spread of ideas (Quinn and Toyoda 2007; Steinberg, Nelson, and Nguyen 2018).

When explaining trade liberalization, and in contrast to models of trade liberalization as a public good, many of these models implicitly build on the norm of reciprocity. Reciprocity, together with most favored nation and national treatment, is one of the key elements underlying the General Agreement on Tariffs and Trade (GATT) and the WTO and most preferential trade agreements. With reciprocal trade agreements, export-oriented

factor owners, industries, or firms support liberalization at home when it gives them access to markets abroad (Bailey, Goldstein, and Weingast 1997; Gilligan 1997a; Pahre 2008; Betz 2017). This form of issue linkage has overcome some of the most entrenched resistance to liberalization, such as that from the agricultural sector in many countries (Davis 2004).³

Mirroring developments in other subfields of political science, international political economy has increasingly turned to more granular theories and data. Over the past two decades, economists have documented that few individual firms engage in international economic transactions, that the gains from openness are concentrated on these firms, and that liberalization induces substantial reallocations across firms within industries (Bernard and Jensen 1999; Melitz 2003; Bernard et al. 2007; Goldberg and Pavcnik 2007; Freund and Pierola 2015; Bernard et al. 2018). This led to a surge of interest in individual-level and firm-level accounts of liberalization.

These studies point to substantial differences across firms within industries, rather than differences across industries or across classes. Firms differ in production technologies, access to inputs from abroad, and access to production networks, and as a consequence differ in their ability to engage with foreign markets: only the largest and most productive firms can afford the fixed costs of entering foreign markets and establishing global production networks, resulting in concentrated gains from trade and capital account liberalization. Moreover, the gains from liberalization are not only concentrated on few firms, but on firms that have political influence.

Plouffe (2015) provides an early overview of a firm-based model of trade politics. Subsequent work elaborated several implications. Within-industry conflict and firm-level lobbying for trade liberalization are more pronounced in industries with higher levels of intra-industry trade (Madeira 2016), challenging a more harmonious view of intra-industry trade (for a review see, e.g., Gilligan 1997b). Other work has built on a firm-based model to explain the creation and management of supply chains (Dallas 2015), intra-industry divisions in public position-taking (Osgood 2016), firm attitudes (Plouffe 2017), lobbying for trade liberalization (Kim 2017), and the institutional sources of trade policy (Betz 2017). Early precursors to these studies are Milner (1988b), who emphasizes the international ties of individual firms within industries as a source of opposition to protectionism, and Chase (2004b), who documents that smaller firms are more protectionist than larger firms and relates these differences to cross-country differences in trade policy. The contrast between the liberalization of tariffs and non-tariff barriers is emphasized by Gulotty (2020). Because only the largest firms can overcome the fixed costs created by non-tariff barriers (such as adjusting to new labeling requirements), the very firms that tend to benefit from, and drive, the liberalization of tariffs also support restrictions in the form of non-tariff barriers.

Recent work also builds on firm-based models to explain capital account liberalization, for example with different financing constraints across firms (Leiteritz 2012; Danzman 2019). This understanding of the drivers of liberalization also speaks to some of the dynamics of liberalization over time: once a market is sufficiently competitive, governments have little reason to maintain barriers to international trade and capital flows, facilitating a “juggernaut” effect toward further liberalization (Hathaway 1998; Rajan and Zingales 2003; Brooks 2004; Baldwin and Robert-Nicoud 2008; Pond 2018b), but also increasing the resistance to initial attempts at liberalization (Davis 2015).

Fusing the granularity of trade policy with a firm-based model in the context of trade agreements reverses the collective action arguments outlined above: liberalization not only

provides diffuse gains for the public, but also concentrated gains for individual, politically powerful firms—which leads to new implications for the institutional theories of liberalization outlined in the previous section (Goldstein and Gulotty 2014; Betz 2017; Betz and Pond 2019). Liberalization, from the perspective of a firm-based model, need not be driven by a higher regard for voter interests or the insulation of policymakers from special interests. Consistent with the growing discontent with liberalization among citizens in developed countries, liberalization may happen not because of, but despite, what citizens and societal groups prefer. Firms may lobby for the liberalization of trade and capital accounts, but they are not doing the bidding of voters in providing public goods—the shape that liberalization takes differs in its timing, in its product coverage, in its trade partners, and in its choice of policy instruments from what voters may have preferred. The largest firms emerge as the main beneficiaries of the liberalization of trade and capital accounts, and of the international rules that have contributed to that liberalization.

The gains from liberalization for individual firms, and the role of individual firms in IPE, has always had a prominent place in British-style IPE (Stopford, Strange, and Henley 1991; Strange 1996; Cohen 2008; Rutland 2013). The turn to firm-based models has thus resulted in a new alignment in themes between American- and British-style IPE, and a growing consensus that trade and capital account liberalization during the twentieth century were shaped by large, politically powerful firms, sometimes in opposition to voters. Additionally, the content of liberalization efforts appears to be tilted toward powerful Western countries and their “domestic” MNCs (Wade 2010). The imbalance between countries is also evident in the dispute settlement body of the WTO, although it appears that the rule-based system of the dispute settlement body has reduced at least some of the power asymmetries between countries (Sattler and Bernauer 2011).

Driven by advances in transportation and information technology, production processes are now not only geographically separated from the location of consumers, but the different stages of production have also disintegrated across firms and across countries (Baldwin 2016). The emphasis on the largest firms as the beneficiaries of openness thus moved MNCs, global production processes, and global supply chains, which featured prominently in earlier literature (Vernon 1971; Moran 1978; Cox 1987; Milner 1988a; Milner and Yoffie 1989; Goodman and Pauly 1993), back to the forefront in explaining liberalization: in addition to exporting and import-competing firms, importing firms play an important role in the politics over trade and capital account liberalization. This has the potential to substantially reshape political responses to external financial conditions (Jensen, Quinn, and Weymouth 2015; Weldzius 2020), patterns of trade liberalization (Manger 2005, 2012; Baccini, Dür, and Elsig 2018; Anderer, Dür, and Lechner 2020), and the role of individual firms in the creation and governance of international economic flows (Dallas 2015; Gaubert and Itskhoki 2018).

Viewing liberalization as a private good also offers a framework for understanding the timing and the shape of liberalization. Diffusion explains liberalization by spreading the perception that liberalization is a public good, as discussed in the previous section. Diffusion also accounts for liberalization, however, through competition effects. If firms from other countries gain access to new markets—through capital account and trade liberalization—it puts domestic firms at a relative disadvantage. Thus, liberalization between pairs of countries, by discriminating against outsiders, triggers demands for liberalization by firms located in those outsiders: they seek to prevent a deterioration in relative terms for their firms’ exports (Dür 2010) and for their firms’ investment (Manger 2009).

Liberalization among groups of countries, most prominently in the European Union (EU), implies a relative increase in market restrictions for firms from outside the arrangement (Limão 2006). This aspect of regionalism is frequently by design, because it helps governments build political coalitions (Chase 2004a). Whether regionalism provides a “stumbling block” or a “stepping stone” to further liberalization remains a largely open question (Mansfield and Milner 1999). Multilateralism itself is subject to similar challenges (Goldstein et al. 2000). Gowa and Hicks (2012) show how governments have used the most favored nations clause in a discriminatory fashion, creating private goods for individual countries. Even in the relatively legalized environment of the WTO’s dispute settlement body, governments frequently strike settlements that disadvantage third countries (Kucik and Pelc 2016). Moreover, trade barriers by foreign governments are more likely to get challenged if they impose concentrated costs: trade disputes that provide public goods across countries are less readily initiated, shaping patterns of liberalization across countries and across products (Johns and Pelc 2018).

That liberalization can also have protectionist undertones is a common theme in a larger body of literature on how governments liberalize. Governments typically do not liberalize their markets across the board. They tailor tariffs to individual products and drive up the variance in tariff rates across products (Goldstein and Gulotty 2015; Kim 2017; Betz 2017, 2019), insert content requirements that limit the scope of liberalization (Chase 2008b), target trade liberalization toward some countries but not others (Kono 2008; Barari, Kim, and Wong 2020), limit migration for low-skilled but not high-skilled workers (Peters 2015), and open their capital accounts toward investment inflows but restrict outflows (Pond 2018c). These forms of partial liberalization facilitate the creation of political coalitions and may help governments accommodate competing demands. But they also open room for arbitrage, corruption, and evasion. The resulting policy complexity may ultimately depress the effects of liberalization efforts, in some environments intentionally so (Wu 2020).

The redistributive character of liberalization also motivates a growing body of literature that seeks to understand the relationship between different policy choices. With open capital accounts, exchange rate movements have clear implications for domestic groups: currency appreciation aids consumers and importing firms, but hurts import-competing firms and exporting firms (Frieden 1991). Consequently, currency appreciation is associated with an increase in both protectionist demands at home (Broz and Werfel 2014) and attempts to remove trade barriers abroad (Betz and Kerner 2016a). Conversely, where trade agreements restrict a government’s trade policy choices, monetary policy autonomy becomes more attractive to governments (Copelovitch and Pevehouse 2013). Notably, Jensen, Quinn, and Weymouth (2015) show how the presence of MNCs, as politically powerful importing firms, breaks the otherwise clear link between external financial conditions and trade policy.

Recent work looks more comprehensively at the relationship between trade liberalization, capital account liberalization, and migration policy. Once trade and capital account liberalization allowed US firms to produce goods abroad and import them back to the US, firms no longer required low-cost labor within the US (Peters 2014). This resulted in less support for open immigration policies from firms, increased influence of nativist forces, and more restrictive immigration policy. Thus, the liberalization of trade and capital flows substitutes for the liberalization of migration. These findings point to the need to consider the relationship between different dimensions of liberalization, and how governments address competing demands over liberalization. Examples include the inclusion of labor rights and

environmental standards in trade agreements, which may be interpreted as a form of non-tariff barrier to reduce the extent of *de facto* liberalization (Lechner 2016) and which may substitute for compensatory government spending (Bastiaens and Postnikov 2020).

New areas of overlap between different research traditions emerge in questions about the mass politics over liberalization and the role of material interests in explaining voter attitudes. The emphasis on production networks and modes of production in British IPE, most prominently articulated by Cox (1987), is increasingly mirrored in work that is more closely aligned with American IPE as well. It is well established that highly educated voters in the US tend to be more supportive of trade liberalization. This may be interpreted as evidence of a factor-based model (Scheve and Slaughter 2001). Yet, contrasting with a factor-model, but consistent with research on the distributional consequences of trade liberalization based on firm-based models and in models that emphasize the skill-bias in international trade (Goldberg and Pavcnik 2007; Bustos 2011), similar empirical patterns are evident in Argentinean surveys (Ardanaz, Murillo, and Pinto 2013). This finding suggests that mass attitudes may be better explained by the skill bias inherent in trade and capital account liberalization (Li and Smith 2002; Menendez, Owen, and Walter 2018; Palmtag, Rommel, and Walter 2020). Recent work also examines the role of individual occupations in explaining attitudes over liberalization. Occupations that require few context-specific skills can be relocated abroad relatively easily, creating new cleavages over liberalization based on occupation, which cut across industry and class lines (Chase 2008a; Owen and Johnston 2017; Owen 2017).

Education may also shape attitudes by providing individuals with information about the gains from trade and through socialization, which explains voter attitudes toward both trade (Hainmueller and Hiscox 2006) and migration (Hainmueller and Hiscox 2007; Hainmueller, Hiscox, and Margalit 2015). That voters may have limited knowledge of the distributional consequences of liberalization suggests that they are susceptible to informational cues, socialization effects, and framing effects (Hiscox 2006; Ahlquist, Clayton and Levi 2014; Naoi and Kume 2015; Rho and Tomz 2017; Bearce and Moya 2020). Consequently, the discourse over liberalization matters for building support for liberalization. Blending constructivist approaches with firm-based models of trade, Skonieczny (2018) documents how pro-trade firms use national identity narratives, rather than arguments about material gains, to shape public support for trade liberalization. In part driven by recent elections in the US and the UK, this research agenda suggests new questions not just about who supports liberalization, but the ideological sources and the cohesion of political coalitions. How individual firms and interest groups succeed in building public support for trade liberalization, sometimes in opposition to (perceived) material interests, is a question of increasing political relevance.

JOURNAL ARTICLES

In this review article, we developed two frameworks for understanding liberalization, stressing either the public- or private-good effects of liberalization. In doing so, we attempted to bridge many divisions in IPE. Nevertheless, the conventional wisdom expects a divide between American- and British-style IPE: American IPE is oriented toward abstract theory and generalizability across cases, as well as economic methodologies and identification of

causal effects. British IPE alternatively emphasizes deep understandings of subject matter in relatively few units and is more open to exploring topics, like power, intentions, and hierarchy, that might challenge traditional forms of empirical inference (see Cohen 2008 for a discussion of the development of both traditions).

To evaluate the extent of division in the literature, we tasked two graduate students to collect the literature on “liberalization” in *International Organization* (IO), frequently associated with American-style IPE, and *Review of International Political Economy* (RIPE), representing the British tradition.⁴ They collected 352 articles, 212 from RIPE and 140 from IO, from 2000 to 2019. They then coded each article depending on the topic, emphasis of the study, and the methods used.

The topics we identified are trade, capital, migration, or globalization. Globalization captures studies that consider the drivers of increased integration, writ large, rather than any specific type of economic flow—the vast majority are studies of trade and capital liberalization. We fit each article into a single category; the categories are thus mutually exclusive.

Figure 37.1 depicts the share of articles in each issue area over time for both IO and RIPE. The overwhelming majority of the studies in both journals emphasize trade or capital flows only. Migration seems slightly more common in IO while studies of globalization as a broad phenomenon appear more commonly in RIPE. This is consistent with a general emphasis on power, hierarchy, and the constraints posed by international economic integration in the British tradition (Strange 1996).

We then coded the papers by the methodology used, including qualitative, quantitative, formal models, or experimental designs.⁵ Because a single paper could use multiple different methods, these categories are not mutually exclusive. Figure 37.2 depicts the share of articles using each method over time. Consistent with anecdotal claims, there seems to be

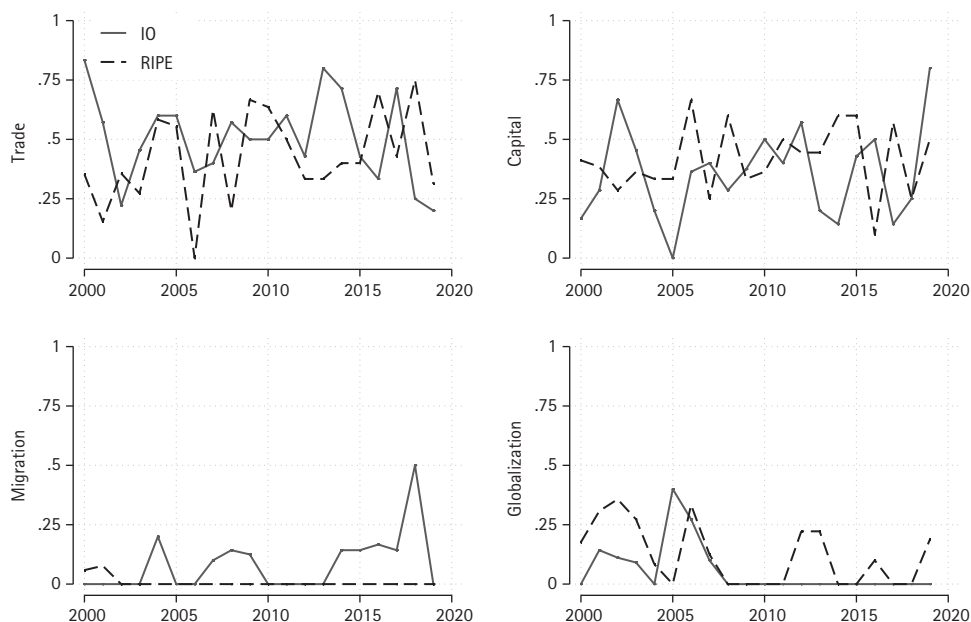


FIGURE 37.1. Share of articles in IO and RIPE, by topic.

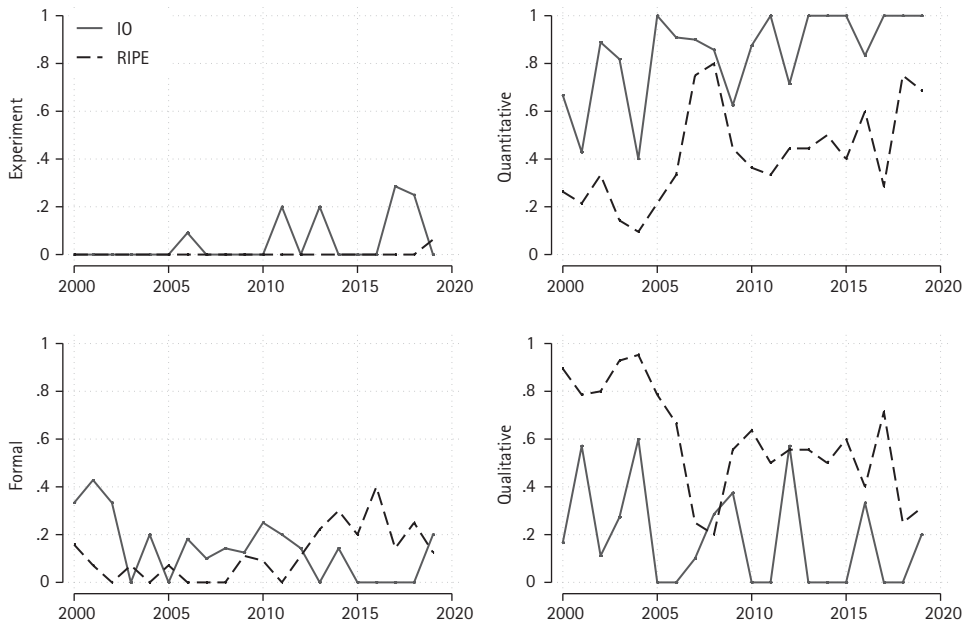


FIGURE 37.2. Share of articles in *IO* and *RIPE*, by method.

a preference for quantitative methods in *IO* and for qualitative methods in *RIPE*—although quantitative methods are becoming increasingly common in both journals. Formal and experimental approaches to liberalization are relatively uncommon.

Finally, we attempted to identify the emphasis in each study. We coded whether each study emphasized the role of the US, MNCs or firms, policy autonomy, state power, ideas, and whether a concern for (causal) identification was mentioned.⁶ Any one study could again fit into multiple categories, for example using an instrumental variables approach to explore the influence of US MNCs in eroding state autonomy. Figure 37.3 depicts the trends in emphasis over time. The largest differences appear over power and identification. Articles in *RIPE* more frequently reference power while articles in *IO* are more likely to discuss the identification strategy—consistent with the perception that American IPE has moved toward research that addresses smaller questions in a rigorous way. The influence of firms over liberalization appears earlier in *RIPE* than in *IO*.

Some other differences between the different research traditions stand out. In contrast to main themes in British IPE, American IPE frequently shies away from normative assessments—despite its reliance on frameworks from economics, a discipline heavily reliant on normative comparisons like “welfare analysis.” American IPE also tends to take existing institutions and power structures as given, rather than questioning their legitimacy or origins.

At the same time, we perceive increasing areas of overlap between British and American IPE. Some of these are driven by methodological innovations, which have allowed American IPE to approach topics usually more central to British IPE. The emergence of text analysis allows for the quantitative assessment of language and discourse; similarly, network models and increasingly granular data made the relations between actors, a central

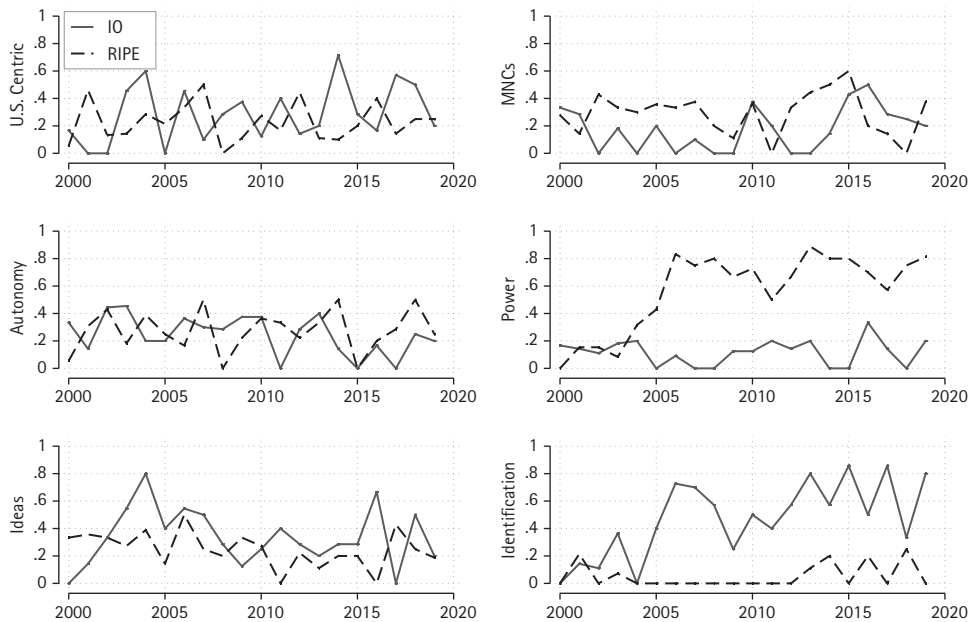


FIGURE 37.3. The share of articles in IO and RIPE, by emphasis.

issue in British IPE, amenable to quantitative analyses by those in the tradition of American IPE. Put (perhaps too) simply, by being able to measure things that were previously not quantifiable, those working in American IPE can now address topics that were previously the purview of British IPE. Overlap also arises from policy debates and on substantive grounds, such as the concern with globalization backlash and the often substantial adjustment costs to liberalization.

CONCLUSION

We oriented our discussion around theoretical models that view liberalization as driven by its broad benefits and as a public good, or driven by the gains for individual groups and as a private good. These distinct views of liberalization also suggest avenues for future research which align with current themes in both British and American IPE.

First, a growing body of work examines the relationships between different policy instruments. This literature points to questions about why governments choose specific “bundles” of policies, and liberalize one dimension but not another dimension. More generally, how governments liberalize their markets and how different types of liberalization are connected with each other remains a topic of increasing importance. One aspect here is the distributional consequences of liberalization, where the literature typically builds on theories of comparative advantage, driven by classes, industries, or firms. But just as importantly, understanding how the discourse over liberalization matters

for the acceptance and legitimacy of a government's policy choice becomes increasingly important for understanding political coalitions over liberalization and the differences across issue areas.

Second, a distinction similar to that between private goods and public goods emerges at the flipside of liberalization: the regulation of domestic markets. Especially when it comes to financial market regulations, the line between prudential regulation and predatory regulation is thin. Whether policies are intended to ensure market stability or to protect market incumbents is difficult to ascertain, but matters both for the distributive consequences of these policies and their normative assessment. Similarly, as we noted above, a growing body of literature examines the dual nature of government policies, which may be liberalizing on some dimensions but protectionist on others. For example, the recent United States-Mexico-Canada Agreement, which updates NAFTA, raises North America-specific content requirements. A framework that pits liberalization against protectionism has difficulty acknowledging the dual nature of such policy reforms.

Third, the concentration of the gains from open markets on individual firms suggest new challenges. For example, Freund and Pierola (2015) report that in a sample of 32 countries, the five largest firms on average account for nearly a third of a country's exports. These activities by individual firms are on a sufficiently large scale that they shape a country's comparative advantage in the aggregate. What accounts for the emergence of these successful firms, and what explains differences in the integration of individual firms into global supply chains, remains relatively little understood. Moreover, some of that market concentration is driven by liberalization; but large firms also have an easier time competing on international markets with other firms. Societies, and ultimately governments, will have to find a balance between the market power of individual firms, which may make them competitive on international markets, and their political power at home. This trade-off is especially challenging in the context of MNCs, which may be viewed as "national champions" but have a large share of their economic activity abroad.

Despite their difference in emphases, British and American IPE remain united by attempts to understand political processes around the liberalization of cross-border economic flows—who supports liberalization, when we observe liberalization, and how liberalization takes shape. And in that, both research traditions address topics central to the social sciences more broadly: the role of states, non-state actors, and order in politics; the political control of territorial borders and cross-border transactions; and the role of power, norms, and ideas in politics. Thus, both British and American IPE seem to take a problem-driven approach, although they may differ in both the problems they identify as important and in how they approach these problems in practice. British IPE, in general, appears more willing to focus on a case or a question stemming from an event that we do not understand well, and is thus able to engage with both policy events and dramatic events more swiftly and critically. American IPE, meanwhile, tends to raise questions that address changes "at the margin," reflecting perhaps their reliance on economic models, and questions that are raised by shortcomings of existing work and within the confines of existing theoretical frameworks. As we noted in the previous section, we see increasing areas of overlap between these two traditions, both in substance and methodology. We hope to continue to see work that advances our understanding of important topics and that transcends barriers in geography or methodology.

NOTES

1. We neglect other important aspects of liberalization, particularly the consequences of liberalization—e.g., creating backlash (Margalit 2011; Frieden 2019), new forms of interdependence (Farrell and Newman 2016), revenue shortfalls (Bastiaens and Rudra 2018), opportunities for government intervention (D’Costa 2009; Sierra 2019), or new regulatory challenges (Büthe and Mattli 2011).
2. Even leading advocates of trade liberalization have suggested that the case for capital account liberalization is much less clear-cut from an economic perspective (Bhagwati 2004).
3. Trade agreements now frequently include a set of non-trade issues, including investment regulations, which has altered the political coalitions around these agreements (Manger 2009; Lechner 2016).
4. The graduate students searched for any articles that included the following terms: “restriction,” “liberalization,” “openness,” “tariff,” “barrier,” “capital,” “trade,” or “migration.” They then limited the articles to those that explain economic liberalization or restrictions. One student coded all resulting *IO* articles and the first 79 *RIPE* articles; the other coded the first 200 *RIPE* articles and the most relevant 83 *IO* articles. Most of their codings (the binary codings in particular) accorded perfectly.
5. Studies are coded as using experimental designs if the researcher was able to randomly assign treatment. Natural or quasi experiments are excluded. These would frequently be captured by the identification coding discussed below.
6. We employ a liberal definition of concern for identification challenges, e.g., a Granger causality test, inclusion of specific controls, or lagging independent variables is sufficient—as long as the justification is to find causal identification. Instrumental variables, natural experiments, or regression discontinuity are also included. Our objective is not to assess the quality of the identification strategy.

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CHAPTER 38

THE INVESTMENT TREATY REGIME

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INTRODUCTION

THE investment treaty regime is one of the most potent and controversial areas of global economic governance. It is also one of the strangest. Consider these facts:

- North Korea has signed more than 20 bilateral investment treaties. Most are in force.
- As of 2022, Spain has been a respondent in as many investment treaty claims as Venezuela.
- When told of Pakistan's first investment treaty claim, Pakistan's Attorney General had to look up "ICSID" on Google (Poulsen 2015). Almost two decades later a tribunal told Pakistan to pay \$6 billion in compensation to a foreign investor; the equivalent of its entire International Monetary Fund (IMF) bailout that year.
- In 2019, Libya was told to pay almost \$1 billion in compensation for lost future profits on a project that had cost the investor \$5 million.
- When Argentina resisted paying awards of about \$1 billion in the aftermath of its 2001 economic crisis, a US hedge fund had a Ghanaian court temporarily seize an Argentine naval vessel with more than 200 sailors.
- Arbitrators in investment treaty claims are typically private lawyers. In about half of all claims, they have acted as legal counsel in other investment treaty disputes *at the same time* (Langford, Behn, and Lie 2017).

Despite often jaw-dropping features, IPE literature took little notice of the investment treaty regime until recently. The decade after investment treaty arbitration took off in the early 2000s, the focus in IPE literature was mostly on a few narrow questions, such as whether the treaties impact the flow of foreign direct investment (FDI). Recent literature has moved on, but not always in the most productive direction. Methodologists and economists often frown at research designs in the literature—including work in leading journals. Much scholarship remains devoted to the standard country-level time-series approach, despite extremely

poor data. In addition, parts of the literature have been based on poor understandings of how international investment law actually works, resulting in sometimes questionable modeling choices. Combined with editors discouraging negligible effects and “mere” descriptive inference, IPE scholarship occasionally produces causal claims about the investment treaty regime that rest on highly shaky foundations and few investment lawyers and other actors within the regime would recognize as credible. In other cases, findings that have been well-known among practitioners for ages are presented as cutting edge and novel within the IPE bubble.

It is not unusual for a nascent subfield to take time to find its feet, of course, and few contributions will stand the test of time anyhow. There is even a parallel to investment arbitration itself, where poorly reasoned tribunal decisions from early years don’t trouble some practitioners, as “good awards will chase the bad, and set standards which will contribute to a higher level of consistent quality” (Paulsson 2006, 13). Others are less optimistic, however, as wrong tribunal decisions do not seem to die in investment law: awards that have been set aside or annulled continue to be cited as authority over and over again (Alschner 2020). If IPE scholarship on the regime is to flourish and be credible, it should avoid making the same mistake as the arbitrators it is studying.

This is more than just navel gazing. Policymakers and stakeholders around the world have been looking for ways to reform it for more than a decade. As economists have largely ignored the regime (mainly because of the data constraints), policymakers have looked to empirical IPE literature instead. Arbitration lawyers and anti-globalization non-governmental organizations (NGOs) routinely use (and abuse) IPE scholarship to justify pre-existing policy positions, and in some cases IPE scholarship appears to have played a genuine role in updating beliefs about the regime among important actors. Policy relevance is not a research goal in itself, but as this is one corner of IPE relied upon by practitioners the stakes are higher than just the publication game. Fortunately, recent work has begun to root research questions and designs in a much more solid understanding of how international investment law works (e.g., St John 2018; Berge 2021; Calvert 2022; Williams 2022) and major advances are underway towards greater methodological rigor and sophistication (e.g., Moehlecke 2019; Strezhnev 2018). This gives reason to be optimistic that at least for this subfield of IPE, the best is yet to come.

The chapter is in three parts. The first asks how we should conceptualize and measure the investment treaty regime. It also points to the many open questions surrounding the regime, particularly in the context of dispute settlement. On this basis, I outline what we know about the economic justifications of investment treaties—a surprisingly understudied subject—and proceed to the political economy of investment treaty adoption. I briefly conclude with reflections on how IPE can provide more valuable contributions to our understanding of the investment treaty regime going forward.

THE INVESTMENT TREATY REGIME: CONCEPTS, MEASUREMENTS, AND QUESTIONS

The modern investment treaty regime consists of three components. The first is investment treaties. The second is the rules, institutions, and treaties that together provide the

governance framework for investment treaty arbitration, and the third component is the decisions of tribunals.

Identifying and Measuring Investment Treaties

There are different ways to conceptualize an investment treaty. One definition is that they are treaties between two or more states, which *focus mainly or exclusively on investment protection and include a broad and binding consent to investor–state dispute settlement*. This definition is implicitly used in much IPE literature. It covers most “bilateral investment treaties” (BITs) which have mainly been negotiated between developed and developing countries (North–South) or among developing countries themselves (South–South). The treaties provide protections against an extensive array of political risks. This typically includes uncompensated expropriation (including indirect, or “creeping” expropriation), discrimination, capital transfer restrictions, losses from war or civil strife, breaches of investor–state contracts, as well as “unfair” and “inequitable” treatment. One challenge to this conceptualization, however, is that it excludes a considerable number of BITs that do not include broad and binding investor–state arbitration provisions. This includes most BITs signed before the mid-1980s but also a number of recent agreements (see Table 38.1). For instance, the most cited IPE study on BITs include treaties from 1960 onward (Elkins, Guzman, and Simmons 2006), but once removing “old generation” BITs the results are no longer statistically significant (Poulsen 2015, 110–13).

Another challenge with this definition is that scholars are usually interested in studying investment treaties because of the functions they can play for investor–state relations and if the focus is on investment protection and/or investor–state dispute settlement, investment chapters in some free trade agreements (FTAs) should also be considered. This is acknowledged in much IPE literature, which thereby seems to conceptualize investment treaties as treaties between two or more states, which *include provisions on investment protection and a broad and binding consent to investor-state dispute settlement*. Yet, if this is our

Table 38.1. What Is “an Investment Treaty”?

	Treaty-based ISDS	No treaty-based ISDS
Mainly, or exclusively, protection obligations	E.g., most BITs since 1980s	E.g., early BITs; postwar Foreign Commerce, and Navigation Agreements
Include protection obligations	E.g., Canada–EU FTA; European Convention of Human Rights; Energy Charter Treaty	E.g., US–Australia FTA; UK–Canada FTA; recent Brazilian BITs
Mainly, or exclusively, non-discrimination and market access obligations	Very rare (e.g., China–Australia FTA)	E.g., General Agreement on Trade in Services; EU–China Comprehensive Agreement on Investment; European Free Trade Association; European Union

understanding of investment treaties then merely adding investment chapters in FTAs to our samples wouldn't be enough. Scholars would also have to consider including the Energy Charter Treaty (ECT) for instance. While not mainly focused on investment protection, the ECT includes the standard panoply of BIT protections and is the basis for a very large number of investment treaty arbitrations. In fact, one could even go further and consider whether some studies may even need to count the European Convention on Human Rights (ECHR) as an investment treaty, since it gives foreign investors the ability to pursue international claims against host states for uncompensated expropriation. If these agreements are included, however, it raises questions about how to count them. Is the ECT equivalent to 40-plus "BITs" for an ECT member for instance? Equally, does it mean dyadic models should consider an "investment treaty" is in place between, say, Germany and the United Kingdom? There are no easy choices here, but parts of the IPE literature could provide clearer justification of what is actually understood by "an investment treaty" as this has knock-on effects for research designs.

In fact, if the aim of the study is to focus on the role of treaties for investment regulation more broadly—rather than just protection and arbitration—then scholars would need an even less demanding definition. In this case, the sample should include treaties between two or more states, which *include substantive obligations for the parties to treat foreign investors in a certain manner*. This has the benefit of capturing all BITs, ECT, and FTAs with investment chapters—unlike the definitions above—but it raises its own set of challenges. For instance, postwar Foreign, Commerce, and Navigation (FCN) treaties were the main investment treaties negotiated by the US for decades (Vandeveld 2017). The agreements were broader, but other obligations (e.g., on trade and employment rights) were ultimately also targeted toward investment as well. As noted by a former American negotiator, "[i]n a real sense . . . the FCN treaty as a whole *is* an investment treaty; not a mosaic which merely contains discrete investment segments" (Walker 1956, 244). Many FCNs are still in force and some have been used in dispute settlement in recent decades. FCN agreements do not include investor–state arbitration provisions, but neither did early European BITs and if early European BITs count as "investment treaties" it would be hard to justify excluding postwar US FCN agreements (Poulsen 2020). A former US negotiator even refer to FCNs as "the first bilateral investment treaties" (Vandeveld 2017). Also, a considerable number of treaties include investment establishment obligations and/or limit protection standards to non-discrimination, such as the European Free Trade Area (EFTA), a large number of FTAs and even the World Trade Organization (WTO)'s General Agreement on Trade in Services (GATS).

Variations in treaty designs raise challenges as well. Even among "modern" BITs there are important differences. North American BITs have included establishment obligations, for instance, whereas European BITs have typically not. This will matter in some cases. To cater for such design variation, a growing trend in IPE is to use continuous index measures of treaty "strength." But while intuitively attractive to plug into regression analyses—and made easier by recent online databases—this often involves adding up fundamentally different provisions and design choices based on implicit assumptions about relative rankings and weights that are difficult if not impossible to justify. It is rarely, if ever, meaningful. Ordinal scales will often be more helpful as long as they sort on dimensions grounded in what we actually know about different treaty obligations. For instance, it is meaningful to rank a treaty with a broad and binding consent to investor–state arbitration as "stronger" than one without, even if we don't know *how much* stronger. By contrast, it is not clear whether a treaty

with consent to arbitration at the International Centre for the Settlement of Investment Disputes (ICSID) should be seen as “weaker” or “stronger” than one with consent to arbitration based on International Chamber of Commerce (ICC) or other rules (Yackee 2014). Nor is it clear how we can rank, say, a full protection and security clause against a national treatment clause in the absence of more specific information about the dimension that is being considered. In fact, given how little we know about the role of investment treaties for investors and other actors it will sometimes be prudent to stick with nominal scales distinguishing between different types of treaty designs without associated judgments about ranking.

In some cases, even this will not solve challenges on how to cater for most-favoured-nation (MFN) provisions. Arbitrators have allowed investors to “import” provisions from other treaties, which can undermine rank-orders across different treaties (at least in the context of substantive obligations). Nor will it address the challenge of “treaty-shopping.” Many investment treaties only require a superficial relationship with a jurisdiction to be considered an investor of that state, which in turn can facilitate claims through shell companies in third states. The permissive approach of tribunals toward shareholder claims is an equally, if not more, important source of treaty shopping (OECD 2018). The upshot is that while investment treaties have largely been negotiated as bilateral deals, meaningful coding will often have to consider their multilateral characteristics.

The Governance of Investment Treaty Arbitration

The second component of the investment treaty regime is the frameworks governing investment treaty arbitration. Under any definition, most modern investment treaties allow investors to pursue treaty claims against host states themselves. Covered investors can do this without first having to exhaust local remedies or ask their home state for permission. The mechanism is unique in some respects, as I’ll outline below, but not in others. For instance, investment treaties are not the only instruments to provide access to binding and enforceable investor–state arbitration. Domestic investment laws and “internationalized” contracts can do this as well. There are important differences between consent to investor–state arbitration in treaty, contract, and law; the latter can be unilaterally changed for instance. But particularly when research questions center on investor–state arbitration as a credible commitment instrument, it is not clear why contracts should be excluded (unless the focus is on small investors who may be unable to negotiate favorable contracts). In fact, investment contracts will often include a range of project-specific provisions that can be superior from the perspective of investors and while some corners of IPE literature have suggested that contract arbitration is somehow unenforceable, this is incorrect (Yackee 2008; Arato 2015). The more pragmatic justification, of course, is that investor–state contracts are rarely publicly available, but at a minimum it is important to acknowledge that focusing only on treaties often underestimates the extent to which investor–state arbitration is available to foreign investors.

Investment treaty arbitration is about acts taken by governments in their sovereign capacity. It is public international law. Yet, the mechanism is based on a system otherwise mainly used to settle disputes between private parties. Arbitrators are appointed by the parties themselves—unlike in the WTO for instance—and in theory the pool of arbitrators is

limitless. In practice, however, arbitrators are mainly drawn from a small club-like community of mostly private lawyers (Puig 2014). The system is ad hoc, often very flexible, and there is no real possibility for appeal. Arbitral institutions often assist tribunals and play important roles behind the scenes—for instance by providing advice and editing of drafts (Tucker 2018, 84–6)—but they are distinct from tribunals themselves, unlike with the WTO's Appellate Body or the International Court of Justice (ICJ).

Once appointed, arbitrators—typically three—have significant powers to rule on their own jurisdiction, decide the merits of the case, and award damages to be paid to investors in case a breach is found. Tribunals have only rarely used non-fiscal remedies. This is an important difference from many domestic legal systems, where compensation is used as a remedy on occasion—for instance in expropriation claims—but rarely when dealing with unlawful administration action. It is also different from remedies in the trade regime, where focus is on restoring compliance. If a government does not comply with arbitral awards, investors can proceed to confiscate commercial assets of the host state around the world. This right is enshrined in the New York Convention on the Enforcement of Arbitral Awards as well as the ICSID Convention. Non-compliance may also trigger sanctions from home states of investors. In practice, however, these mechanisms are rarely invoked. This is likely due to reputation concerns among host states, although this has yet to be subject to empirical testing. Also, many claims are ultimately settled on agreed terms between the parties, as is common in commercial arbitration, though this is also an area largely untouched by empirical studies to date.

Only a small IPE literature has begun to study the governance and politics of investment treaty arbitration. Core questions remain largely unaddressed. The role of international institutions underpinning the regime is one (on promotion of BIT models, see Alschner, Elsig, and Wuthrich 2021). UNCTAD promoted investment treaties in the early 2000s, but has since become much more sceptical about the regime. The Organisation for Economic Co-operation and Development (OECD) has also grown increasingly critical, cautioning, for instance, against the challenges of investor protections going beyond those in advanced systems of corporate law. By contrast, the World Bank continues to promote the treaties in developing countries (see generally Poulsen 2015; St John and Berge 2020) and the Energy Charter secretariat has promoted the Charter in African states while much of the membership has called for reforms. (Legal staff within the IMF was never a friend of investment treaties, as most treaties clash with the Fund's Articles of Agreement by preventing capital controls in case of balance of payments difficulties; e.g., Siegel 2013).

Other questions relate to the identity of the adjudicators. The fact that a small cadre of closely networked private lawyers have been delegated considerable governance functions in the investment treaty regime raises pertinent questions; not least since many act as both counsel and arbitrator across disputes as mentioned in the introduction. Do investment arbitrators hold shared views of appropriate government conduct and if so, what does this imply for global economic governance (e.g., Tucker 2018; Bonnitcho and Williams 2020)? Also, sociologists and political scientists have pointed to the role of arbitration practitioners in creating and sustaining the regime of international commercial arbitration (Dezalay and Garth 1996; Hale 2015), yet IPE scholars have thus far shown little interest in the political agency of arbitration lawyers (Poulsen 2015, ch. 4).

Finally, the European Union (EU) has pursued a different mechanism of investment treaty arbitration that allows for appeal and does away with party-appointed arbitrators.

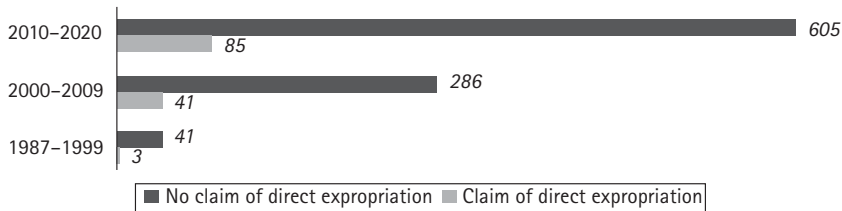
In other cases, modern investment treaties have either limited or removed the option of investor–state arbitration entirely. At the time of writing, these initiatives have had a limited impact, but they raise critical questions about exactly how the governance frameworks for investment treaty arbitration impact the investment treaty regime. Also, when recent investment treaties have removed the right for investors to pursue claims themselves—as in old-generation investment treaties—we are back to the definitional issue of what exactly is meant by “an investment treaty” and the “investment treaty regime,” and what their purposes are if not to provide the basis for investor–state claims.

Investment Claims and Decisions by Tribunals

Modern investment law is primarily based on decisions of arbitral tribunals filling in the blanks of brief and vague treaties. Tribunals do not always agree, as I’ll return to below, but they routinely refer to each other’s rulings and have converged on shared views in important areas of investment law. As such, tribunal decisions are not just important for the resolution of individual disputes but perform fundamental lawmaking and governance functions. This is important for how we understand and study the modern investment treaty regime. At a basic level, it means the regime—as understood here—was only really established by the turn of the century (see also Hepburn et al. 2020). The first treaty-based award came in 1990, but it was not until 1999 that a NAFTA award against Mexico opened the floodgates of investment treaty claims and subsequent tribunal decisions. The vast majority of investment treaties had already been signed by then and it was 50 years after the initiation of the US postwar FCN program and 40 years after the first European BIT. Although sometimes glossed over in IPE literature, it is difficult to exaggerate the practical and political relevance of this gap between investment treaty adoption and the much later rise of investment treaty arbitration.

Claims don’t always have to be made public, but we know of more than 1,000 brought to date. About half of all known claims have been resolved in favor of the investor or settled on often unknown terms. Awards can be very large, and a growing number involve billions of dollars. Disputes have been most prevalent where investors tend to make large investments with significant sunk costs—such as infrastructure and natural resources. Most claims have been against developing countries. This is where political risks tend to be highest, but it is also the main countries with which developed countries have signed investment treaties. The few investment treaties *among* developed states—such as NAFTA and the ECT—have been the basis for a high number of claims, particularly in recent years. Claimants tend to be large or very large investors. This is not surprising given the very high costs of investment treaty arbitration. There are examples of small and medium-sized firms filing claims, but most compensation arising from investment treaty arbitration has been awarded to very large firms and extremely wealthy individuals (Van Harten and Malysheuski 2016). Third-party financing of investment treaty claims—a phenomenon driven largely by the rise of “mega-awards”—also mainly goes to large investors (Dafe and Williams 2020).

The often broad and vaguely drafted treaties have been used by investors to seek compensation for a very wide range of government decisions, including environmental regulation, public health regulation, taxation, court decisions, contractual relations, and much more. A narrative in parts of IPE scholarship is that this is a change from early investment treaty claims, which instead were more about outright takings. Some basic descriptive



Notes: Figure shows initiated investment treaty claims separated by whether uncompensated direct expropriation was among alleged breaches.
Source: UNCTAD.

FIGURE 38.1. Rarely about outright sovereign theft.

statistics can help inform this debate, as claims about outright expropriation—what some IPE scholars refer to as “sovereign theft”—have always been a very small minority (Figure 38.1). This includes early claims. Only three out of 44 known claims brought by the end of the 1990s were about direct expropriation. Two small claims were brought by individuals and the third was brought against an expropriation taking place in the 1970s. Unlike in the trade regime, claims about nationality-based discrimination have also been rare. Instead, the most important substantive provision in the first “wave” of claims was the indirect expropriation clause but since then by far the most important provision has been “fair and equitable treatment”—the “super-standard” of investment law. Tribunals have sometimes taken inconsistent, and sometimes contradictory, positions on this and other core provisions. This has been subject to growing focus in recent IPE literature, and rightly so, but often the more important area of inconsistency surrounds compensation standards. Here, limited treaty guidance again leaves major discretion to tribunals to determine the size of awards, which has resulted in very different fiscal implications for similar breaches and fact-patterns. It also provides the background to understand the rise of “mega-awards” typically based on assumptions about expected future income—sometimes many decades into the future.

The rise of investment treaty claims leaves considerable low-hanging fruit for IPE scholars, which until recently focused mainly on treaty adoption. At a basic level, we know very little about when investors rely on investment treaty arbitration to resolve grievances compared to other legalized mechanisms—such as contract arbitration or court adjudication—or informal mechanisms, such as firm lobbying or home state diplomacy. Apart from the basic descriptive patterns mentioned above, we also know little about *which* investors rely on the treaties and why. Most treaties cover not just multinational corporations, but also many other foreign investors, such as private individuals, financial institutions, banks, and shareholders. Many also provide protections to quasi-sovereign investors—like state-owned enterprises and sovereign wealth funds—who have been allowed to pursue claims *as if* they are private investors. Important questions also remain about when investors bring claims as a last resort or as part of managing their ongoing relations with the host state (Wellhausen 2019). Concerns with regulatory chill have been salient in political debates about the investment treaty regime in recent years - increasingly in the context of climate change (OECD 2022; Tienhaara et al. 2022). Yet, IPE has provided little systematic evidence to date about how claims actually impact regulation (Tienhaara 2011; Moehlecke, Thrall, and Wellhausen 2019) or investor–state bargaining takes place “in the shadow” of international investment law (Post 2014; Moehlecke 2019).

Moreover, a range of questions arise about how the governance frameworks of investment treaty arbitration impact actual investment claims and tribunal decisions. For instance, with millions to earn on each case, are there ways to test the often-raised concern that arbitrators have expanded the scope of their own jurisdiction so as to raise the number of claims foreign investors can file against governments and thus increased the demand for their own legal services? More specifically, what has party appointments meant in practice and to what extent do the “private” arbitrators pursue their own material interests compared to societal norms within the arbitration community? Equally, does the design of the dispute settlement mechanism bias investors over states? Or is it perhaps more meaningful to talk about a bias in favor of “claimants” as opposed to “investors” or “capital” given the possible managerial inefficiencies that can result from tribunals’ approach to shareholder claims and corporate legal form (Arato 2021)? What does bias even mean in this context, and how can we measure it? Lawyers have different answers to these questions, but IPE scholars have largely stayed clear of these discussions thus far (exceptions include Streznev and Puig 2017a, 2017b).

ECONOMIC JUSTIFICATIONS: EMPIRICS AND THEORY

The costs and controversial features of investment treaty arbitration raise pertinent questions about the justification for the regime. But while a poster child for anti-globalization movements (Walter 2001), the justifications and normative underpinnings of investment treaties have been left almost exclusively to lawyers to assess. Here, I’ll address the *economic* justifications for investment treaties. The spotlight will be on investment treaties focusing on investment protection and allowing for investor–state dispute settlement. In theory, the treaties can enhance efficiency—more specifically Hicks-Kaldor efficiency—by addressing three problems in investor–state relations: “hold-up,” “fiscal illusion,” and/or discrimination (Bonnitcha and Aisbett 2013). The three problems are analytically distinct but can often overlap in practice. Before addressing them, however, I’ll first outline what has been the focus of most IPE literature, namely whether the treaties impact investment *flows*—a related, but distinct, question.

Impact on Foreign Investment Flows

The implicit assumption in much IPE literature is that if investment treaties promote aggregate FDI, they “work” for host states. This assumption requires some modification. The economic effects of FDI vary significantly across industries and economic and institutional conditions (e.g., Blomstrom and Kokko 2003; Alfaro and Charlton 2007). Also, just as FTAs can both “create” and “divert” trade, so can investment treaties result in diversion of investment from one host state to another (Elkins, Guzman, and Simmons 2006), or even from domestic to foreign investors. Equally, when investors restructure their investments through corporate entities in third countries it would show up as an increase in investment between treaty parties but the treaty itself would not have actually “created” any new investment. A

final complication is that investment treaties do not just protect FDI but also many types of portfolio investments. With these caveats in mind, the literature is nevertheless important as the treaties were historically justified by developing country governments by their promise to promote FDI as mentioned above. In theory, this could happen through two main, and complementary, causal mechanisms. An investment treaty can act as a costly signal to imperfectly informed investors that the host state offers a favorable investment climate. In addition, investors protected by the treaty can also see an investment treaty as a credible commitment that the host state will not expropriate or otherwise mistreat their investments post-establishment.

The empirical literature assessing the impact of investment treaties on FDI have produced mixed results but the thrust of the literature suggests the impact is, at best, very limited (see Brada, Drabek, and Iwasaki 2020). Much of the literature is plagued by estimation challenges however. Governments typically signed up to investment treaties while simultaneously implementing investor-friendly domestic policies. Another endogeneity challenge is that many investment treaties were signed due to the possibility of future FDI between the parties, which raises concerns with reverse causality (Aisbett 2009). Secondly, and as discussed above, there is relevant heterogeneity among treaties, investors, and host states. Some studies have tried to address these challenge (e.g., Yackee 2008; Haftel 2010; Peinhardt and Allee 2012; Kerner 2014; Kerner and Lawrence 2014; Danzman 2016), but significant data constraints remain. The major concerns with FDI data have prompted a few studies attempt to use other dependent variables (e.g., Jandhyala and Weiner 2014) or rely on qualitative data. For instance, a 2009 survey of in-house legal counsel in large American firms found that investment decisions hardly ever depend on investment treaties (Yackee 2009). This is notable since investment treaties don't "cost" firms anything—they are somewhat akin to a regulatory subsidy—and firms are therefore prone to responding they appreciate the treaties even if they've never used them in practice. Another survey found that underwriters in the political risk insurance industry rarely concern themselves with investment treaties when considering the availability and pricing of political risk insurance (Poulsen 2010). It is unclear whether these results look different today after investment treaty arbitration has become more widely used.

Impact on Applied Investment Regimes: Revisiting Internalization

In theory, investment treaties can also promote investment through a third mechanism, whereby treaty compliance enhances property right protections for *all* investors—foreign as well as domestic. This channel requires that governments internalize their treaty obligations in their decision-making practices; a core premise in some corners of IPE (e.g., Arias, Hollyer, and Rosendorff 2018; Bodea and Ye 2020) and among both regime advocates praising the trickling down of treaty obligations (e.g., Echandi 2011) and critics concerned with the same (e.g., Cutler and Lark 2020). Qualitative work on the internalization of investment treaty obligations is relevant here. For while there is variation across countries, only few developing country governments appear to have had more than a rudimentary awareness of their investment treaty obligations until recently (Poulsen 2015), and hardly

any have established mechanisms to promote awareness among officials (UNCTAD 2010). Even when bracketing uncertainty about what core substantive standards actually compel or deter, very few governments have internalized their obligations in administrative, legislative, and judicial decision-making in the absence of claims or threats of claims. The conclusion from a compilation of case studies of Asian states, for instance, is worth quoting at length:

[T]he impact of investment treaties on governmental decision making has been minimal. The conclusion of investment treaties has led generally to little or no internalisation in the countries covered. Moreover, while experience with ISDS appears to have triggered at least some internalisation, even following ISDS the level of internalisation has remained generally low and largely limited to ad hoc, informal informational measures.

(Calamita and Berman 2022, 4)

This conclusion comes as no surprise to practitioners in the regime. It also follows from previous qualitative work (e.g., Sattorova 2018) and is in line with what we know about expectations among developing countries when negotiating the treaties (see below). It is not controversial, but it is crucial as assumptions about internalization are critical when social scientists make predictions about the impact of the regime on state behavior.

Also, even for those states have internalized their obligations, it is worth noting that this is not necessarily evidence that the treaties promote “good governance” or the “rule of law.” As most investment treaties allow foreign investors to sidestep domestic legal systems and go straight to investment treaty arbitration, they could theoretically result in less pressure by foreign investors for the host state to promote property right protections and judicial due process (Ginsburg 2005). Moreover, if arbitral decisions have resulted in over-protection of foreign investors by causing “regulatory chill” in areas such as health or climate change, this may promote investment but hurt overall economic development—as I’ll return to below.

Hold-Up, Fiscal Illusion, and Discrimination

Much IPE literature on the impact of the investment treaty regime is based on implicit assumptions about the economic justifications of investment protection obligations. The typical reference is the potential to address hold-up problems (van Aaken 2009). Here, an investor seeks to invest in a host state but the government has an incentive to partly or fully expropriate the investment after it has been made. Rational investors will therefore avoid making their investments in the first place, to the detriment of both the investor and the host state. Investment treaties can theoretically address such “obsolescent bargaining” by increasing the cost of expropriation for the government.

In practice, hold-up problems are most likely to incur in sectors with large sunk costs relative to overall costs—such as oil and gas. Yet even there, investors can structure their investments in ways that reduce their expropriation risks, such as building links with the host economy or bringing in technology and managerial improvements throughout the lifespan of the investment. In addition, traditional models of obsolescent bargaining are a one-shot game, but in practice repeated interactions between the host state, the investor, and other investors make reputation effects an effective disincentive to expropriate. There are also effective legal instruments to protect against hold-up problems other than investment treaties. In most Western countries, domestic courts can be trusted to protect both domestic

and foreign investors against expropriation and above I mentioned how foreign investors can also negotiate investor–state contracts backed up by investor–state arbitration that include the same, or greater protections, as investment treaties.

If governments do not give sufficient weight to the consequences of regulation and legislation for foreign investors, it can result in what economists call “fiscal illusion” (Blume, Robinfeld, and Shapiro 1984; Aisbett et al. 2010). In theory, investment treaties can address this problem by forcing governments to internalize the costs of their measures on affected investors. In this context, however, questions arise about the potential efficiency reducing effects of investment treaty protections. If investment treaties are important for foreign investors’ decision on where to invest (see above) some models of fiscal illusion can result in “over-investment,” where investment projects proceed even if they have a net negative pay-off. This is because the rational investor knows she will *always* be compensated for government interference, even if such interference provides net benefits overall (e.g., environmental regulations where the social benefits outweigh costs on the investor) (Aisbett et al. 2010). Equally, investors may be less likely to avoid production techniques and structures with high negative externalities, knowing that government interference will anyway be accompanied by full compensation. Secondly, if the government fully internalizes the costs of investment treaty arbitration it may result in “under-regulation”; what some observers refer to as regulatory chill (Tienhaara 2011; Pelc 2017; Moehlecke 2019). This occurs when the costs of compensation are greater than interfering with—or shutting down—a project with high social costs. If the social benefits of regulation are not incorporated into considerations about compensation to foreign investors, it can result in both over-investment and under-regulation. Importantly, the costs of both will be borne by the host state. One solution to these challenges is for treaty drafters and tribunals to target liability rules to opportunistic host state conduct (Bonnitcha and Aisbett 2021).

In practice, there is little systematic evidence on whether governments undervalue the costs of their actions on foreign investors. Although plenty of anecdotes can be identified where foreign investors are mistreated, empirical evidence to date suggests that foreign-owned firms are not necessarily treated any worse than domestic investors. I will return to this below. And since multinational corporations and other foreign investors are a small and concentrated constituency in host states, it is likely that governments are more concerned with their interests than costs imposed on a dispersed majority. If so, then the real “fiscal illusion” could be government failure to internalize other social costs, for instance to communities affected by foreign investors. Even if some governments do not fully consider the cost of their behavior on foreign investors, investment treaties will only address this problem if governments internalize the potential costs of investment treaty arbitration. This again relates to questions for IPE scholarship introduced above: to what extent are government agencies interacting with foreign investors actually aware of their investment treaty obligations? And even if they are, what incentives do government officials have to internalize treaty constraints? An added complication is that under-investment can be because of risk aversion on the part of the foreign investor rather than due to fiscal illusion. The enterprise may be concerned that governments will interfere with their project in the future when the negative externalities of the project become apparent. In that case, however, the more efficient response is not an investment treaty but investment insurance. This internalizes the costs of the risk without incentivising the investor to proceed with projects that have net costs overall. IPE scholarship has increasingly pointed to the large, and sophisticated, public and

private markets for political risk insurance in recent years (e.g., Arel-Bundock, Peinhardt, and Pond 2019), but the link with the investment treaty regime remains understudied (Poulsen 2010; Peinhardt and Allee 2016).

Whereas hold-up problems and fiscal illusion address the relationship between the foreign investor and the host state, investment treaties can theoretically also promote efficiency by establishing a level playing field between the foreign investor and her competitors (Stiglitz 2007). Only a minority of treaties include binding non-discrimination provisions on the conditions and restrictions for new investment (pre-establishment), however, so focus here will be on efficiency considerations regarding national treatment provisions for already established investment (post-establishment). Here, the premise is that host states are prone to discriminate in favor of domestic investors, which makes binding treaty obligations necessary to establish competitive equality between like firms. This is an often-made justification for the treaties by policymakers, yet there are significant caveats to this justification as well. For despite their non-discrimination provisions, investment treaties are ultimately preferential instruments by providing rights to foreign investors that are often unavailable to domestic investors. It is partly for that reason that libertarians (Lester 2015) and some economists (Aisbett and McAusland 2013) have been critical of the treaties. The most important right is the ability to sidestep domestic courts and instead take disputes with host states directly to international dispute resolution. This is in contrast with most other areas of public international law with private access to dispute settlement. In the ECHR, mentioned above, foreign investors can also file expropriation claims against host states, but they first have to exhaust reasonably available local remedies.

Apart from access to a unique dispute settlement system, foreign investors are also privileged by substantive provisions in investment treaties. For instance, the two provisions most frequently invoked in disputes—“fair and equitable treatment” and “indirect expropriation”—are independent of domestic laws and regulations as well as the treatment of like domestic investors. There is an ongoing debate among legal scholars, whether tribunal interpretations of these provisions have granted foreign investors protections exceeding those granted to domestic investors in advanced industrial countries. One indication that they may is that some developed country investors have begun to restructure their ownership to use investment treaties against their “own” government.¹ In addition, the permissive attitude to shareholder claims in investment treaty arbitration mentioned above goes beyond advanced systems of national corporate law, where shareholders can only pursue claims for *direct* losses (e.g., voting rights) rather than separate claims for losses in an enterprise (OECD 2018). Finally, and as mentioned, remedies will often be different in investment treaty claims and claims under municipal law, as the latter are less likely to offer financial compensation for administrative decisions (except in expropriation and contract claims).

One argument for granting special rights to foreign investors is that it makes up for special rights granted to domestic investors by administrative and judicial institutions in host states. Yet in practice, there is little evidence to support the premise that foreign investors are mistreated compared to domestic investors. Although there are few systematic studies available, those that do assess relative treatment of foreign and domestic investors do not find that host states systematically treat foreign firms less favorably than their domestic competitors (Desbordes and Vauday 2007; Aisbett and McAusland 2013; Aisbett, McAusland, and Poulsen 2021). This should be an important subject for future studies in IPE. Finally, investment treaty arbitration is very expensive and may therefore only realistically be available to

large foreign investors as mentioned above. This raises additional questions for IPE scholarship about the impact of the investment treaty regime on competitive equality between small and large foreign investors.

POLITICS OF INVESTMENT TREATY ADOPTION

Most IPE literature on the investment treaty regime has focused on why—and how—governments adopted the treaties. Focus has mainly been on “modern” BITs dealing mainly or exclusively with investment protection and allowing for investor–state dispute settlement and the majority of studies have dealt with developing countries, as they are exposed to greater risks of claims and their adoption of investment treaties therefore pose a particular puzzle.

Developed Countries

Investment treaties emerged in developed countries, first with postwar US FCN agreements and later with European BITs. But while important aspects of the regime were promoted by corporate norm entrepreneurs, like the chairman of Deutsche Bank, it is surprisingly difficult to find evidence of broad-based corporate interest (Bonnitcha et al. 2017). Not surprisingly, national business groups were involved in developing national treaty programs, but corporate interest was typically limited to relatively few firms. The exceptions were when investment treaties were linked to public political risk insurance (Poulsen 2015; on the US see Chilton 2016) or firms had already been in disputes with host states (e.g., Manger 2008). Even the failed Multilateral Agreement on Investment (MAI) didn’t enjoy sufficient support by the international business community to launch a counter-campaign against that initiated by NGOs (Graham 2000). After the potency of investment treaty arbitration has revealed itself in recent years there has been greater corporate push to sign the treaties, but the establishment of the regime was rarely the result of widespread foreign investor lobbying and mobilization.

Developed country governments had other reasons to adopt investment treaties however. Theoretically, at least, the treaties allow home governments to distance themselves from disputes involving “their” investors and host states. Expropriation of foreign investors have sometimes been accompanied with a strong response from home governments (Maurer 2013)² and by referring investors directly to international arbitration investment treaties can “de-politicize” investment disputes by insulating them from interstate politics. Although a compelling theoretical argument, and often used in legal scholarship, it is doubtful whether the treaties have had this effect in practice (Gertz et al. 2018; Na-Kyung Lee 2019), and empirical work has thus far not found de-politicization of investment disputes to have been a driver of investment treaty adoption (e.g., St John 2018; Poulsen 2020). In some contexts, politicization of investment disputes can also be functional for cooperative outcomes in investor–state disputes (Yackee 2019). A more important driver for developed countries in the early years of the regime was to use investment treaties to bolster customary international law on investment protection (e.g., Denza and Brooks 1987). During the 1970s,

developing countries questioned Western standards of investment protection in the United Nations (UN). Through hub-and-spoke networks of bilateral treaties, developed countries could use investment treaties' standard-setting function to counter this trend. A related function was for developed country governments to use the treaties as focal points in informal diplomatic dispute resolution, where the substantive standards, such as full compensation for expropriation, could be referred to as the "correct" legal standard when engaging with host governments (Poulsen 2020). Finally, investment treaties were used to promote broader foreign policy agendas. Close diplomatic allies are more likely to sign investment treaties (Poulsen and Aisbett 2016) and in the case of the US, for instance, geopolitics was important when signing investment treaties with governments in the former Communist bloc (Vandeveldt 2009; Chilton 2016; Voeten 2021).

Important questions remain however. Corporate mobilization for the treaties is understudied; particularly the role of energy companies and private law firms. And whereas there is a large literature on the role of domestic institutions and bureaucratic politics in trade policy, we know hardly anything about why, and how, different government actors in developed countries have shaped the investment treaty regime (on ratification, see Haftel and Thompson 2013). The recent rise of claims against developed countries themselves makes this particularly relevant, as major OECD states have sought to get out of core sources of exposure, as seen through reforms of NAFTA (with ISDS omitted for US-Canada relationship), removal of the application of intra-EU BITs, and the removal of the application of the Energy Charter among EU member states (pending at the time of writing). The role of developed countries as home states in investor-state dispute settlement also raises pertinent questions, not just in the context of enforcement but also when investors have pursued claims that seemingly conflict with home states' broader public policy agendas (e.g., in the context of climate change).

Developing Countries

Rather than developed countries, most IPE literature has focused on the adoption of investment treaties in developing countries. Here, the most important driver was the expectation that the treaties would help them attract foreign investment. This is also how the treaties were initially sold to developing countries (Poulsen 2015). The literature suggests a range of different negotiation mechanisms, including prisoner's dilemma dynamics (Guzman 1998; Elkins et al. 2006), the attraction of future investment after expropriations of colonial-era investments (Bubb and Rose-Ackerman 2007), network effects (Montt 2009), bounded rational competition (Poulsen 2015), as well as the search for BITs during times of capital scarcity (Betz and Kerner 2016). Some, but not all, of these mechanisms are complementary. A related driver of investment treaty adoption by some developing countries was the expectation that the treaties could lock in domestic reforms (Manger 2009; Bonnitcha et al. 2017, ch. 8). As in developed countries, many developing countries also used the treaties for diplomatic reasons. Although de-politicization was rarely an important driver for developing country governments, the treaties were often used to cement diplomatic relations (Poulsen and Aisbett 2016). Some were also the result of a norm-diffusion mechanism where transnational mimicry and symbolism resulted in treaties being signed to signal an adherence to the Washington consensus and an international rule of law (Jandhyala et al. 2011).

Although there was imperfect information about how arbitrators would “fill in the blanks” of vaguely drafted substantive protections, many accounts assume that developing country governments knew and accepted the costs of the treaties when adopting them. For many developing countries, this assumption is misleading (Poulsen 2014, 2015). Typically, it was not until developing country governments were subject to their first investment treaty claim that they appreciated the nature of the treaties. Before then, many governments mistakenly saw BITs as “soft-law” arrangements with no significant implications for investor–state disputes. Negotiations were often haphazard both in terms of the choice of treaty partners as well as the drafting of provisions. The latter were typically copy-pasted from template agreements of Western countries as “default rules” that required little, if any, discussion. There were exceptions of course (see, e.g., Nottage and Thanitcul 2016; Berger 2019), but even arbitration practitioners supportive of the regime have noted that many developing country governments “had no idea that this would have real consequences in the real world” (Schreuer quoted in ICSID Case No. ARB/04/14, Award, 8 December 2008, para. 85).

The rise of investment treaty arbitration has resulted in greater awareness of the costs and scope of the regime. The more politicized and cautious approach to the treaties has resulted in changes to treaty design (e.g., Manger and Peinhardt 2017; Alschner 2017; Haftel, Link, and Broude 2021), as well as a general slowdown in adoption (Poulsen and Aisbett 2013), re-negotiations (Haftel and Thompson 2018; Thompson, Broude, and Haftel 2019; Huikuri 2021), and a few terminations (Calvert 2018). Exactly what this means for the regime is unclear, however, as investment treaties include survival clauses—sometimes covering existing investors for decades into the future after unilateral termination—and often provide generous opportunities for treaty shopping as mentioned above. Another critical question is how arbitrators actually respond to changes in investment treaty design (Berge 2021). The most comprehensive empirical account suggest that recent and more carefully specified treaty designs made little impact on arbitration outcomes (Alschner 2022). By reading new treaties like old ones, tribunals have pushed back against state-driven reforms.

Finally, another important development in recent years has been the role of outward flows from developing countries. There are examples of developing countries using investment treaties to protect their own investments during the Cold War as well (St John 2018; Denza and Poulsen 2020), but in recent years this has become a much more important consideration in some countries—not least China.

CONCLUSION

IPE scholarship on the investment treaty regime is rapidly expanding and becoming considerably less dull, repetitive, and narrow than the first generation of studies focusing largely on whether the treaties impact FDI flows. A core challenge going forward is to root research questions and modeling choices in a more solid grasp of how international investment law works and affects actors at the micro-level. The data constraints in the field is another major challenge and the combination of the two occasionally results in IPE scholarship that substantive experts in the field know to be either irrelevant or misguided. Although it is slowly changing, much of the credible quantitative research on the regime still comes from empirical legal scholars or interdisciplinary teams. There is much to learn here from the long

history of interdisciplinary work on trade agreements. Quantitative scholars should also consider exploring and developing richer data sources, even if that means honing in on “smaller” cases—such as a country, firm, or legal network. The recent rise of qualitative work on investment treaties should be encouraged as well. Substantively, the field could also benefit from broadening its focus. Future work could do more to better understand a broader range of investment treaties—including those focusing mainly on establishment—and identify linkages between the investment treaty regime and other regimes, such as human rights, public health, climate change, and indeed the soft law mechanisms governing investor obligations. Even within the growing body of work on investment treaty arbitration, core questions remain almost entirely untouched. The power and multiple roles of investment lawyers is one of the most contentious aspect of the regime, for instance, yet IPE scholarship has surprisingly little to say about this. The good news, of course, is that this and many other unexplored questions in the literature leave considerable low-hanging fruit for future PhD students to inform our understanding of what may be the most potent (and, for many, weird) regime underpinning economic globalization.

NOTES

1. See, e.g., ‘Power firms move ownership offshore to protect against Labour renationalisation,’ *Guardian*, 24 November 2019.
2. By contrast, Tomz shows that private bondholders were rarely able to mobilize their home governments in cases of sovereign default abroad (2007, chs 6–7).

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CHAPTER 39

INTERNATIONAL TRADE AND CONFLICT

EDWARD D. MANSFIELD

INTRODUCTION

FOR centuries, the nature and strength of the links between international trade and conflict have been the subject of heated disagreement. Until lately, however, these links remained the subject of remarkably little systematic scrutiny. Over the past four decades, a growing number of studies have attempted to fill this gap in the literature. Many of these studies have concluded that heightened trade fosters cooperative political relations and inhibits conflict. Nonetheless, some research indicates that increased trade generates political discord, and various observers insist that economic exchange has no strong bearing on the use of force.

In this chapter, I survey the empirical literature on the effects of foreign trade on political-military conflict, placing primary emphasis on the quantitative research that has been conducted. Systematic empirical analysis on this topic started to gain momentum around 1980 and has generated three “waves” of work (Keshk, Pollins, and Reuveny 2004). The initial wave took place during the 1980s and early 1990s. A second wave was linked to research on the liberal peace and the “Kantian Triad;” it took place during the 1990s and the first few years of the twenty-first century.

Since then, a third wave of research has emerged. It has aimed to address a number of issues. First, the initial two waves of research on the links between interdependence and conflict focused largely on assessing whether a relationship exists and largely sidestepped the causal mechanisms underlying any such relationship (Mansfield and Pollins 2001). A number of recent studies have made some significant headway in addressing these mechanisms. Second, most of the existing literature has focused on the relationship between the flow of international trade and conflict. The third wave of research has addressed various aspects of trade, including trade policy and trade agreements.

Third, existing studies have analyzed various types of conflict, ranging from general patterns of conflict and cooperation to militarized disputes to full-blown wars. More attention needs to be focused on determining what types of conflict should be emphasized in light of existing theory. Nonetheless, it is clear that liberal and mercantilist theories

should help to explain the onset of war. Liberal theories, however, have had a difficult time accounting for the onset of the First World War. This event occurred in the face of extensive trade involving all of the antagonists and concluded a period of pronounced globalization. A variety of recent studies have shed new light on the effects of trade on the outbreak of war, as well as the effects of war on the trade ties of combatants. Finally, the third wave of research on trade and conflict has addressed whether the effects of trade stem from market capitalism more generally and whether a simultaneous relationship exists between trade and conflict; that is, whether trade influences conflict at the same time that conflict influences trade.

THE CONCEPTUAL LINKS BETWEEN TRADE AND CONFLICT

Central to much of the literature on trade and conflict is the longstanding claim advanced by liberals and others that open international markets and heightened economic exchange inhibit interstate hostilities (Doyle 1997; Keohane 1990; Stein 1993). Advocates of this claim have emphasized a number of arguments. First, economic exchange and military conquest are both means of acquiring the resources needed to promote national security and economic growth (Staley 1939). As trade increases, there is less reason to bolster security and growth via territorial expansion, imperialism, and foreign conquest (Rosecrance 1986). Alternatively, protectionist barriers tend to precipitate conflicts of interest that can contribute to political-military discord (Viner 1951, 259). Second, overseas trade increases contact and promotes communication between private actors in different countries, as well as between governments. Various observers have argued that heightened contact and communication spur cooperative political relations (Viner 1951, 261; Hirschman 1977, 61; Stein 1993; Doyle 1997, Ch. 8).

Third, many liberals stress that international trade generates efficiency gains. These gains promote the efficient allocation to resources in society, foster growth, and render private actors dependent on foreign markets. Because the use of force can interrupt economic relations among the combatants and jeopardize the gains from trade, these actors have reason to lobby their governments to avoid military conflicts and avert the escalation of any conflicts in which they become enmeshed. Government officials rely on the support of these actors to retain office and consequentially are expected to attend to such demands. This argument has been a longstanding cornerstone of liberal claims. Montesquieu, for example, claimed that “the natural effect of commerce is to lead to peace. Two nations that trade together become mutually dependent: if one has an interest in buying, the other has an interest in selling; and all unions are based on mutual needs” (quoted in Hirschman 1977, 80).

However, liberal arguments have been challenged by mercantilists and realists who insist that open trade can adversely affect national security. Albert Hirschman ([1945] 1980; see also Keohane and Nye 1977; Baldwin 1980), for example, has noted that the gains from trade are often disproportionate and that the distribution of these gains can influence power relations between states. Changes in power relations affect the national security of countries and have been linked to the onset of military conflict (Gilpin 1981; Levy 1989; Mearsheimer 1990). Similarly, trade partners are rarely equally dependent on their commercial relationship.

Even if one partner is heavily dependent on the relationship, its counterpart may not be (either because the latter state is not dependent on trade in general or because it could easily replace the trade it conducts with its partner). Trade may do little to inhibit the less dependent state from initiating hostilities.

Critics of the liberal view have also argued that states have reasons to minimize their dependence on foreign commerce. Territorial expansion is one way to achieve this end. Consequently, heightened trade flows can create incentives for states to take political-military actions to reduce their economic vulnerability (Gilpin 1981, 140–1; Liberman 1996). Consistent with such arguments, Alexander Hamilton argued in 1796 that protectionist policies directed at shielding the industrial sector from foreign competition would enhance America's "security from external danger" and generate "less frequent interruption of their peace with foreign nations" than liberal trade policies (quoted in Earle 1986, 235). In addition, realists and mercantilists frequently point out that heightened commerce increases the array of economic issues over which disputes can emerge. Kenneth Waltz (1970, 205 and 222), for example, opined that since "close interdependence means closeness of contact and raises the prospect of at least occasional conflict . . . the [liberal] myth of interdependence . . . asserts a false belief about the conditions that may promote peace." More generally, many mercantilists and realists maintain that extensive trade can actually provoke belligerence.

Still other observers conclude that trade has no systematic influence on political-military conflict (Buzan 1984; Gilpin 1987; Ripsman and Blanchard 1996/97). That extensive economic globalization marked the period leading to the outbreak of the First World War but trade relations were marked by protectionism and beggar-thy-neighbor policies prior to the Second World War is frequently presented as evidence that trade has little systematic bearing on the use of force when core national interests are at stake.

EMPIRICAL STUDIES

Despite enduring and heated debates about the relationship between interdependence and conflict, systematic empirical analyses of this issue have only emerged over the past four decades. The first wave of studies on this topic was sparked by the seminal work of Solomon Polachek (1978, 1980), who found that heightened bilateral trade flows decreased political conflict based on an analysis of 30 pairs of countries (often referred to as dyads in this literature) from 1958 to 1967. Similarly, Mark Gasiorowski and Polachek (1982) found that increased trade between the US and members of the Warsaw Pact between 1967 and 1975 contributed to a diminution of East-West tensions and the emergence of *détente*. These studies measured hostility using conflict events data, which as I discuss further below, reflect the extent of enmity and cooperation embodied in a wide range of interactions between countries.

A number of studies pitched at different levels of analysis and focusing on the onset of war reinforced the finding that an inverse relationship exists between trade and the use of force. William Domke (1988) analyzed the relationship between trade and war at the level of the nation-state from 1871 to 1985 and concluded that countries with more extensive commercial ties are less likely to become embroiled in wars. Mansfield (1994) addressed this relationship at the level of the international system throughout the nineteenth and twentieth centuries

and found that greater global trade inhibits the onset of both all interstate wars and wars involving major powers.

A second wave of studies was linked to research on the Kantian Tripod. Led by Bruce Russett and John Oneal (2001; see also Oneal and Russett 1997, 1999a, 1999b), a large body of research analyzed a set of hypotheses inspired by Immanuel Kant, one of the foremost liberal theorists. These hypotheses emphasized that democracies rarely fight each other, that states with extensive trade ties tend to be peaceful, and that participation in intergovernmental organizations inhibits conflict. Most of the research on this issue was cast at the level of the dyad and focused on explaining militarized interstate disputes (MIDs), a broad class of conflicts that include threats to use force, displays of force, or armed aggression (including wars). Russett and Oneal concluded that, throughout the nineteenth and twentieth centuries, there was a strong inverse relationship between trade flows and the odds of a MID.

However, both the first and second waves of research also produced a variety of studies that challenged the liberal view. Gasiorowski (1986), for example, addressed various aspects of commercial interdependence and found that some aspects promote conflict whereas others inhibit conflict. In addition, Katherine Barbieri (1996, 2002) concluded that trade increases the likelihood of conflict. Finally, a number of studies issued methodological challenges to quantitative tests of the liberal hypothesis, finding that trade had little bearing on conflict (e.g., Beck, Katz, and Tucker 1998). These studies generated considerable debate and contributed to the ongoing interest in the links between trade and conflict (Oneal and Russett 1999a; Russett and Oneal 2001).

ESTABLISHING CAUSAL MECHANISMS

One shortcoming of the empirical literature on interdependence and conflict during the first two waves of research on this topic has been the dearth of attention paid to establishing the causal mechanisms underlying key results (Mansfield and Pollins 2001). During the third wave, a number of efforts have made to improve our understanding of these mechanisms.

First, a key pillar of liberal theories is that heightened trade increases the opportunity costs associated with armed conflict. Most existing studies emphasize the costs to society as a whole. Trade, however, has distributional consequences, benefiting certain segments of society while imposing costs on other segments. Patrick McDonald and Kevin Sweeny (2007; McDonald 2009) have analyzed the conditions under which the distributional effects of overseas commerce create coalitions that favor war and peace, respectively. More specifically, import-competing sectors are a potential source of support for an aggressive foreign policy because conflict acts as a trade barrier and protects these sectors from foreign competition. In the same vein, export-oriented and non-traded sectors do not gain from conflict. When they are especially influential, governments will have a harder time mobilizing support for the use of force. The upshot is that the balance of power between the sectors that gain from trade interruptions and those that lose from such interruptions play a key role in shaping the government's ability to pursue a belligerent foreign policy. Based on a set of tests covering the period from 1865 to 1914 that I discuss in more detail below, McDonald and Sweeny find that this approach helps to explain the outbreak of MIDs.

In a related study, Katja Kleinberg and Benjamin Fordham (2013) address whether the distributional effects of trade influence whether members of the US House of Representatives support belligerent policies toward China during the period from 2005 to 2009. They find that members whose districts depend on exports to China were unlikely to support hostile measures. They also find weaker evidence that members whose districts were sensitive to Chinese imports were more likely to support such measures. Their study and McDonald and Sweeny's research suggest that the distributional effects of trade may influence conflict, a topic that has stimulated rising interest of late (Brooks 2013).

Second, a number of studies have recognized that the ability of societal actors—including consumers, firms, or sectors—to affect foreign policy is filtered through domestic political institutions, a factor given short shrift in most liberal explanations (Mansfield and Pollins 2001, 841–3). Christopher Gelpi and Joseph Grieco (2008), for example, argue that the constraints placed on leaders by democratic institutions heighten the tendency for economic interdependence to inhibit conflict. Etel Solingen (1998) analyzes how a country's integration into the global economy guides the preferences of domestic coalitions, which then press for foreign policies that advance their interests.

MEASURING INTERDEPENDENCE AND TRADE

The first two waves of research on the links between trade and conflict focused almost exclusively on trade flows. Some studies emphasized openness, which is typically calculated as the ratio of trade to total economic output. Underlying measures of openness is the idea that as a greater fraction of total output is traded, it becomes increasingly costly to disrupt the flow of overseas commerce. Mansfield (1994) used such an indicator and found that heightened global trade (as a percentage of global output) was inversely related to the frequency of war throughout the international system during the nineteenth and twentieth centuries. Oneal and Russett (1997, 1999a, 1999b; Russett and Oneal 2001) employed a related measure in a series of studies cast at the dyadic level of analysis and also report results consistent with liberal claims. Similarly, at the unit level, Domke (1988, 131) concluded that nations more connected to the global economy were less likely to go to war. Thus, studies based on the openness dimension of interdependence offer considerable support for the liberal view.

It is frequently argued that the ratio of trade to output—the leading indicator of commercial openness—is a valid measure of both sensitivity and vulnerability interdependence (e.g., Oneal and Russett 1997). This ratio does provide a useful measure of sensitivity interdependence, since it indicates the extent to which trade partners' economies are intertwined. Its validity as an indicator of vulnerability interdependence, however, rests on the claim that as commerce between countries makes up a larger portion of each country's total economic output, it is increasingly costly for either partner to replace the trade conducted with the other. The basis of this claim can be questioned on at least two grounds.

First, the size of the flow of trade between states (taken either by itself or as a percentage of national income) may not furnish an accurate indication of the costs to them if their economic relations were disrupted. Yet, as noted earlier, the magnitude of these costs is central to assessing the extent of vulnerability interdependence (Hirschman [1945] 1980; Keohane and Nye 1977; Baldwin 1980; Gasiorowski 1986). States trading heavily that can easily locate

close substitutes for the goods being exchanged clearly are not very dependent on each other. At the same time, states conducting little trade that would have great difficulty locating substitutes for the goods being exchanged may be highly vulnerable.

To more directly measure vulnerability, researchers have adopted a number of tactics. Some have analyzed trade asymmetry, typically by using the portion of trade (imports and/or exports) between a given pair of states, A and B, represented in the total trade of A and the total trade of B. The more these two figures differ, the greater the asymmetry between A and B. In a recent study of trade during the First World War, Joanne Gowa and Raymond Hicks (2017) take a different approach. They directly measured the extent to which the war disrupted trade between belligerents and the ability of these states to find alternative trade partners. Gowa and Hicks conclude that the war led to a substantial shift in trade: commerce decreased among antagonists and increased among allies and neutrals. Taken as a whole, however, they find that the First World War stimulated a substitution process and did not impose substantial trade-related costs on the combatants.

Second, the “cost” conception of vulnerability may be too restrictive since some claims regarding interdependence and conflict do not center on the economic consequences of disrupting commerce, but rather on the security implications of dependency or highly asymmetric trade relations. Some realists, for example, argue that highly asymmetric interdependence may restrain the more dependent partner in a dyad, but is unlikely to deter the less dependent partner from resorting to force should their strategic interests collide (Hirschman [1945] 1980). Thus, economic ties between states may restrain only one party from resorting to armed force should a dispute arise, while having no effect on (or possibly even inflaming the aggressiveness of) the less dependent party.

In addition to openness, some research on trade and conflict has emphasized the gains from trade. As Polachek (1980) points out, the microtheory underpinning the liberal claim hinges on the gains from trade, rather than the flow of trade. At best, these gains can only be measured indirectly since, strictly speaking, they presume the observation of a counterfactual condition (i.e., what total economic output would be if there was no cross-border trade). Economists argue that the gains from trade are correlated with import (or export) price elasticities. Unfortunately, the limited availability of price data has severely restricted the range of countries and years over which such elasticities can be used, although some effort has been made to address this issue in work on trade and conflict (Crescenzi 2005).

The vast bulk of the literature on trade and conflict has focused on the effects of trade flows. Recently, however, various studies have started addressing the effects of trade policy. McDonald and Sweeny (2007; McDonald 2009) found that heightened protectionism stimulated conflict during the period from 1865 to 1914. Interestingly, they also found that, after accounting for protectionism, trade openness did not have a statistically significant influence on belligerence. Another set of studies have analyzed the effects of trade agreements. Mansfield and Jon Pevehouse (2000, 2016) have argued that members of the same preferential trade agreement (PTA) are less likely to become embroiled in political-military disputes than other states. Moreover, their results indicate that these agreements condition the effects of trade flows on conflict: heightened trade flows inhibit conflict between PTA members but have no effect on conflict between other states. A related vein of research has analyzed the institutional conditions under which PTAs are most likely to promote cooperation. David Bearce and Sawa Omori (2005) find that those agreements that involve regular meetings of members are better able to avert conflict than others. Yoram Haftel

(2012) concurs that regular interactions among members help to dampen disputes, but he also finds that heightened economic integration within a PTA makes a significant contribution to peace.

TYPES OF CONFLICT AND THE CASE OF THE FIRST WORLD WAR

Empirical research on trade and conflict has addressed a wide variety of interstate conflict behavior, from hostile statements to full-scale war. From a theoretical standpoint, however, it remains unclear whether trade should influence the outbreak of both low- and high-intensity conflict. Liberals, for example, clearly expect that heightened trade will inhibit war; but many of them place little emphasis on lower-intensity conflict, whereas others expect trade to dampen these types of disputes as well (Doyle 1997, Chs. 7–8). Equally, the issue of whether—and if so, how—trade influences the escalation of low-intensity conflict to high-intensity conflict is unresolved (Morrow 1999; Mansfield and Pollins 2001). As a result of these theoretical ambiguities, extant studies analyze various types of conflict, and this, in turn, has contributed to some of the disagreement in the empirical literature about the relationship between trade and conflict.

Methodological orientation helps to shape the type of conflict that is addressed in studies of this relationship. Most qualitative studies focus on international war (e.g., Rosecrance 1986; Ripsman and Blanchard 1996/97; Liberman 1999/2000; Copeland 2015). Most quantitative studies focus on a much broader range of interstate disputes, although this range varies considerably across such studies. Some of the earliest statistical research analyzed events data, such as the Conflict and Peace Data Bank (COPDAB) and the World Event Interaction Survey (WEIS) (Polachek 1980; Gasiorowski 1986). The overwhelming number of studies conducted during the second and third waves of research on trade and conflict have focused on MIDs, which are episodes in which one state threatens, displays, or uses force against another state. Largely absent, however, has been any clear rationale for why focusing on MIDs is theoretically appropriate or the implications of shifting among these different datasets.

The COPDAB, WEIS, and MIDs datasets tap very different types of conflict. The former two compilations record events over the broadest spectrum of international interactions—cooperative as well as conflictual—from low-intensity hostility (such as a verbal protest) to wars. In contrast, the MID dataset only records instances involving the threat, display, or use of armed force. Subsequent interactions surrounding this triggering incident are aggregated into a single data point or observation, an “event” that might persist for years. The differences between the two may be substantial in terms of both the conceptualization and the observation of conflict. In the COPDAB tradition, conflict is conceived as a continuous flow marked at regular time intervals. Such measures of diplomatic relations are based on a very different way of conceptualizing friendliness and hostility than MIDs, which are by definition episodic—sometimes brief, sometimes prolonged—and at least fairly conflictual. And both conflicts events and MIDs obviously include a far wider range of episodes than wars (Pevehouse 2004).

Although much of the existing empirical literature addresses this wider range of episodes, the third wave of research on trade and conflict includes various studies of the First World War. For liberals, the circumstances surrounding the First World War are difficult to explain. The latter portion of the nineteenth century and early years of the twentieth century were a period in which economic globalization flourished (Frankel 2000). In fact, Jeffrey Frankel (2000) points out that the level of global openness in the run-up to the First World War—measured as the ratio of total global merchandise trade to total global output—was not matched until 1970. In contrast to the liberal argument, however, extensive trade did not avert armed conflict. Liberals have typically responded to concerns about this case by arguing that it is simply an empirical anomaly. Nonetheless, it is obviously an important case and the third wave of research on trade and conflict has included a variety of studies on the First World War.

A number of qualitative studies have attempted to shed fresh light on this case. Some of them argue that the First World War is not, in fact, a deviant case from the standpoint of liberal theory. Erik Gartzke and Yonatan Lupu (2012), for example, argue that crises among the more interdependent countries during the first decade or so of the twentieth century did not escalate to war, that crises involving various less interdependent countries during this period did lead to the use of force, and that conflict that sparked war in 1914 occurred between Austro-Hungary and Serbia, two states that were not particularly interdependent. McDonald and Sweeny (2007) cover the 50-year period leading up to the First World War. As discussed earlier, they focus on trade protectionism as well as trade openness and conclude that the former has a far more pronounced impact than the latter. Moreover, heightened protectionism was strongly associated with the onset of MIDs during the era from 1865 to 1914, a finding that accords with liberal theories.

Gowa and Hicks (2017) address a somewhat different feature of trade during the First World War. They point out that liberal arguments typically assert that trade deters war because war imposes heavy trade-related costs on the belligerents. However, Gowa and Hicks find that the First World War imposed few such adjustment costs because combatants were able to divert foreign commerce to third parties. Fordham (2007) also argues that the First World War provides some evidence for the liberal claim, but ultimately concludes that the behavior of the US during this event corresponds with “revisionist” arguments. He finds that, as liberals predict, the gains from trade induced the US to avoid political-military disputes with key commercial partners during this war. However, the US had experienced an export boom during the run-up to the First World War, especially among its allies. In order to protect the gains from this boom, the administration of President Woodrow Wilson was particularly forceful in attempting to check German aggression against American allies and ultimately decided to enter the war on their behalf.

Dale Copeland (2015) offers a different twist on the liberal argument. He argues that the effects of trade on conflict are far less about the amount of interdependence generated at a given point in time and more about the expectations of what trade flows and relations will be in the future. His expansive study covers the era from the end of the eighteenth century to the conclusion of the twentieth century. But in the case of the First World War, he maintains that despite the extensive trade ties that characterized the run-up to conflict, German worries about “long-term access to raw material and markets” contributed to doubts that it would continue to thrive in the international system and limited the extent to which trade ultimately served as a brake on German belligerence (Copeland 2015, 123; but see Snyder 2015/16).

SOME ADDITIONAL ISSUES REGARDING THE RELATIONSHIP BETWEEN TRADE AND CONFLICT

Taken together, the third wave of research on trade and conflict suggests that heightened trade flows, the expectation that such flows will persist, liberal trade policies, and trade agreements inhibit conflict. Furthermore, the First World War may be far less anomalous than critics of liberal theories have charged. Nonetheless, some recent empirical research continues to challenge some key tenets of liberalism.

First, various studies have addressed whether market capitalism helps to explain any observed relationship between trade flows and conflict. Gartzke (2007) and Michael Mousseau (2013) find that capitalism discourages MIDs; and after accounting for its influence, trade openness (and democracy) has no systematic effect on all disputes. Mousseau (2013), however, also uncovers marginally significant evidence that trade openness inhibits those MIDs yielding battle fatalities. Similarly, McDonald (2009, Ch. 9) finds that capitalism reduces the likelihood of a MID and that the effects of trade openness are not robust. But he also concludes that, in addition to capitalism, liberal trade policy discourages military disputes. Taken together, these studies indicate that economically liberal policies dampen conflict, but that the effects of trade openness are ambiguous.

Recent research by Omar Keshk, Brian Pollins, and Rafael Reuveny (2004) also casts doubt on whether extensive trade flows inhibit conflict. Liberal theories typically posit a simultaneous relationship between trade and conflict. Underlying many such theories is the view that trade-reliant groups in society recognize that conflict harms their interests and that they press their governments to avoid the use of force because conflict reduces trade.

In earlier studies, Polachek (1980) and Mansfield (1994, 186–90) estimated simultaneous equations models of the relationship between trade and aspects of conflict. Polachek focused on “net conflict” (using events data) between country-pairs, whereas Mansfield focused on the outbreak of war (both all interstate wars and those involving major powers) throughout the global system. Both studies found that heightened commerce dampens hostilities. However, Keshk, Pollins, and Reuveny (2004) analyze whether a simultaneous relationship exists between bilateral trade flows and dyadic MIDs and arrive at a different conclusion. They find that trade does not exert a statistically significant effect on military disputes, but that disputes reduce trade.

CONCLUSION

Over 30 years ago, a well-known review of the causes of war lamented the dearth of research on the relationship between economic interdependence and hostilities (Levy 1989, 261). Since then, scholars of international relations have addressed this issue with considerable energy, stimulating a rapidly growing literature.

Consistent with Jon Pevehouse and Leonard Seabrooke’s argument about the field of International Political Economy (IPE) in the introductory chapter to this volume, recent

research on interdependence and conflict has been largely problem-driven. Further, this research has been both methodologically and theoretically diverse. These features have contributed to a rich and vibrant literature that has made considerable progress in assessing the links between interdependence and political hostilities.

But this body of work has yet to resolve various core issues. First, a stronger theoretical foundation is needed for many of the competing claims on the relationship between interdependence and conflict. Second, existing studies often rely on different definitions and measures of both commercial interdependence and conflict. While that poses no inherent problem, these differences seem to contribute to variations in the results of empirical studies; and existing theories offer no clear guidance as to which definitions and measures are most appropriate. Moreover, the most widely-used measures of interdependence are excessively narrow, focusing on trade flows. There is a glaring need to resolve questions about the merits of relying on particular measures of interdependence and conflict, as well as to assess the sensitivity of empirical results to the use of different measures.

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CHAPTER 40

FINANCIAL SYSTEMS

LUCIA QUAGLIA

INTRODUCTION

FINANCIAL systems and the governance of global finance are key topics in International Political Economy (IPE) for a variety of reasons. First, the massive size of the financial sector worldwide in comparison to other economic sectors. Second, the high mobility of finance, the cross-border nature of many financial services, and the fact that financial stability can be regarded as an “international public good.” In fact, the international crisis that began in 2008 highlighted how financial instability can spread quickly across national borders due to the internationalization or globalization of finance, which is not a new phenomenon, but which intensified in the 2000s. Third, financial stability is a precondition for economic stability *tout court*, as highlighted by the 2008 crisis, which underscored the need for “fit for purpose” financial regulation and supervision. Furthermore, financial systems and the governance of finance have implications for monetary policy as well as the flow of credit to the real economy (Helleiner 2014). More recently, the economic repercussions of the COVID-19 pandemic on financial systems were a stark reminder of the two-way link between the real economy and finance.

The 2008 international financial crisis increased the political salience of financial regulation, which became (albeit, temporarily) a matter of great public interest for elected officials and the public opinion (Helleiner and Pagliari 2011), outside the restricted circle of policymakers, stakeholders, and experts involved. In fact, policy failures in this sector directly affect the citizens in their position as bank account holders, small investors, insurance holders, pensioners, and so on. They are also indirectly affected as taxpayers, whenever the public authorities have to intervene to bail out or prop up ailing financial institutions (see Culpepper and Reinke 2014; Woll 2014). In other words, there is a (potentially very large) “price tag” on getting financial services governance wrong. Moreover, there are ongoing concerns about the transparency and accountability of institutions and policy-making processes in this area (Mügge 2011).

The international regulation of finance is complex for a variety of reasons. First, different jurisdictions often adopt divergent rules, following different timing and with limited international coordination (for a post-crisis overview, see Germain 2010; Helleiner et al. 2010;

Moschella and Tsingou 2013a, b; Porter 2014). Even if jurisdictions share the same policy objectives (and that is not always the case), they might prefer different instruments or regulatory strategies, also depending on the configuration of their national financial system and domestic regulatory framework (e.g., Fioretos 2010). All this results in regulatory overlaps, gaps, and, occasionally, jurisdictional conflicts (Newman and Posner 2016a, 2016b, 2018). Second, large financial institutions often engage in regulatory arbitrage by moving their activities to less tightly regulated centers or less highly-taxed jurisdictions (e.g., Palan et al. 2013; Seabrooke and Wigan 2017; Sharman 2012, 2017a), triggering regulatory competition across jurisdictions. Other times, cross-border financial activities are hampered by the patchwork of domestic regulation worldwide. A notable case was derivatives trading and clearing post-crisis (Helleiner et al. 2018). Third, there is a variety of international, regional, transnational, and national bodies involved in financial rule-making and monitoring. The multi-level governance of financial services (Baker, Hudson, and Woodward 2005) is characterized by two main interrelated regulatory phenomena: the interaction between rule-making processes in multiple arenas; and the “accommodation” or “coexistence” of their rule-making outputs.

This chapter reviews the existing literature on financial systems and the governance of finance in IPE. It first outlines the main theoretical approaches that have been used. It then takes stock from the existing works in the field, discussing to what extent various bodies of scholarly works speak to each other, and identifying blind spots in the literature. This chapter does not deal with monetary matters and international financial institutions (such as the International Monetary Fund (IMF)), which are examined in other chapters of the handbook.

THEORETICAL APPROACHES TO FINANCIAL SYSTEMS AND THE GOVERNANCE OF GLOBAL FINANCE IN IPE

This section reviews the main theoretical approaches that have been used in the literature on financial systems and the governance of finance in IPE, namely: state-centric, business-led, network-based, and interdisciplinary approaches.

State-Centric

A first body of scholarly work has taken a state-centric approach to financial systems and the governance of finance in IPE. The early academic literature stressed the preferences and power of the “hegemonic” country, the United States (US) (Simmons 2001) in order to explain why and how rules for global finance were set (or not). The assumption was that international regulatory cooperation benefited some countries more than others, and, therefore, had redistributive implications. This approach highlighted the hegemonic power of the US in global finance. More recently, Helleiner (2014) pointed out the “structural power” and “active policy choices” of the US in order to explain the limited change of the status quo after

the international financial crisis. In turn, the power of the US stemmed from the size of its large domestic market (Drezner 2007) and from the fact that the US authorities were often “first-movers” in initiating domestic regulatory reforms, which then informed the making of international rules (Posner 2010). Frequently, the US authorities acted as “pace-setters” internationally, following the adoption of domestic reforms because international regulatory harmonization would help to minimize the competitive disadvantages ensuing for the US financial industry (Kapstein 1989), and would limit negative cross-border externalities for financial stability (Simmons 2001).

Reflecting the changed global context in the twenty-first century, several works have questioned the hegemony of the US in governing global finance by considering other jurisdictions that have large financial markets. Singer (2007, 2004) investigated the domestic politics of preference formation for or against international harmonization concerning the three main segments of the financial sector (banking, securities markets, and insurance) in the US, the United Kingdom (UK), and Japan. He argued that international cooperation was a way for domestic regulators to restore the domestic balance between domestic financial stability and international competitiveness in response to an external shock, so as to appease elected officials and the financial industry. Drezner (2007, 5) examined the role of the “great powers,” defined as “governments that oversee large internal markets,” namely, the US and the European Union (EU), in a variety of regulatory regimes, including finance. Jurisdictions on the fringe were mostly followers of international financial rules set by the great powers, and their domestic implementation of those rules was often slow and piecemeal (see, e.g., Mosley 2010; Walter 2008), albeit sometimes, also the US and the EU did not comply with international soft law in finance (Quaglia 2019). The regulatory preferences of the various jurisdictions depended on the domestic adjustment costs to the new international rules (Drezner 2007) and domestic regulatory institutions, which were the sources of preferences and power in international regulatory harmonization in finance (Buthe and Mattli 2011; Fioretos 2010, 2011; Posner 2010). Other state-centric works drew attention to the configuration of national financial systems, arguing that national policymakers engaged in international harmonization sought to defend the comparative institutional advantages of their domestic financial industry, especially in the case of shadow banking (Thiemann 2018, 2014), offshored financial centers, and tax havens (e.g., Palan et al. 2013; Rixen 2011; Sharman 2006, 2012).

State-centric works generally start from the assumption, grounded in the broad tradition of rational choice analysis, that states use international institutions to further their own goals, and they design institutions accordingly (Koremenos et al. 2001). For instance, with reference to the international regulation shadow banking and taxation, Rixen (2011, 2013) distinguished between coordination games, on which international agreement is within reach, and more controversial distributive games, which have winners and losers. Yet, several works on international financial regulation have shown that the rational pursuit of interest-driven goals by state actors may result in a less than rational institutional design. For example, Kalyanpur and Newman (2017) argue that the evolution of the international securities regime was strongly shaped by an experimental bricolage approach of trying out different solutions to the problems at hand, rather than by comprehensive rational institutional design.

The state-centric body of scholarly literature has mainly drawn attention to explanatory factors related to financial *interests*, hence, the distributional effects of various sets of

international financial rules across jurisdictions, and the negative and positive externalities related to cross-border financial flows for various jurisdictions. It has also paid considerable attention to *institutions*, especially, national varieties of financial capitalism, domestic regulatory frameworks, and regulatory capacity in finance. The concept of power, in various forms (structural power, ideational power), has considerable analytical leverage in state-centered approaches (e.g., Hakelberg 2016; Emmenegger 2017). Overall, this body of scholarly works has detected a shift away from US hegemony in finance in the two decades following the Second World War, to bipolarity, to be precise, the US-EU condominium in the 2000s (Posner 2009; Quaglia 2014), which was challenged after the crisis by emerging powers.

Business-Led

A second body of IPE literature on financial systems and the governance of finance has focused on the role of the financial industry, arguing that financial regulation tends to reflect the interests of private actors (Baker 2010; Bell and Hindmoor 2015; Underhill and Zhang 2008), that is to say, the rules that the financial industry finds more advantageous (or less burdensome), on the basis of the expected costs and benefits and the distributional implications of the new rules. For example, some authors argued that the lowering of bank capital requirements in the Basel II Accord for internationally active banks was a clear example of an international race to the bottom and industry's capture of regulators (Lall 2012; Tsingou 2008; cf. Young 2012). The literature has identified two main sources of business influence: structural power and instrumental power (for a review, see Woll 2014). Structural power recognizes that governments are dependent on investment decisions by business to sustain economic growth and fund public services. Business, therefore, wields an "investment veto weapon" as it can implicitly threaten to disinvest or "exit" from a given jurisdiction. In anticipation of negative inducement effects, policymakers avoid policies that threaten to undermine business confidence. Instrumental power comprises the mobilization of financial and human resources for the purpose of influencing policymakers. This includes the role of campaign donations, lobbying activity, and the existence of "revolving doors" between government and industry, all of which provide privileged access to the policy process. Recent contributions have emphasized the mutual dependency of the two forms of business power. Culpepper and Reinke (2014) have distinguished the sources of business power (structural vs. instrumental) from how they are mobilized (automatically or strategically). From this perspective, structural power has to be asserted strategically through costly instrumental political action. For example, firms may use their lobbying resources to deliberately amplify policymakers' concern over disinvestment by making claims about the detrimental economic impact of new regulations (Bell and Hindmoor 2016; Baker and Wigan 2017). Firms can also manipulate their relative structural power by reshaping the availability of exit options, which Farrell and Newman (2015) have referred to as "structuring power." Finally, some authors (Braun 2018, 2020; Gabor 2016; Gabor and Ban 2016) have pointed out the "infrastructural power" of the financial industry, "which stems from entanglements between specific financial markets and public sector actors, such as treasuries and central banks, which govern by transacting in those markets" (Braun 2018).

Some works have argued that the structure of the national financial industry (or the variety of national financial capitalism) affect national preferences on the regulation of various financial services. Thus, international hedge funds regulation was resisted by countries that had a large hedge funds industry (Fioretos 2010), and the regulation of tax havens and off-shore centers was resisted by countries that were tax havens or off-shores centers (Hakelberg 2016; Palan et al. 2013; Rixen 2011; Sharman 2017a, b). However, this strand of the literature can be regarded as part of the state-centric explanation discussed above. Other works have focused, instead, on the activity of transnational associations, such as the Institute for International Finance (IIF) (Newman and Posner 2016a, b), or the International Swaps and Derivatives Association (ISDA) (Newman and Bach 2014), the International Accounting Standards Board (IASB) (Perry and Nölke 2006), and the formation of cross-border coalitions in the making of rules for global finance (McKeen-Edwards and Porter 2013). Thus, according to Cerny's "transnational neopluralism" (2010, 4–6), the most important "movers and shakers" are no longer domestic forces, but rather "actors that can coordinate their activities across borders." For example, Farrell and Newman (2014) and Newman and Posner (2018) show that cross-border coalitions were instrumental in settling recent transatlantic regulatory disputes.

Prior to the international financial crisis, the financial industry was very influential in shaping (or even preventing) statutory regulation. In certain segments of the financial sector, such as derivatives (Pagliari 2012) and credit rating agencies (Sinclair 2005), private sector governance was widespread. After the crisis, the regulatory pendulum moved away from private sector governance. The scope of public regulation was extended to previously unregulated financial entities and activities, notably, derivatives markets (e.g., Helleiner et al. 2018) and, to a lesser extent, shadow banking (Ban and Gabor 2016; Ban et al. 2016; Bryan et al. 2016). Moreover, existing rules were tightened up, for example, the Base III accord, which superseded the pre-crisis Basel II accord, set higher capital requirements for banks and new rules were set for macroprudential regulation (Baker 2013) and bank resolution (Quaglia 2017). Post-crisis, the financial industry became less influential, at least in the short and medium term, and had to find new ways of mobilizing. Some scholars studied, for instance, the formation of coalitions within the financial industry, as well as between the financial industry and firms in the real economy (Pagliari and Young 2014; Young and Pagliari 2017). Others examined how the lobbying strategies of the financial industry changed in the post-crisis context (Bell and Hindmoor 2016; Young 2013), seeking to exert pressure not so much at the rule-making stage, which was in the public eye, but rather at the implementation stage.

The literature that has adopted a business-led approach has mainly drawn attention to explanatory factors related to private financial *interests*, hence, the costs and benefits of regulation for various parts of the financial industry, and the incentives of transnational actors to promote cross-border financial flows (e.g., Lall 2012; Tsingou 2008; Underhill and Zhang 2008), which were often resisted by domestically-oriented actors. *Institutions*, which shaped access points to the policy process and veto players (e.g., Tsingou 2015b), were also important intervening factors. Overall, this body of scholarly works has detected a shift away from the pre-crisis power of the financial industry and private sector governance, toward a diminished influence of the industry in the wake of the crisis, whereby private actors had to fight rear-guard battles with a view to limiting post-crisis regulatory reforms. Yet, this was a temporary shift in the balance of power, which had to do with the political salience of

financial regulation in the wake of the crisis (Helleiner and Pagliari 2011). As the memories of the international financial crisis faded away, the pendulum swung back, and the financial industry regained significant influence in shaping financial regulation at the international and national levels. Moreover, it also took increasing advantage of the “disjuncture between territorially fixed fiscal and intrinsically fluid financial systems” through global wealth chains, discussed below (Seabrooke and Wigan 2017, 2).

Networks-Based

A third body of literature on financial systems and the governance of global finance has highlighted the role of transgovernmental networks of domestic regulators (Tsingou 2015a; Porter 2014) and professional networks (Seabrooke and Tsingou 2014; Seabrooke and Wigan 2016). International regulatory harmonization in finance no longer belongs to the realm of intergovernmental negotiations, but falls within the responsibility of a transnational policy community of experts actors (Tsingou 2010) that have common professional and educational backgrounds (Chwioroth 2010) and share similar epistemological views (Kapstein 1992). Of particular interest are transgovernmental networks of regulators that have some core features: they are multilateral bodies that are not established pursuant to treaties; their membership consists of domestic regulatory agencies; they work by consensus; and they issue (non-legally binding) soft law (Bach and Newman 2010, 2014). The autonomous role of international standard-setters is often downplayed by state-centric accounts because they are considered as fora (or “empty vessels”), where national interests play out and negotiations amongst different jurisdictions take place. Alternatively, these fora are considered by business power accounts as targets for the lobbying activity of the financial industry, which might result in regulatory capture (Lall 2012).

Yet, transgovernmental networks of regulators are more than the sum of their members. Several academic works have stressed the influence of transnational bodies that issue soft law, which, over time, “disrupt” domestic regulatory templates and affects the patterns of interactions amongst domestic actors (Newman and Posner 2018). In turn, these dynamics have feedback effects (Newman and Posner 2016a, b) by reshaping the way in which domestic actors engage in international standard-setting. On the one hand, domestic regulators are “reluctant diplomats” (Singer 2007, ix) because they have a prominent role in negotiating international standards with their counterparts, given the level of required technical knowledge, but would prefer to stay out of the political limelight. On the other hand, financial regulators are not purely technocratic actors insulated from the economy and politics of the national context in which they are embedded. Hence, regulators have to be mindful of the implications of international rules designed to protect financial stability for the competitiveness of domestic financial firms, striking a balance between the preferences of elected officials and those of the financial industry (Singer 2004, 2007). Consequently, the process of international harmonization does not simply revolve around establishing consensus on the technical details; it also involves striking a balance between the domestic political economy concerns of various jurisdictions (Quaglia and Spendzharova 2017). Furthermore, transnational standard-setting fora suffer from limited direct democratic accountability to voters (see Mügge 2011), which is particularly problematic when international rules have potential fiscal implications domestically.

Transgovernmental networks can be effective in dealing with information asymmetries in international negotiations or in reducing the lack of trust amongst jurisdictions, but are less effective if the domestic preferences of jurisdictions are not aligned (e.g., in the case of hedge funds regulation, see Fioretos 2010) or the international problem to be addressed has major distributional implications, for instance, the regulation of financial offshore centers (Rixen 2013), or the regulation of tax havens (Hakelberg and Schaub 2018; Rixen 2013). Similarly, if an issue has high political salience—hence, it is no longer “quiet politics” (Culpepper 2011)—elected officials are likely to follow it closely and might seek to intervene directly in the regulatory process. Furthermore, in the wake of the crisis, the transnational policy community in finance came “under stress” (Tsingou 2010) because there was “much less consensus on optimal regulatory models” (Helleiner and Pagliari 2011). For example, in the case of accounting standards, regulators were unable to agree on how and when to apply core principles such as fair value accounting (Mügge and Stellinga 2015).

Transgovernmental and professional network approaches tend to pay attention to ideas (Baker 2013) and other socially constructed elements (Abdelal 2007), having to do with similar educational training (such as exposure to neoliberal ideas, see Chwieroth 2010), cognitive learning through transnational capacity-building (Broome and Seabrooke 2015), a shared expert-like problem-solving approach (Kapstein 1992), and socialization in the same professional milieu or organization (Seabrooke and Wigan 2016; Tsingou 2015a), where the same individuals repeatedly interact over time. Moreover, it is noteworthy that financial regulators tend to share a common interest in the “quest for coherence in finance” (Quaglia and Spendzharova 2019) because inconsistent rules across levels of governance (international, regional, and national) as well as at the same level (e.g., across jurisdictions) can jeopardize the safeguard of financial stability.

Heterodox Interdisciplinary Approaches

A fourth body of IPE literature on finance brings together heterodox interdisciplinary approaches, which are mostly influenced by economic sociology and economic geography. First, there is research on financialization (see Engelen et al. 2010; Langley 2008; Nesvetailova 2015), which has investigated “how an increasingly autonomous realm of global finance has altered the underlying logics of the industrial economy and the inner workings of democratic society” (Zwan 2014). Second, there are works on performativity. For example, MacKenzie (2006) demonstrated that financial models devised and used by financial institutions act as “engines,” rather than “cameras” in shaping, rather than reflecting, markets. Other authors discussed performativity with reference, for example, to credit rating agencies (Paudyn 2012). Third, there are works on global wealth chains (Arjaliès et al. 2017; Harrington 2016; Seabrooke and Wigan 2017; Sharman 2017a), which are defined as “transacted forms of capital operating multi-jurisdictionally for the purposes of wealth creation and protection” (Seabrooke and Wigan 2017; see also Bryan et al. 2017). As Seabrooke and Wigan (2017) have noted, global wealth chains in the past were mostly studied in IPE under the rubric of the “offshore world” (Palan 2003), which included money laundering (Sharman 2011), tax avoidance, and evasion (Sharman 2006; Palan, Murphy, and Chavagneux 2013). The literature on global wealth chains speaks to the two bodies of works outlined above, notably, the literature on transnational financial networks (global wealth chains are based on networked

activities) and financial industry power (global wealth chains are an expression of business power). It also highlights the limits of state-centric approaches, whenever financial entities and activities seek to transcend national borders and bypass national regulation.

AN OVERALL ASSESSMENT

The editors note in their Introduction to this handbook that IPE has become largely problem-centered, meaning that rather than speaking to theoretical debates or contributing to specific “schools of thought,” most research engages with real-world questions. This problem-centered approach means that scholars use theories and methods that are suitable to investigate particular topics. This is certainly true in the IPE literature on financial systems and the governance of global finance, where a variety of theoretical approaches, discussed above, have been used. A vast array of quantitative, qualitative, and mixed methods has also been used. Thus, beside the (few) studies that have made use of formal modeling and statistical analyses, researchers have used case studies—notably, country studies that focus on the main financial jurisdictions, such as the US; or case studies of key international financial standards, such as the Basel accords—structured focused comparisons across countries or segments of the financial sector, and process-tracing. Scholars have also carried out informal and formal network analyses of financial experts, coalition formation (Pagliari and Young 2014), and cross-border financial flows (Oatley et al. 2013). Borrowing from economic sociology, scholars have mapped professional ecologies in finance and have carried out analyses of career histories of financial experts (Seabrooke and Nilsson 2015). Finally, textual analysis, for instance, of post-crisis reports prepared by various expert groups (Seabrooke and Tsingou 2014), or responses issued by financial industry groups to public consultations concerning specific pieces of regulation (James et al. 2021) has been used. In terms of empirical coverage, the IPE literature on financial systems and the governance of global finance has examined banking, securities markets, accounting, money laundering, off-shore financial centers, and tax havens. After the crisis, the literature has extended to shadow banking, derivatives, sovereign wealth funds, global wealth chains, and financialization, seeking to keep pace with financial developments. A limited number of works have also examined domestic compliance (or non-compliance) with international soft law in finance (e.g., Mosley 2010; Quaglia 2019)—which remains the Achilles heel of international financial regulation—as well as the second-order effects of international soft law at the domestic level (Newman and Posner 2018).

Several works reviewed in this chapter offer interesting insights into different aspects of financial systems and their regulation. Taken together, they shed considerable light on the governance of global finance. In their introductory chapter of this volume, the editors note that today’s IPE scholars who work in different strands of the literature in the same subfield often do not engage with each other. In the case of finance, the absence of dialogues between different traditions is somewhat less stark than in other subfields of IPE, albeit it is fair to say that there is still limited cross-fertilization between the more traditional state-centric, industry-led, and transgovernmental approaches and the interdisciplinary work that is influenced by economic sociology and economic geography. Three main blind spots can be identified in the existing literature. First, there have been limited attempts to combine

various theoretical approaches in order to provide a holistic understanding of a given subject, for instance, through the synthesis of different theories or by sequencing them on the basis of domains of application. Second, financial systems and the governance of global finance have become more complex and more entangled in the wake of the regulatory reforms that followed the international financial crisis. Yet, the concept of regime complexity, which has been deployed in several subfields of IPE (notably, trade policy, environmental policy) has only recently been used to shed light on the governance global finance (for some exceptions, see Heldt and Schmidtke 2019; Quaglia 2020). Finally, the economic consequences of the pandemic and its repercussions on financial systems need to be investigated both theoretically and empirically.

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CHAPTER 41

DEVELOPMENT POLICY

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IN 1960, over 15 European colonies located in Africa became independent. In recognition of the challenges facing these new countries, the United Nations (UN) resolved in January 1961 that the 1960s would be the “Decade of Development.” As part of the Development Decade, UN agencies, the World Bank, and individual wealthy countries worked to channel resources to the newly independent countries of the developing world with the aim of spurring economic development.

In thinking about efforts undertaken by wealthy countries to promote economic development in poor countries, many scholars focus on the sending of resources—usually under the label “foreign aid”—through government-to-government channels or through multilateral organizations. Other scholars study non-governmental organizations (NGOs) that make use of donations by private citizens and funding from governments to undertake programming in developing countries. The ways in which the wealthy countries of the world can affect the poor countries of the world, however, extend well beyond the direct transfer of financial resources. In both intended and unintended ways, economic policy, migration policy, environmental policy, and human rights policy, for example, all have consequences for development in poor countries. In some cases, debates over these policies directly invoke the idea of development and revolve around the consequences of policy change for developing countries; in other cases, policies are debated and decided on other terms but nonetheless have serious impacts on the poor and middle-income countries of the world.

In this chapter, we argue that the field of International Political Economy (IPE) has underinvested in studying the determinants of non-foreign-aid policies that affect development. Reviewing literature on a set of eight policy areas where there are identifiable development consequences, we discuss the findings of the IPE literature with regard to policy origins, changes, and consequences. We find a consistent role of NGO pressure on wealthy-country governments in bringing about pro-development policies, although we also identify instances where pro-development policies originate in domestic and strategic interests. Overall, we argue that there is significant space for additional exploration through a development lens of how policies come into being in the wealthy countries of the world.

THE DEVELOPMENT LENS IN DEVELOPED COUNTRY POLICYMAKING

Government policies result from actors with preferences over outcomes pursuing the enactment of specific policies in specific institutional contexts. These actors include legislators who vote on new laws, executive branch agencies that implement laws, and lobbyists and citizens who pressure these legislators and administrators. Both IPE scholarship and comparative political economy literature have concerned themselves with understanding how particular arrays of preferences and institutions result in specific policy outcomes (e.g., Grossman and Helpman 1994; Kim 2017).

In the foreign aid literature, scholars historically have proposed that foreign aid policy might result either from the dominance of preferences that favor the needs of the poor countries of the world or else the dominance of interests that seek to use foreign aid as a foreign policy tool (McKinlay and Little 1977, 1978; Lumsdaine 1993; Schraeder, Hook, and Taylor 1998; Alesina and Dollar 2000; Bueno de Mesquita and Smith 2007, 2009; Heinrich 2013). That is, foreign aid policy might result, on the one hand, from the ascendance of humanitarian concerns for alleviating poverty that originate in a normative worldview where wealthy countries should use their resources to help poor countries. On the other hand, foreign aid policy might result from the fact that foreign aid provides a useful tool for securing policy concessions or developing soft power in other countries. We see evidence that legislators most concerned about national security or market access promote aid policies that pursue those ends (Milner and Tingley 2010, 2011). Bermeo (2017, 2018) combines these two viewpoints into a theory of “targeted development” in which alleviating poverty in foreign countries is seen as a desirable foreign policy goal because of the benefits that such poverty alleviation will bring to a wealthy country.

These same lenses (i.e., recipient needs and wealthy country strategic interests) might also capture the positions of relevant actors in other arenas of development policy. Actors competing to define any of trade policy, migration policy, or environmental policy might do so with an eye to the humanitarian ends that the policy will achieve or else with an eye to the impact of the policy on self-interested goals of improving security or maximizing economic gain for domestic actors.

TRADE POLICY

Trade policy is a realm where we typically assume that self-interest will dominant. Yet a number of wealthy countries employ the Generalized System of Preferences (GSP) with the stated goal of providing “opportunities for poor countries to use trade to grow their economies and climb out of poverty.”¹ The US, the European Union (EU), and other industrialized countries established GSPs in the mid-1970s ostensibly to promote economic development of poor countries through trade by means of unilaterally lowering trade barriers (Borrmann, Borrmann, and Stegger 1981). Some scholars identified this as a paradigm shift in development strategy, since industrialized countries previously

had endeavored to promote economic growth of developing countries through aid (Clark 1991).

The spread of GSPs appears to have happened because of the spread of new ideas about development through elite networks. Bhattacharya (1976) argues that the United Nations Conference on Trade and Development (UNCTAD) played a critical role in creating GSP regimes. UNCTAD argued that an export orientation was key for economic development and advocated for developed countries to provide trade preferences (Graham 1978) and for influential developing countries to cease demanding selective preferences (Bhattacharya 1976). These ideas spread through elite-level bureaucratic contacts, consistent with a bureaucratic politics model of transgovernmental relations (Meltzer 1976).

At the same time, the adoption of GSP by the US may have been guided at least in part by material interests. On the one hand, the European Economic Community (EEC)'s system of selective preferences was giving European countries special access that the US lacked to some markets. On the other hand, the US seemed unlikely to gain exclusive access to Latin American markets because of left-wing opposition in those countries to selective trade deals. Joining a global GSP program would serve to put the US and Europe on a more level playing field with regard to expanding market access (Bhattacharya 1976).

Beyond GSP, both Europe and the US have developed region-specific trade policies where the ostensible goal is to catalyze development. For Europe, the 1975 Lomé Convention enshrined trade and aid relationships between the EEC and 40 countries from Africa, the Caribbean, and the Pacific (ACP). According to multiple scholars, this arrangement was driven less by humanitarian concerns and more by the interest of major European countries—particularly France—in retaining influence in their former colonies or in maintaining stable market access (Gruhn 1976; Akokpari 2017; Whiteman 2017). Under the Lomé Convention, ACP countries achieved free access to European markets (with some exceptions) and obtained additional foreign aid (Gruhn 1976; Whiteman 2017).

In 2000, the Contonou Agreement succeeded the Lomé Convention and eliminated the principle of non-reciprocity in trade (i.e., the principle that ACP countries could maintain tariffs even in the absence of European tariffs). ACP states were generally opposed to this measure, but the EU pushed through the change, arguing that the non-reciprocal trade preferences of the Lomé Convention were not compatible with the trade principles of the World Trade Organization (WTO) (Carbone 2013). The Contonou Agreement also explicitly incorporated political dialogue into the relationship between the EU and the ACP countries (Carbone 2013). Insofar as these changes were not desired by the ACP states and dampened the development orientation of the Lomé Convention, this shift has been described as “a triumph of realism over idealism” (Farrell 2005).

At the international level, this shift in Europe's approach to the ACP countries can be attributed to the failure of the ACP states to negotiate from a unified standpoint (Slocum-Bradley 2007). Suggesting a strong bargaining asymmetry, one observer asserted that “the single best predictor of what an ACP-EU Agreement will eventually look like” would be “the results of the internal negotiations within the EU” (Elgström 2000, 195). To explain why the EU shifted to a less development-oriented arrangement, Farrell (2005) points to the decreasing political salience of Africa and the desire to be in compliance with General Agreement on Tariffs and Trade (GATT)-WTO arrangements; Elgström (2000) partially attributes the change to increasing interest in using aid and trade policy to support democratization.

Comparable to the Europe-ACP arrangements, in May 2000, the US legislated the African Growth and Opportunity Act (AGOA), initially including 34 countries. AGOA-eligible countries can export eligible goods to the US without paying tariffs (USTR 2019a); the list of goods covered by AGOA is broader than that covered by the US GSP (USTR 2019c). In exchange for trade preferences, AGOA countries are required to change economic and political policies, such as promoting the market economy, eliminating barriers to American trade and investment, strengthening rule of law, and protecting human rights (Fuhr and Klughaupt 2004). Such conditionalities might be seen as either pro-development or else as political.

As with the origins of GSP in elite thinking about non-aid-based policies for economic development, some see AGOA as the product of paradigm shift in US development strategy (Akech 2001; Hendrickson 2014). A scheme for promoting liberalization in Africa was in line with calls being made by American business representatives from the early 1990s for more liberalization in Africa (Hendrickson 2014). In addition, US business argued that it was losing out on opportunities in Africa due to European preference schemes (Hendrickson 2014).

Overall, the extent to which pro-development trade policies have been effective is not immediately obvious. On the one hand, evidence exists that the US GSP program substantially affects beneficiaries' export volumes (Pelzman 1983; MacPhee and Oguledo 1991; Grossman and Sykes 2005; Gil-Pareja, Llorca-Vivero, and Martínez-Serrano 2014; Blanchard and Hakobyan 2015; Sharma, Boys, and Grant 2019). On the other hand, some authors critique GSP policies for dampening developing countries' willingness to adopt liberal trade policies (Özden and Reinhardt 2005; Shadlen 2005) or for hindering economic diversification (Herz and Wagner 2011). For the ACP, a European Commission report similarly expresses skepticism about the extent to which ACP can bring about economic growth in the absence of domestically-driven economic reform (EU Commission 1996; Thelle et al. 2015). With regard to AGOA, on the other hand, scholars largely agree that AGOA has contributed to increases in exports from AGOA countries (Collier and Venables 2007; Frazer and Biesebroeck 2010; Cook and Jones 2015); AGOA eligibility, however, is linked to political criteria, and critics have argued that the enforcement of eligibility requirements is subjective and sometimes unfair (Fuhr and Klughaupt 2004).

LABOR RIGHTS POLICY

Similar to how the Contonou Agreement incorporated provisions about political dialogue into a trade policy framework, trade policy conditionality can be used more generally to promote workers' rights in developing countries, something that might be seen as positive from a broad development perspective (Compa and Vogt 2001). The 1984 renewal of the GSP program in the US included provisions related to workers' rights, and Hafner-Burton, Mosley, and Galantucci (2019) find that countries violating workers' rights are more likely to experience GSP suspensions. At the same time, however, these authors also provide evidence of the strategic use of these sanctions at the country-product level (i.e., that sanctions originate in concerns about economic competitiveness and not workers' rights alone), which ultimately could be counterproductive from a development perspective.

The issue of labor rights has also entered into other debates about trade policy toward developing countries. A common argument is that the desire of developing countries to

attract foreign capital in order to build export-oriented economies leads to a “race to the bottom” in which a developing country hollows out labor rights in order to appear as a better location for investment (O’Donnell 1998; Drezner 2001). Mosley and Uno (2007) find that trade openness is, in fact, negatively associated with labor rights, although they also find that investment flows positively correlate with labor rights.

In the 1990s, transnational activists in the anti-sweatshop movement began to pressure corporations and rich-country governments to put policies in place that would improve labor rights in developing countries. In the US, the Clinton administration responded to civil society pressure by forming a special task force, which led to the establishment in 1998 of the non-governmental Fair Labor Association that monitors labor standards and provides accreditation to companies for good labor practices.

Other work looking at the “race to the bottom” has found evidence that trade openness actually improves the quality of labor standards in developing countries. Kim (2012) discovers that developing countries are likely to strengthen labor standards prior to negotiating or signing trade agreements with the US, as they know that public opinion favors signing or ratifying trade agreements with countries having high-level labor standards. Postnikov and Bastiaens (2014) test the effectiveness of the enforcement mechanisms of labor provisions in EU trade agreements, which are designed to improve trading partners’ labor standards through dialogues and consultations. They find that the enforcement mechanisms advance the labor standards of the trading partners. Good labor practices in importing countries may also influence the quality of labor standards in exporting countries through diffusion. Greenhill, Mosley, and Prakash (2009) find that labor standards in export destinations predict the labor standards in the countries where manufacturing is happening. Greenhill, Mosley, and Prakash (2009) also report that membership in preferential trade agreements containing hard human rights conditions appears to be associated with improvements to labor protection laws.

INTELLECTUAL PROPERTY RIGHTS POLICY

Another aspect of trade policy relevant for development is the pursuit of intellectual property rights protection. Through bilateral and multilateral means, wealthy countries work to make other countries respect the patents and trademarks held by firms based in the wealthy countries. Patent and trademark enforcement should bring additional profits to those firms. Such intellectual property rights protection may, however, have negative development consequences. While some argue that implementing strong property rights protections will catalyze more investment and technology transfer for developing countries (Kampf 2007), others argue that protecting the intellectual property rights of wealthy-country businesses establishes roadblocks to development for poor countries (Chang 1994).

During the Uruguay Round of GATT negotiations (1984–1994), the US led a group of wealthy countries in negotiating the Agreement on Trade-Related Aspects of International Property Rights (TRIPS). Under TRIPS, developing countries are obligated to develop strong protections for intellectual property rights, and complaints against states failing to protect intellectual property rights can be brought to the WTO’s dispute settlement mechanism.

While many developing countries resisted TRIPS, ratification of the agreement became an *ex ante* requirement for membership in the WTO, severely constraining the ability of

developing countries to avoid signing the agreement without foregoing a host of other benefits. Ultimately, perhaps surprisingly, some developing countries have adopted intellectual property standards that actually are more stringent than those required in the agreement (Deere 2009; Morin and Gold 2014).

The potential negative consequences for developing countries of strong intellectual property rights protection have most clearly been seen with regard to the protection of patents on pharmaceuticals. Under TRIPS, patent holders can restrict developing countries from producing and distributing generic medicines, and pharmaceutical companies typically attach high price tags to medicines protected by patent laws (Lanoszka 2003). In the 1990s, this meant that many people in the developing world were unable to gain access to the first generation of antiretroviral drugs starting to help address the HIV/AIDS crisis.

The South African government enacted a law in 1997 allowing the production of generic AIDS drugs. Thirty-nine international pharmaceutical companies quickly brought a lawsuit in South African court. At the same time, arguing that the Brazilian government had breached the TRIPS Agreement, the US brought a case against Brazil through the WTO dispute mechanism (WHO and WTO 2002).

With an eye to the negative consequences of denying HIV/AIDS drugs to people in need, transnational civil society movements began to fight back against the international pharmaceutical companies—essentially calling for a more pro-development policy in the realm of intellectual property rights protections. Transnational activists and NGOs directly encouraged developing countries to keep producing generic versions of the drugs (Sell and Prakash 2004) and lobbied American and European officials to address the AIDS crisis in developing countries (Drezner 2008). Transnational activists informed American and European citizens about the negative consequences of intellectual property rights protection in an attempt to bring about pressure on rich-country governments and also provided resources to developing countries to help them fight legal battles (Drezner 2008).

As a result of pressure from this transnational movement, signatories to TRIPS issued the 2001 Doha Declaration on the TRIPS Agreement and Public Health, which reaffirmed a set of flexibilities for developing countries that had been included in the original TRIPS Agreement (Lanoszka 2003; Chorev 2012). These flexibilities include the granting of licenses to produce generic medicines. A 2005 WTO agreement created a mechanism allowing WTO members to export generic versions of patented mechanisms to developing countries lacking manufacturing capacity. These policies have enabled many developing countries to provide generic medicines to their citizens (Nicol and Owøye 2013). While Chorev (2012) describes this as a situation in which global activism precipitated a change in a global norm, Drezner (2008) raises the possibility that the security concerns of the US (i.e., that the AIDS pandemic could cause state failure and threaten US security interests) were the main drivers of policy change.

FOREIGN INVESTMENT POLICY

Foreign direct investment (FDI) is considered the most crucial type of investment for promoting economic development (Jensen 2003). As compared to portfolio investment, FDI

involves some amount of control over the management of the enterprise by the investor and tends to have a long-term investment horizon (Borensztein, De Gregorio, and Lee 1998). FDI promotes capital accumulation (de Mello 1999) and technology transfer (Borensztein, De Gregorio, and Lee 1998) and may generate foreign exchange and promote employment (Jensen 2003). Many developing countries regard the attraction of FDI as a crucial part of their development strategy (Kerner 2009, 75). Investment-sending countries therefore have the opportunity to enact pro-development policy by creating arrangements that encourage outward direct investment.

A major limitation on direct investment is the fear among investors of expropriation. Historically, a customary international law known as the “Hull Rule” required countries nationalizing property to provide “prompt, adequate, and effective” compensation (Guzman 1998, 644). The newly independent states of the twentieth century, however, collectively objected to the principles of the Hull Rule and successfully passed two UN resolutions (3171 and 3201) challenging it. Guzman (1998) describes this transformation of international law as a “clear victory” for developing countries. In the 1960s and 1970s, newly independent countries frequently nationalized the assets of foreigners (Kobrin 1984; Guzman 1998; Li 2009).

In this new international environment, developed and developing countries began to sign more bilateral investment treaties (BITs). BITs are designed to protect investments from host-country expropriation, thereby giving investors the sense of security that they need in order to make investments. They do so by imposing significant obligations on host countries, and they typically include clauses allowing investors to appeal to an international arbitration body, usually the International Centre for Settlement of Investment Disputes (ICSID), a significant sovereignty cost (Guzman 1998; Allee and Peinhardt 2011).

Despite these high potential costs, almost 3,000 BITs have been signed as of 2019 (UNCTAD 2019). Elkins, Guzman, and Simmons (2006) suggest that the competition among potential host countries to attract greater FDI has accelerated the proliferation of BITs. Tying their hands by entering into BITs signals a credible commitment to not expropriate foreign investment and to the provision of a favorable investment environment (Tobin and Rose-Ackerman 2011). Allee and Peinhardt (2010) discover that poor countries with slow economic growth and trade-dependent developing countries are more likely to sign BITs that contain investor-state arbitration clauses.

For wealthy countries, the policy of signing BITs has both economic and political origins. Industrialized countries seek out BITs with developing countries that have characteristics that make them attractive places to invest or that share similar political views to the wealthy country on international issues. At the same time, developed countries are more likely to sign BITs with low-GDP-per-capita countries where FDI inflows are important for economic development (Neumayer 2006).

The literature generally finds that signing BITs increases FDI inflows (Egger and Pfaffermayr 2004; Neumayer and Spess 2005; Bütte and Milner 2008; Kerner 2009; Allee and Peinhardt 2011), although some scholars have found only marginal or insignificant effects (Hallward-Driemeyer 2003; Tobin and Rose-Ackerman 2005). Allee and Peinhardt (2011) find that countries that are accused of expropriation in ICSID cases subsequently lose inward FDI. Critics propose that ICSID clauses help foreign investors threaten governments that attempt to take measures to protect their environment and strengthen the public

welfares (Tienhaara 2009; Gallagher and Shrestha 2011). Haftel and Thompson (2018) find that countries experiencing investor-dispute arbitrations are more likely to renegotiate their content of BITs.

Recent years have indeed seen significant renegotiation and cancelation of BITs by middle-income countries. After losing a dispute in 2011, India subsequently terminated more than 50 BITs and developed a new “Model BIT” that seeks to qualify the broad interpretations of some controversial clauses of older BITs and to delay international arbitration until after investors have first pursued domestic legal remedies (Ranjan 2018; Rolland 2020). Brazil, Indonesia, and South Africa have followed similar paths, terminating many BITs since the mid-2010s and seeking to complement or replace conventional investor-state dispute procedures with domestic legal procedures (Mhlongo 2019; Rolland 2020; Vidigal and Stevens 2018).

POLICIES ON SOVEREIGN DEBT

The extent to which lower- and middle-income countries have raised debt on sovereign debt markets is limited. Gelos, Sahay, and Sandleris (2011) look at 139 developing countries over the period 1980–2000 and find that only 16 of them regularly accessed international credit markets. Since 2005, however, at least 15 low-income countries—including Cote d’Ivoire, Ethiopia, Ghana, Senegal, and Zambia—have issued sovereign debt (Presbitero et al. 2016). Sovereign debt issuance by poor countries is risky because their ability to repay is highly subject to macroeconomic fluctuations. As with dispute arbitration under BITS, the legal frameworks employed in the case of default or sovereign debt restructuring are typically those of wealthy countries. This creates an opportunity for wealthy countries to put in place policies that will either hinder or facilitate debt restructuring by poor countries.

In the 2000s, such policy debates became relevant because of the rise of “vulture funds,” private equity or hedge funds that seek to make profits by purchasing the debt of struggling states at a discounted price and attempting to receive full payment through litigation (Wheeler and Attaran 2003; Blackman and Mukhi 2010). Payments to vulture funds can hinder the economic development of poor countries, since such payments reduce the funding available for building infrastructure or undertaking development projects.

In October 2014, the UN recommended that its members adopt anti-vulture-fund legislation. Belgium, the UK, and France all passed legislation to hinder vulture funds from benefiting from poor countries (Wozny 2017). The UK law discourages vulture funds from seeking whole repayment and provides incentives for multilateral discussions about debt restructuring (Lumina 2018). The Belgian Anti-Vulture Fund Act similarly prevents vulture funds from claiming more than their original purchasing price (Sourbron and Vereeck 2017).

Both the UK and Belgium viewed the passage of their anti-vulture-fund laws as a means of protecting their foreign aid resources intended for development (Sookun 2010, 88). That is, through successful litigation against poor countries, vulture funds were taking resources from those countries that might otherwise be used in a complementary fashion to those being provided by aid donors. NGOs campaigned for these measures to prevent vulture funds from making excessive profits from poor countries (Pettifor 2006).

ANTI-CORRUPTION POLICIES

Corruption plagues the developing world, and huge amounts of ill-gotten gains are transferred out of poor countries on a regular basis, spent on precious metals, luxury goods produced in the developed world, or real estate investments in rich countries (Schott 2006). These outbound flows are likely larger in volume than inbound flows of foreign aid or direct investment, and their existence implies reduced public good and service provision and reduced domestic investment (OECD 2014). Insofar as illicit financial flows are laundered in the developing country, this potentially damages the reputation of the financial institutions in the country (Chaikin and Sharman 2009).

One form of development policy that wealthy countries can implement is policy aimed at reducing illicit financial flows or more generally reducing levels of corruption in poor countries brought about by developed-country business interests. In 1977, the US passed the Foreign Corrupt Practices Act (FCPA), which prohibits American corporations from offering payments to foreign government officials for business purposes.

While the potential impact of the FCPA on corruption in developing countries is clear, the passage of the act is somewhat surprising. Limiting US firms from paying bribes at a time when no similar law existed in other developed countries left US firms vulnerable to losing business activities: the law more or less directly hurt US interests (Earle 1995).

Brewster (2017) proposes that the Watergate hearings contributed to the push to legislate the FCPA. During the hearings, the US public realized that US firms had laundered illegal contributions to the Nixon campaign by using foreign connections (Abbott and Snidal 2002) and had made improper payments to foreign governments (Brewster 2017). Congress worried that such bribery scandals would undermine public confidence in the integrity of US firms and pushed for the FCPA because of the long-term good it might do in terms of restoring confidence in the US system (DOJ and SEC 2012). While Congressional debate about the bill also highlighted the negative effects of international bribery on developing countries' economic development, the motives for the legislation seem to have originated in US actors wanting to restore the reputation of the US (DOJ and SEC 2012).

A broad group of wealthy countries established the OECD Anti-Bribery Convention in 1997. As of 2019, 36 OECD members and eight non-OECD countries had joined the convention. Member states of the Convention penalize domestic firms that pay bribes in accordance with their own laws; the degree of enforcement varies (Spahn 2012). Brewster (2017) argues that the primary motivation for passing the Convention once again originated in domestic corruption scandals. As scandals in Europe made the public highly sensitive to corruption, governments turned to the OECD Anti-Bribery Convention as a remedy (Abbott and Snidal 2002). Once again, the idea that bribery could damage economic development in poor countries seemed like more of a secondary concern.

International organizations and international civil society also played an important role in bringing the anti-corruption agenda to a global stage. Realizing how corruption interfered with the economic productivity of its development projects, the World Bank began a broad conversation about governance and anti-corruption in the 1990s (Weaver 2008; Winters and Kulkarni 2014) and pushed for the establishment of the OECD Anti-Bribery Convention (Brewster 2017). The international anti-corruption discourse in the 1990s and early 2000s

also involved significant input from international NGOs like Transparency International (Earle 1995).

In addition to anti-bribery policies, the 1990s and 2000s saw an increasing prevalence of anti-money-laundering policies. In general, money laundering had not been a crime until 1986 (Sharman 2008); by the end of the 2000s, more than 170 countries had implemented anti-money-laundering policies.

While policies to deter money laundering can have pro-development consequences, the promotion of these policies for developing countries by developed countries again appears to have originated primarily in developed-country concerns. Sharman (2008) proposes that the major drivers of anti-money-laundering policies were efforts to combat drug trafficking and terrorism.

For developing countries, adopting these anti-money laundering policies is typically a burden, even if the policies might have some positive effects in terms of reducing corruption. Anti-money laundering policies require that bank users have proper documentation, and in developing countries, many people lack formal documents (Tsingou 2010). Some scholars believe that money laundering can promote liquidity in the financial system in developing countries, which helps firms borrow from banks and invest (Levi 2002; Williams, Slutzky, and Villamizar-Villegas 2019).

Because of the secretive nature of corruption and money laundering, assessing the success of these global policies can be challenging. D'Souza (2012) finds evidence that the OECD Anti-Bribery Convention changed export patterns in a way that would suggest that companies were more reluctant to use bribes after its ratification. Jensen and Malesky (2018) provide evidence that the same convention reduced acts of bribery by foreign businesses trying to register in Vietnam.

GLOBAL ENVIRONMENTAL POLICY

Climate change demands global solutions, and one of the solutions on which the wealthy countries of the world have agreed is the policy of promoting reduced deforestation in developing countries. According to Blom, Sunderland, and Murdiyarso (2010), deforestation is believed to be responsible for 18 percent of global greenhouse gas emissions. In 2005, at the annual Conference of the Parties (COP) to the United Nations Framework Convention on Climate Change (UNFCCC), Costa Rica and Papua New Guinea, acting on behalf of the Coalition for Rainforest Nations, introduced the idea of “reducing emissions from deforestation in developing countries,” which over the next two years transformed into “reducing emissions from deforestation and forest degradation and the role of conservation, sustainable management of forests and enhancement of forest carbon stocks in developing countries” (REDD+) (Fry 2008).

In 2007, at the COP, the UNFCCC parties discussed a proposal for directly compensating developing countries for undertaking emissions-reducing activities. Reducing carbon emissions in this way was considered cost-effective and technologically feasible, and various parties expected developing countries to be eager to join the regime because of the financial compensation (Pistorius 2012).

Norway has been the most active funder of REDD+ programming (Tellmann 2012). On the one hand, Norway has long been a global leader on climate change issues, having introduced a carbon tax as early as 1991 (Andresen and Butenschøn 2001). On the other hand, Norway's support for REDD+ is due in part to powerful lobbying from domestic industry that hindered the country's ability to put in place new domestic policies that would further cut carbon emissions (Andresen and Butenschøn 2001). With domestic policy change blocked, environmental NGOs in Norway promoted large-scale financing of anti-deforestation measures as a way of addressing carbon emissions (i.e., "doing mitigation abroad") (Hermansen and Kasa 2014). These NGOs promoting REDD+ in Norway also emphasized its development impact.

REDD+ projects mainly target local forest owners, offering them payments in exchange for forest conservation. Such payments require establishing land tenure, yet uncertainty over the ownership of forest lands is ubiquitous in developing countries (Sunderlin et al. 2014; Duchelle et al. 2014). Sunderlin et al. (2018) found little evidence that REDD+ programs across five countries improved the clarity of land ownership, although they also found little evidence of increased smallholder tenure insecurity feared by critics. Generally speaking, under unequal distribution of landownership, REDD+ faces challenges in significantly reducing poverty or alleviating inequality (Chomba et al. 2016; West et al. 2018; Duker et al. 2019); initial evidence about the impact of REDD+ programming on reducing carbon emissions, however, appears to be positive (Duchelle et al. 2018; Roopsind, Sohngen, and Brandt 2019).

MIGRATION AND REMITTANCES POLICY

Although some have worried that migration leads to "brain drain" (where individuals with high human capital exit from poor countries to find better employment opportunities in wealthy countries) and others have alleged that remitted income from migrant workers gets spent on unproductive consumption or causes macroeconomic stability (Rubenstein 1992), the literature seems to have settled on the view that migration from low- and middle-income countries can promote economic growth in both the migrant-sending and the migrant-receiving countries (de Haas 2010, 2012). Regardless of the systemic impacts, migration from a poor country to a rich country is highly likely to lead to an improved economic situation for the individual who migrates.

Whereas migration to wealthy countries has historically provided an outlet for impoverished citizens from other countries (e.g., nineteenth-century Irish migration in response to famine), in recent years, migration policy has become more restrictive. Peters (2017) attributes this to the fact that globalization has allowed labor-seeking firms to offshore production to labor-abundant developing countries and a complementary trend in which technological advances have further dampened the demand for low-skilled labor. This changing political economy of production has resulted in anti-immigration voices being more powerful in policymaking. Peters (2017) argues that changes in production technology provide a more powerful explanation for more restrictive immigration policy than identity-based arguments. In this sense, the lack of a development-oriented migration policy is the

result of changing material conditions in the world, although the absence of development discourse from migration policy debates presents a puzzle.

For people who are able to migrate—legally or illegally—increasing globalization and improved technology have implied a greater ability to send money home. In 2017, remittances flows into low- and middle-income countries reached \$466 billion and these remittance flows have been larger than flows of foreign aid since 2018 (World Bank 2018). Moreover, remittances have been more stable than other sources of external finance, including foreign aid (Mosley and Singer 2015).

Remittances promote economic development at both micro- and macro-levels. Locally, the inflow of money can increase local demand and boost economic activity (Kapur 2010); it can also enhance public goods and service provision (Adida and Girod 2011). Nationally, remittance inflows contribute to foreign exchange reserves that are important for international investment and trade and sovereign interest rates (Mosley and Singer 2015). Giuliano and Ruiz-Arranz (2009) find that remittances particularly promote investment and growth in countries that have underdeveloped finance sectors.

De Haas (2010, 2012) points out that remittances from migrant workers do not necessarily guarantee development; rather their development impact depends on macro-economic policies and other social and political conditions. Adida and Girod (2011) propose that remittances can either complement or substitute for government-provided services, while Tyburski (2014) finds that remittances can either increase or mitigate corruption.

In recognition of the role that remittances can play in development, the wealthy countries of the world incorporated a target for remittances into the 2030 Agenda for Sustainable Development (Martin, Martin, and Cross 2007). Whereas the Millennium Development Goals did not say anything about migration, Goal 10 of the Sustainable Development Goals (SDGs) aims to reduce the transaction costs for sending remittances as a means of reducing income inequality across countries (United Nations 2015). Similarly, the G8 and the G20 have committed to curtailing the costs of sending remittances.

CONCLUSIONS

The policies that the wealthy countries of the world pursue have development consequences—intended and unintended. In this chapter, we have reviewed eight policy arenas where concerns about development have entered or might plausibly enter into decision-making. In the spirit of the editors' characterization of the field of IPE as "problem-driven" in the introductory essay, we have been conscientiously problem-driven in our review of the literature. We draw some tentative conclusions from this review, but we note that there remains significant room to explore the origins and trajectories of these policies through a development lens.

First, we find repeated evidence that pro-development NGOs in wealthy countries mobilize around development issues and meet with success in changing policy. With regard to labor standards, intellectual property rights, and sovereign debt, there is clear evidence that

wealthy countries have put policies in place because of pressure from civil society actors who want better protection for workers in poor countries, more access to patented medicines in poor countries, and a diminished ability for investors to pursue damaging lawsuits against poor countries.

Second, we find evidence that pro-development policies sometimes come about because of domestic or strategic politics that do not directly invoke development concerns. In the realm of trade, part of the logic behind the presence of pro-development trade preferences is that they help wealthy countries maintain a foothold in poor countries in ways that allow the wealthy countries to pursue their geostrategic interests. One explanation for the willingness to reduce intellectual property rights enforcement proposes that this was done for self-interested reasons. Pro-development anti-corruption and anti-money-laundering legislation appears to have originated in a desire to show action on the issue of domestic corruption.

Third, while we show that development discourse does enter into debates on many policy issues, we also show that some arenas are resistant to such discourse. Whereas migration is the most successful individual-level development strategy that there is, there is not much evidence that migration policy has been debated in terms of its benefits for development: the policy debates have remained much more focused on the economic consequences of migration for wealthy countries. Likewise, while it is widely believed that foreign investment spurs development, scholars have not yet collected evidence—if it exists—about how a development lens has altered the design of bilateral investment treaties.

Overall, our review of the literature reveals how much space there is for more investigation of how development discourse enters into policymaking. We find it easy to imagine replicating Van Der Veen's (2011) study of the frames used for debating foreign aid policy in other policy domains related to development: how have humanitarianism and the global benefits of local development been incorporated into discussions of foreign investment treaties and domestic laws surrounding foreign sovereign debt? Which pro-development ideas gain persuasive power and why? We also see significant room for deepening our understanding of how NGOs gain legislative leverage, producing policies that do not necessarily maximize material interest of other important domestic actors in wealthy countries (e.g., a willingness to forego pursuing intellectual property rights cases). In the introductory essay, the editors argue that "the field of IPE is at its best when engagement occurs around substantive problems." We have outlined some substantive problems in need of interrogation in this chapter. We believe that, by applying the development lens to policy areas outside of foreign aid, IPE scholars will be able to deepen our understanding of international political-economic processes more broadly, identifying relevant actors for setting policies and the role that ideas play in policy change.

NOTE

1. USTR, 2019b, "Generalized System of Preferences (GSP)," accessed November 10, 2019, <https://ustr.gov/issue-areas/trade-development/preference-programs/generalized-system-preference-gsp>.

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CHAPTER 42

INFORMAL MARKETS

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INFORMAL markets are a regular part of the economy, and different varieties of informal markets have been analyzed from the 1970s onward—but much of this research has not necessarily been linked to core questions of International Political Economy (IPE), and has been carried out instead by scholars focusing on questions of national labor policies, development, crime, corruption, or security. Therefore, varying emphases and perspectives can be found, including diverse understandings of what “the problem” of informal markets actually is. Informal markets are markets that remain outside formal regulatory oversight. They follow from an intended or unintended mismatch between economic activities and regulation—caused by lack of enforcement, including the systematic undermining, violating, and circumventing of regulations, but also to economic activities that are unregulated and therefore outside of the regulatory scope. This can include a lack of documentation, limited market transparency, or market participants that act illegally, but these elements vary depending on the market and its regulatory context. “Informal” refers to the market activity, not necessarily to the market participants who can range from criminal organizations to undocumented workers and well-respected business people of the formal economy.

What exactly the link is between the legal economy and informal markets has not yet received sufficient attention in IPE. Research on informal markets can draw on two substantially different understandings of informal markets, with research strands that exhibit large variation: The first strand analyzes informal markets with a strong focus on how informality relates to formal markets, how large the informal market is, and what kind of deviation exists between the formal and informal sector (e.g., Roberts 2014, 421; Williams 2014b, 2015b). The second strand analyzes informal markets against the background of illegal markets and criminal acts, focusing on how informality is related to crime, or how criminals use informal market exchange (Andreas 2009; Columb 2017; Friman 2009). While the former strand is concerned with the intersection of research on the national political economy, social policies, and labor markets, the latter strand often has more in common with security studies, criminology, sociology, or economics. A typical example to elucidate the different perspectives of these research strands would be the analysis of digitalization and its impact. Informal markets could be examined under the lens of digital labor markets and precarious types of work—for instance, the “platform economy” (e.g., Kovács et al. 2017). Informal markets could also be examined, however, with a view to illegal markets on internet platforms in the “dark net,” where users invent new forms of exchange to circumvent

difficulties of contracting outside the law (e.g., Tzanetakis et al. 2016). While the differences of informal, illicit, or illegal markets can thus be substantial, terms like “shadow economy” often use these concepts interchangeably when analyzing informal markets (e.g., Peksen and Early 2019; Schneider 2015a, 3–4).

This article shows the commonalities and differences in these approaches, reflecting the problem-based account outlined in the Introduction to this volume. The text is structured along four main problems linked to informal markets, namely the subject of informal markets, theories explaining them, methods for assessing them, and policy solutions for dealing with them. In a first step, the article elaborates on the many varieties and definitions of informal markets. In a second step, the article details explanations for the rise of informal markets. Research typically analyzes the local and national levels of informal markets, and less so the international one, but many links to IPE nonetheless exist in substance. In a third step, the article discusses the methodological challenges linked to assessing the existence and size of informal markets, and the fourth section presents different policies on how informal markets are governed. The concluding section outlines areas for further IPE research on informal markets.

WHAT ARE INFORMAL MARKETS?

The first strand of research on informal markets started in the 1970s as a study of developing countries and their local markets (Hart 1973). Informal economies have since then been a research topic for development studies, with early research assuming that these economic exchanges are uncommon in industrialized countries. Over time, however, research results showed that the dichotomous thinking of informal markets being located in a development context while formal markets were those of developed economies was not applicable, and that even advanced economies still incorporate informal markets, with some of them even expanding (Castells and Portes 1989, 18–20). Research on informal economies can be distinguished as focusing on informal businesses, informal employment, or informal activities more broadly (Williams 2019, 2–4). Informal businesses are small-scale entrepreneurial activities that often require little equipment or capital and rely on the work of the business owner and, if any, a few employees. A typical example would be a small car repair or the provision of household labor to neighbors. The study of informal markets, however, also recognized informal practices in formal enterprises—which means that formal businesses might employ some workers on different, informal terms than others, or that formal labor contracts may be accompanied by informal arrangements. From such perspective, informality is employment-related, not enterprise-based. Further research then developed around informal economic activities in general, which range from precarious employment to hidden cash transactions that remain outside fiscal operations of the political economy. Today, many studies exist on nearly every aspect of informal markets, ranging from structural comparisons of developing countries to examinations of market participants in industrialized countries (e.g., Cling et al. 2015; Williams 2019).

Informality, however, is sometimes also linked to illicit or illegal activities. “Black markets,” the “illicit economy,” or “illicit flows” represent a particular type of informal market (for an early perspective, see Castells and Portes 1989, 14–15). Illicit and informal

flows across countries are an element of mainstream IPE (e.g. Sharman 2011) but they have also received growing scholarly attention at the intersection of IPE and international security. Illicit flows are an important concern with regard to war economies: Circumventing international sanctions, these informal transactions can sustain conflict (e.g., Berdal and Malone 2000; Blattman and Miguel 2010, Collier and Hoeffler 1998), while at the same time, the informal sector typically grows during civil wars (Peksen and Early 2019). Starting with the first examples of natural resources that financed civil wars, a growing amount of literature analyzed wartime economic transactions at the intersection of formal and informal markets. The analyses covered a range of different economic activities and actors, like the financing of civil war through art crime, informal economic activities around cities under siege, or ransom payments to pirates (e.g., Andreas 2008; Efrat 2012). These illegal markets take different forms and involve different crimes. Some informal transactions are aimed to bring illicit products to the legal sector, for instance when undocumented diamonds are mixed with those certified to sell on the legal market (Engwicht 2017, 205–7). Some of these informal market activities involve other crimes like corruption or smuggling. The research emphasis on these illicit, informal economic exchanges reflects two different trends: On the one hand, analyzing illicit flows and economies represented a new perspective on violent conflict, drawing on rationalist economic perspectives. While these perspectives usually had limitations in explaining the huge costs of interstate war (Fearon 1995), they could be more easily be integrated into theories of civil war, showing that war benefited participants economically (e.g., Collier and Hoeffler 1998). On the other hand, this new perspective also paid tribute to the fact that economic incentives for violence and warfare grew, and that a globalizing international economy can thrive on illicit as well as licit flows.

HOW ARE INFORMAL MARKETS CONCEPTUALIZED AND EXPLAINED?

Given the varieties of informal markets, explanations for their existence vary. Labels vary in the literature, but three established arguments exist—linked to ideas of modernization, to liberal ideas of regulatory burden, and to Marxist-inspired ideas of labor exploitation, each with overlap (e.g., Roberts 2014, 420–6; Williams 2015a, 2014a, 196–7; 2019). Following these approaches, modernization theories conceive informal markets as simply not yet regulated. In this reading, informality is a pre-stage of formal market order, and informal markets are thus a step in economic development to be overcome. As a consequence, many economic development programs target informal markets with regulation and, ultimately, integration into formal markets. Informality can, secondly, be understood as a way out of formality, a way to reduce regulatory burden. From this perspective, informal markets can be explained as resulting from the high costs of regulation, including social rights, that incentivize market participants to choose informal employment, businesses, or activities. In contrast to modernization theory, informality would therefore be reduced through less regulation, not increased. Marxist-inspired perspectives, in contrast, would emphasize that informal markets are a necessary element of the regular economy, as they enable better exploitation of labor and capital than regulated sectors. Informal forms of employment are

usually precarious, informal businesses have less costs than regulated ones, and both kinds of informality support holders of capital. These three perspectives not only offer different explanations of informal markets, but also different value judgments linked to them. From a modernization perspective, formal markets are preferable to informal, pre-modern markets. This is not only due to worse labor conditions in informal markets, but also because informality usually hinders effective taxation, upscaling, international market participation, and national competitiveness. If informal markets are explained as an exit option from formal markets, the liberal approach would also suggest that less regulation could result in lower, informal economic activity. From that perspective, the existing regulation of formal markets is the problem. Marxist-inspired political economy, in contrast, rather views the problem in informal markets per se, problematizing that regulation is not comprehensive enough to integrate all economic activities and protect workers (e.g., Roberts 2014). Those perspectives that emphasize the relation of formal and informal markets point at the functions that informal markets can fulfill, recognizing a value in informality as a functional supplement.

Some research on informal markets applies institutionalist arguments—arguing that institutions, or the lack thereof, create a specific informal market. Despite overlap, such work is therefore not necessarily subsumed into one of the above frameworks. For instance, Peksen and Early (2019) show that civil war causes a growth of informal markets, and Elbahnasawy et al. (2016) show that not only taxation, but also systems of democratic representation or democratization processes have an impact on informal markets. Dreher et al. (2008) model how public corruption drives market participants into the shadow economy, while Al-Mataani et al. (2017) show that informal entrepreneurs can emerge due to migration regulations. In all these cases, institutions, and not only regulations on the formal market, influence informal markets.

Some of these above arguments can also be applied to explain illegal and criminal markets (e.g., Beckert and Dewey 2017; Beckert and Wehinger 2012; Reuter 1985). From a liberal economic perspective, illegal markets can provide some goods at a lower price than legal ones—for instance stolen goods or goods that do not comply to some high-cost regulatory requirements. They are thus attractive for buyers and sellers who do not fear the potential costs of circumventing the law. Some illegal markets satisfy unmet demands and provide goods or services that are prohibited and thus unavailable on legal markets, for instance narcotic drugs. Market theories cannot, however, explain all deviant behavior linked to informal and illegal markets, for which criminology has developed a range of different theories (e.g., Liebling et al. 2017; Tierney 2010). For instance, so-called “white collar crime,” like insider trading or business bribery, is often committed by individuals whose personal gains are marginal compared to the legal and social risks taken. Instead of rational choice explanations (e.g., Becker 1968), biased self-perception or perceived organizational demands offer better explanations for such informal activities.

Informal markets also have a gender dimension (e.g., Chant and Pedwell 2008). Gendered participation in informal markets can also be caused by the demand for it, like care work or household tasks that are delivered outside registered companies. Like for other disadvantaged groups, informal labor market participation in low-skilled jobs is also particularly attractive for women if they have a lower degree of education (including education regarding their own rights) and fewer competitive qualifications for formal employment. It is also documented that some global illegal markets—for instance the market related to human trafficking—have specific demands for women as victims (UNODC 2009). Yet, little

systematic knowledge is available on gender dimensions of informal markets, in particular from an IPE perspective.

HOW ARE INFORMAL MARKETS MEASURED?

Much qualitative insight exists regarding local informal markets, but the statistical measurement of informal markets remains a difficult research task. This is due not only to the lack of exact data on non-formal social activity, but also to the varying definitions of informal markets. Some estimates include all non-registered market activities, including illegal and criminal ones, while others isolate those activities that are informal, but not illegal (e.g., Al-Mataani et al. 2017; Dell'Anno and Teobaldelli 2015, 2; Peksen and Early 2019; Schneider 2015a, 3–4; Schneider 2015b; Williams 2019). Informal markets can be measured directly and indirectly: direct approaches use surveys on the participation in informal activities, acknowledging that many participants might hide their activities. Indirect measurements approximate the informal economy by deriving data on very small enterprises, on electricity demand, or employment statistics. Methods assessing the difference between official household income and expenditures are also used. Some estimates are based on calculations of latent variables, like regulatory burden and tax morale (Williams 2019, 35–42). Statistical information on illegal markets is more difficult to gain due to the evasive and often criminal nature of these activities and their unreported dark figure. National and international crime statistics can rely on victims or police reports, convictions, or seizures, but each of the resulting figures bears shortcomings and does not cover all activities that might be relevant for estimating the illegal market (e.g., Andreas and Greenhill 2010b; Heller 2009; Maguire and McVie 2017; Tierney 2010, 40–3). For instance, victims of illegal market activities might not wish to talk about it for fear of shame or punishment, which results in underreporting of illegal market activities. They might not report it because they fear the police, so that police statistics are highly influenced by trust in police forces—a variable that differs significantly across countries. Those engaged in illegal market activities might never be convicted, so that data on conviction are not only influenced by the extent of criminal activities, but also by how effective a judicial system is in holding offenders accountable—again, this variable fluctuates significantly across countries. In each case, the statistical information on illegal markets is thus influenced in distinct ways (Williams 2019).

Much international data is based on national statistics, which is one factor that influences their validity (Linsi and Mügge 2019). Moreover, some market activities that are deemed informal or illegal in some countries might be part of the legal economy elsewhere, raising questions of definition and comparability. Countries are, moreover, not necessarily comparable concerning the quality of their national statistics, which impacts comparative estimates. Some statistics on informal markets rely only on a subset of countries, for instance those of the European Union (EU), where better data is available (Williams 2014b). Other models are based on data that is linked to the legal economy or on other, latent and available statistics (e.g., Schneider 2015a, 5–20). While some comparative statistics on informal markets are based on available statistical information on the legal economy and related factors, similar methods cannot necessarily be applied to estimating illegal markets. Efforts to establish international and reliable statistics on crime date back to the League of Nations, with more

intensive efforts by the United Nations (UN) after the end of the Second World War (Lopez-Rey 1985, 95–100). Today, the UN publishes data that contain comparable statistics against a common classification on crimes, for instance homicide reports, drug trafficking reports and others (e.g., UNODC 2011). The size of illegal markets is intensively debated among scholars, also because the estimates published, while often low quality, still serve political agendas in security-sensitive settings (Andreas and Greenhill 2010a). For instance, the estimation of global money laundering activities has been debated since the 1990s, with a prominent expert group dissolving after agreeing that no serious estimates existed (Levi et al. 2018). Still, many actors—in politics and research—have put forward figures, often with questionable scientific foundations (FATF 2019; Reuter 2013, 224), even criticized as “heroic assumptions” (Levi et al. 2018, 311). Similar problems of data are also known from other illegal markets. Human trafficking data show huge variance amongst publishing actors (see, e.g., General Accounting Office 2006), and research regularly acknowledges the difficulties in estimating the extent of trafficking (e.g., Feingold 2010; Andreas and Greenhill 2010c). Some data is also published by non-governmental organizations—for instance on global slavery or corruption. While often providing estimates not available elsewhere, the quality of this data is usually limited and the data are primarily targeted to prompt political debates. For instance, the corruption figures published by Transparency International are well recognized, but they rely on victim reporting and perception data that might simply misrepresent the extent of corruption (Heller 2009; Transparency International 2016). Quantitative research on informal markets and corruption has also found perception data to be a weaker variable than other, indirect measurements (Dreher and Schneider 2010). All in all, measuring informal economies remains a difficult task, both on the national level as well as from a comparative or global perspective. In particular, it is the quality of statistics on illegal markets which is often low.

HOW ARE INFORMAL MARKETS GOVERNED?

The governance of informal markets has an internal and external dimension. The internal dimension refers to how the market is organized—due to its informality, many institutions that usually regulate and monitor markets do not apply, yet market exchange and organization nonetheless exist (Beckert and Wehinger 2012; von Lampe 2016). The external dimension of informal markets refers to how states try to govern markets, including related policy development and counter-measures. This external governance of informal markets depends on what exactly is identified as “the problem” linked to them, and whether this requires international or national approaches. On the national level, four policy options represent established governance strategies to deal with informal markets, namely to not interfere and take no action, to eradicate the informal market, to formalize it, or to deregulate it (Williams 2019).

If informal markets are mainly reduced to their positive contribution to the economy as a whole, including employment and innovation (e.g., Al-Mataani et al. 2017), and the establishment of similar environments would be difficult with a higher regulatory burden, it may be reasonable to ignore a local or national informal market—in particular since an openly informal market might be preferred over a non-transparent “underground economy.” This

also concerns local illegal markets that provide goods or services without a major impact on the rule of law or citizens' security. The eradication of informal markets—if at all possible—is only one among several strategies to tackle informal markets, and mainly concerns informal markets that are deemed illicit. Formalization, in contrast, requires the transfer to regulated employment, organizations, or activities. For instance, the International Labor Organization (ILO) targets informal markets with regard to formalizing work and employment conditions and strengthening social rights of those currently involved in the informal economy (ILO 2015). In contrast, and based on the assumption that informal markets are an exit from highly regulated formal markets, governance can also aim to deregulate formal markets, so that entry and operation costs in these markets are lowered and businesses, customers, and those employed have stronger incentives to formalize their market activities.

A cross-border market that is difficult to regulate, eradicate, and ignore are informal markets for pharmaceuticals. Pharmaceuticals, in particular established products for large markets, like anti-malarial medicine or antibiotics, are traded worldwide and often through a large number of providers and traders. While developed countries usually regulate pharmaceutical markets strictly, with only a few pockets of informal or lightly-regulated trade, developing countries are more permeable for supply chains that operate outside established quality control. As one result, local stores can sell pharmaceuticals at a cheaper price than other licensed providers. The products sold can be original, yet often they are counterfeit or substandard pharmaceuticals. These informal markets are nonetheless an important market for low-income groups (Zaman 2018). International conventions have been introduced to criminalize and eradicate falsification, but these measures have only been taken in recent years (e.g., Council of Europe 2011). While some parts of the informal markets are thus to be eradicated, others are acknowledged as important for supplying the local population.

A further possibility of governance, the eradication of informal markets on the international level usually means criminalizing and prohibiting markets, which is common in many fields of global crime governance. Bribery, for instance, has been an informal business practice in many countries. In the 1970s, the US prohibited bribery for all companies listed on the New York Stock Exchange (NYSE), while European countries—main competitors to US firms—did not apply similar regulation. This brought competitive advantages to these companies, as they could use informal payments to secure formal public procurement contracts. Only in the 1990s, and following many attempts from the US, did the Organisation for Economic Co-operation and Development (OECD), the Organization of American States, and the Council of Europe, ultimately introduce anti-corruption and anti-bribery instruments that aimed to eradicate these informal payments (Abbott and Snidal 2002; Guttermann 2019). While these regulations have not eradicated corruption, the surrounding informal practices and payments are increasingly perceived as illegitimate and illicit. At the same time—also due to different rules of informality—there remain difficulties in determining where corruption starts and where acts of courtesy end in different cultures. The eradication of informal markets thus not only suffers from regulatory gaps, but also from calculated ambiguity that is inherent to many markets and that leads to difficulties in defining an illegal exchange in contrast to other informal practices.

An important example for formalizing an informal market has been the global diamond market, whose formalization started after the sanctions against conflict-diamonds remained ineffective (Jakobi 2013b). A widely informal market for raw and uncut diamonds thus stretched from mining to trade, and made regulation and separation of conflict diamonds

particularly difficult. Ultimately, the global regulatory regime aimed to formalize markets. Diamonds now need certification of origin, they are transported in sealed containers with documentation, and states establish import and export statistics, which also enable the detection of large-scale informal trading across borders. The global market for raw diamonds is now a widely formalized trade. Still, such formalization is not complete and has not formalized all markets linked to diamond trade. Diamonds are often mined in informal employment or under difficult labor conditions in artisanal mining. Autonomous miners can trade their diamonds informally on local markets, where they are channeled toward the formal certification process. Finally, informal cross-border markets still exist (e.g., Engwicht 2017). The Kimberly Process also had some unintended consequences linked to the formal rule definition. The formalization of the diamond trade was carried out with a view to civil wars—not for other types of conflict. As a consequence, human rights violations caused by states do not constitute an obstacle to certification, as the widely discussed case of Zimbabwe has shown. In that sense, formalization of trading raw diamonds has been criticized as “whitewashing” local conditions (e.g., Global Witness 2011). Notwithstanding these difficulties of the regulatory regime, the supply chains of other conflict minerals are also increasingly formalized, and include certification and documentation that aims to guarantee that the often artisanal mining of these minerals does not benefit rebel groups (Haufler 2015). Comparable regulatory efforts along the supply chain can be found to prevent environmental crime, most prominently illegal logging and related smuggling (Elliot 2012; Cashore et al. 2004).

The difficulties in formalizing informality, including widely different informal practices, are also visible in the regulation of other parts of the global economy. For instance, “shadow banking” is a highly unregulated field in global banking (Helgadóttir 2016), yet it differs significantly from heavily informal banking practices like “hawala.” Hawala is a traditional system of informal money transfer between accounts of trusted traders, often bridging a diaspora and the country of origin, including those where no formal banking system exists. Here there are no formal accounts, and payment orders are informally exchanged among traders. Being feared as a possible tool for money laundering and terrorism financing, this global, informal system has increasingly been formalized by regulations (Lindley 2009; Tsingou 2010; FATF 2013). Whether or not informality is tackled is thus not only a decision of practicability and capability, but also of political preferences.

IPE STUDIES OF INFORMAL MARKETS

Much research on informal markets occurs outside of IPE, but IPE is well equipped to contribute to a more systematic analysis of informal markets. This includes contributions to a better terminology and understanding of informal markets, and better insights on how informal and formal markets relate—also with regard to global markets and supply chains. Also, the global platform and digital-sharing economy often result in precarious employment or markets that undercut existing regulations in countries with strict labor laws—in that sense, informal markets are globalizing to the same degree as formal markets. A more systematic analysis of why specific informal markets emerge, and what kind of theory is best suited to explain their existence, is thus much needed. IPE has shown its strength in research

on the functioning of informality in financial markets and tax regimes (Seabrooke and Wigan 2017; Rixen 2013), including the linkage of national legal systems and the emergence of global market and transaction models outside national regulatory reach. How other forms of informal markets are tied together across different levels still requires further analysis.

In particular, markets at the intersection of illegal and informal are often discussed with regard to decreased regulation, for instance concerning liberal approaches to drug or organ trade (e.g., Becker and Elías 2007). What is often missing in such debates are the societal effects of deregulated markets on societal norms and on the weakest market participants. Economic explanations of informal markets through “exaggerated costs of regulation” sometimes highly simplify policy solutions to informal and illegal markets, suggesting that informal markets disappear with deregulation, despite evidence to the contrary. For instance, one among many liberal arguments against drug policies is the argument that legalization and regulation of narcotic drugs would bring the “black market” to the legal economic sphere and enable oversight and monitoring. Still, current developments show that illegal markets continue to exist, supplying a demand that cannot be realized in the regulated market, for example, due to age limits or higher costs of the legalized drugs (UNODC 2019, 14). Legal and informal markets thus remain deeply intertwined and the latter does not cease to exist if the former expands. Legalization also does not involve side-effects of informal markets like violence as means to enforce rules and target competition in an informal market. Such use of violence as regulatory instrument of criminal groups is unlikely to disappear with the legalization of an informal market. The strength of IPE lies in a broader understanding of informal markets with their multi-level and globalized institutional characteristics.

The terminology of “informal markets” that suggests a merely local context (which is often reflected in case studies of informal markets), and of “illicit economies” that suggests more relevance for international economic exchange, seem to have resulted in different, mostly unconnected research traditions. Yet, IPE could enrich the study of informal markets by emphasizing institutional theory. Institutions, like laws, government agencies, but also borders, are central to the emergence and definition of informal or illegal markets, yet they are rarely analyzed as such. Still, questions that emerge in research on local informal markets can also be applied in IPE: For instance, qualitative research often shows how state formality and market informality are interconnected (e.g., Dewey 2017; Engwicht 2017), which could be similarly relevant for IPE research on illicit markets, offshore finance, or other economic exchange. There is a growing literature on crime and its linkage to global illegal and illicit markets (e.g., Jakobi 2013a; Efrat 2012; Friman 2009; Friman and Andreas 1999), which is usually disconnected from the literature on informal markets.

IPE could develop a more nuanced understanding of informal markets, in particular with regard to the difference between merely “informal” as well as “illegal” or “illicit” markets. This distinction warrants attention as informal and illegal markets exist for different reasons, yet much research on informal markets linked to security or economic models regularly conflates this distinction, either in substance or in the data. Terms like “shadow economy” or “black markets” mean different things, even within similar research traditions, so that cumulated knowledge on the impact of informal markets remains limited. The measurement of informal markets could also benefit from more attention from IPE scholars, as these statistical indicators can be a source of policy decisions, even though they might need more critical discussion. The contribution of IPE could be particularly relevant with regard to the literature on corruption and informal markets, where many economic models show—or

even start with—strong relations between corruption and informal markets (e.g., Dell’Anno and Teobaldelli 2015; Dreher et al. 2008; Gillanders and Parviainen 2018). Yet, this existing research develops little theoretical insights on how exactly corruption and informal markets are related, or how exactly they contribute to each other. Business bribery in procurement is likely to result in a different impact on the formal and informal economy than large-scale petty corruption in all public institutions, but knowledge on these relations is sparse.

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CHAPTER 43

CLIMATE CRISIS

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CLIMATE CRISIS

POLITICAL science has historically treated human-induced global warming as an environmental problem. In International Relations (IR), this means that climate change scholarship exists primarily in the subfield of Global Environmental Politics (GEP).¹ GEP has produced countless insights into the politics of environmental problems. Yet, for complex reasons, it remains relatively isolated from the rest of IR (Morin and Orisini 2013; Keohane 2015; Green and Hale 2017). Until recently, mainstream IPE effectively treated climate change as outside its substantive purview (Newell 2012). Such de facto boundaries have always been misplaced. Market-based mechanisms, like emissions trading, serve as the primary inter-governmental response to climate change. In the postwar period, trade agreements have increasingly incorporated environmental provisions (Morin and Jinnah 2018; Lechner 2016). The major international organizations (IOs) of interest to IPE scholars, like the World Bank, are the largest recipients of public funds for mitigation and adaptation.

But if these boundaries were always misplaced, they are now fundamentally at odds with reality. The European Union (EU) is proposing a “carbon border tax” to facilitate mitigation (Allan 2019); as governments falter to strengthen emission reduction targets, private banks are “waging a war on coal” (Meyer 2019). Central bankers are increasingly called upon to actively address global warming (Tooze 2019). More broadly, addressing the climate crisis requires nothing less than a global energy transition. As Newell writes, “historical precedents for previous transformations in dominant structures of production, finance and technology in the global economy,” must be brought to bear on this central question (Newell 2019, 25). In short, *the climate crisis is an IPE problem*.

This chapter reviews work produced at the intersection at IPE and climate research. The exercise of boundary drawing is not without challenges. Despite a welcome surge in climate-focused articles in generalist IR and IPE journals, historically this work has not been well-represented in top-ranked outlets.² My aim is to cover well-placed scholarship in IR, but also to point to useful, relevant contributions elsewhere. Consistent with the Introduction’s characterization of the broader IPE field, the selected works reflect a diverse set of theoretical approaches and methodological tools (Pevehouse and Seabrooke, this volume). GEP is

an interdisciplinary field at its core, which perhaps dampens stark transatlantic or methodological divides. However, to the extent that IPE scholars begin to engage the climate crisis as a central topic, the “parallel patterns in research,” (Pevehouse and Seabrooke, this volume) that characterize the broader field are likely to emerge. While “constructive non-engagement” between parallel tracks may be satisfactory as the editors suggest, this review reveals many examples where insights likely to fall on one “track” would be useful to those traveling on another.

In addition to reviewing work at the climate-IPE intersection, the chapter identifies IPE insights that are currently detached from climate scholarship but might usefully inform questions related to the climate crisis. In particular, I connect to recent IPE contributions on global performance indicators, patterns of institutional deference, and global value chains. The chapter’s content is not exhaustive, but rather is organized around three substantive areas. I begin with the traditional core of IR climate research, focusing on the UN Framework Convention on Climate Change (FCCC) and its associated finance mechanisms, followed by sections on the trade-climate nexus, and renewable energy transitions. In each section, I identify questions and topics for IPE scholars to engage. Along with similar efforts (e.g., Clapp and Helleiner 2012; Newell 2019), these contribute to an emerging climate crisis agenda for IPE.

THE CLIMATE REGIME

The UN Framework Convention on Climate Change (FCCC) and its subsequent protocols and agreements—notably the Kyoto Protocol and Paris Accords—represent the core of the regime complex for climate change (Keohane and Victor 2011). Emissions targets provide the central component of these agreements. The FCCC included a (non-binding) provision for industrialized states to stabilize greenhouse gas (GHG) emissions at 1990 levels. The Kyoto Protocol created binding emissions targets for industrialized states. These varied widely, with some states committing only to slow rising emissions trajectories (e.g., Australia, Iceland) and others committing to substantial reductions (e.g., Germany, UK). The Paris Accords revert to a non-binding model but one in which *all states* submit an emissions target and a plan to achieve it. These are referred to as intended nationally determined contributions (INDCs) and they too vary widely in ambition.

Scholars have provided explanations of a multitude of FCCC design features and are particularly insightful in explaining how and why different flexibility mechanisms are incorporated to protect states from uncertainty and unwanted costs (e.g. von Stein 2008; Thompson 2009). These explanations treat climate change as a collective action problem, treat states as unitary actors, and shed light on why shallow, even lowest common denominator agreements emerge from the FCCC process. The question of how domestic political actors affect agreement ambition or design has received only limited attention in the literature (Colgan et al. 2020). When domestic actors are incorporated, it tends to be under an implicit assumption that economic actors, and especially polluting industries, uniformly present an obstacle to greater ambition.

IPE scholars are prodding at that assumption, which Genovesi (2019) argues is outdated. An increasingly complex empirical reality shows variation in regulatory preferences across

firms and sectors and investigates how this variation affects state support for national and international climate policy (Genovese 2019; Vormedal 2010; Meckling 2015; Genovese and Tvinnereim 2019). Trade openness, firm reputation, and time, which often maps onto periods of increasingly stringent regulation, are among the factors that explain firm support for regulation. Genovese demonstrates that even across pollution-intensive sectors, the response to international climate regulation is unlikely to be uniform. Rather, those highly engaged in trade are more likely to oppose regulation than others, since the former face international competition and are less able to pass the costs of abatement on to consumers (Genovese 2019, 822). Low-emissions sectors with high levels of trade openness can gain the most from regulation and be its strongest business advocates. Meckling offers a framework in which the strategy of firms facing high compliance costs depends on political pressure for regulation, which impacts firm reputation (Meckling 2015). Firms facing high costs but strong political pressure are more likely to “hedge,” attempting to minimize compliance costs rather than oppose policy outright. Underlining the crucial importance of political pressure, even those firms expected to benefit from regulation may not explicitly support it absent political pressure to do so (Meckling 2015, 23).

Scholars also see longitudinal changes in business preferences for climate regulation over time. Vormedal expects business interests to shift toward advocating for international regulation after opposing (and losing) domestic regulatory battles (Vormedal 2010, 256–7). With domestic carbon regulation in place, polluting firms shift toward advocating for international regulation. This leveling strategy (Meckling 2015, 24) aims to eliminate disadvantages relative to foreign competitors (Kelemen 2010). Alternatively, polluting sectors may support early phase regulation for reputational benefits but become *less* supportive over time as regulatory ambition increases (Genovese and Tvinnereim 2019). This shift can produce “carbon leakage” as energy-intensive sectors move abroad to countries with weaker regulation.

These contributions represent important first steps in understanding how business preferences and strategies affect international climate policy. Empirically, they focus primarily on support for the EU’s Emissions Trading System (ETS). Similar inquiries about firm and sector impacts should be directed toward other regions, but could also be directed to the FCCC agreements to help explain variation and compliance with INDC commitments made under the Paris Agreement.

Climate Finance

Climate finance—defined in the United Nations (UN) context as money for mitigation and adaptation projects in developing and middle income states—has emerged as a second, central component of the FCCC regime. This funding is essential to reducing the carbon footprint of industrializing states and helping vulnerable states to adapt. Three IOs, the Global Environment Facility (GEF), the Green Climate Fund (GCF), and the Adaptation Fund, serve as formal financial mechanisms of the UN treaties. Climate funds at the World Bank, regional development banks, and UN organizations, also distribute money. To date, scholarship focuses primarily on questions related to the governance and efficiency of the financial mechanisms (Graham and Thompson 2015; Graham 2017), and on distributional questions (Bayer et al. 2015; Iannantuoni et al. 2021; Graham and Serdaru 2020).

Historically, the GEF required developing states to work with a designated “implementing agency” to receive GEF funds. Studies have examined how the type of implementing agency—whether it is an “egalitarian” UN agency or a “non-egalitarian” multilateral development bank (MDB)—affects how funds are distributed. Bayer et al. (2015) found that for projects implemented by the World Bank, the recipient’s economic strength predicted the GEF’s funding share. Recipients with low economic strength fared less well in terms of the proportion of their project paid for with GEF funds (Bayer et al. 2015, 1092). More recently, Iannantuoni et al. demonstrate that when external funding from the implementing agency and other foreign funders is included, this finding disappears (Iannantuoni et al. 2021). Instead, stronger economies pay a greater share of resources for projects than poorer ones across the Bank and UN agencies.

Perhaps due to its longevity, the GEF receives disproportionate scholarly attention. But the GCF’s establishment, the continued presence of the World Bank climate investment funds,³ and the gradual shift in the GEF’s portfolio toward non-climate (environmental) projects, mean that in relative terms it is receding from the multilateral climate finance landscape. Graham and Serdaru (2020) expand the empirical scope of climate finance studies to explain institutional design outcomes across 18 multilateral climate finance institutions. They demonstrate that large economy-donors push for climate finance institutions to be hosted by MDBs rather than UN organizations. When the UN is the host, donors lobby for earmarked funding rules that allow them to exert influence over how funds are distributed.

These “hybrid” designs (egalitarian voting rules with earmarked funding rules) affect conventional expectations about how egalitarian and non-egalitarian institutions distribute money and are likely to influence forum-shopping dynamics. Beyond expanding studies on distributional impacts beyond the GEF, research should address questions related to the supply of climate finance. States vary in their willingness to deliver funds and in their preferred forum. These questions remain largely unexplored.

THE TRADE-CLIMATE NEXUS

Trade represents the most populated intersection of IPE and GEP. Trade liberalization writ large and its more specific legalized components have direct impacts on the climate crisis. From a critical view, the expansionary logic of liberalization, with goals of facilitating increased consumption and growth, is inherently problematic for sustainability. More narrowly, the strength of the trade regime relative to international environmental agreements is problematic from the perspective of environmental protection. Eckersley (2004, 25) makes the point effectively by characterizing the World Trade Organization (WTO) “as a large tiger and the MEAs (multilateral environmental agreements) as a ragged collection of small cats.”

One important body of scholarship investigates how these two regimes interact. In the 2000s, scholars argued that the WTO had a “chilling effect,” both on the likelihood that states’ would implement trade restrictive measures to meet MEA goals, and on the negotiated content of international environmental agreements. The former expectation rested on the premise that, fearing legal challenge at the WTO, states would steer away from trade restrictions to achieve the environmental goals outlined in MEAs. The latter claim implied that MEA designs would be disciplined in the post-Uruguay Round era to achieve

WTO compliance (Conca 2000). In particular, Conca predicted that the WTO's establishment would effectively end the inclusion of trade restrictions in environmental agreement design.

The conventional wisdom reflects Eckersley's characterization of the WTO and MEAs (Thomas 2004), but some suggest that concerns are exaggerated (Young 2005; Neumayer 2004, 1–3), that the WTO Appellate Body moved toward greater incorporation of environmental concerns over time (DeSombre and Barkin 2002), or that it contains the potential to do so (Thomas 2004; Eckersley 2007). Somewhat surprisingly, there are few systematic analyses of the WTO's effect on MEA design or on states' strategies to achieve goals related to environmental agreements.⁴ The concept of *institutional deference*, developed elsewhere in the IPE literature (Efrat and Newman 2016; Pratt 2018) and defined as one actor's acceptance of another's authority could be usefully applied to these questions. Eleven percent of states' INDCs under the Paris Agreement “entailed a reference to the regulation of trade on climate grounds” (Das et al. 2019, 10554). The rise of green industrial policy to support domestic renewable energy industries is now widespread (Lewis 2014, 14). These policies, which include feed-in tariffs, subsidies, and local content requirements (LCRs), implicate a range of WTO provisions (Lewis 2014, 12–13) and LCRs have become frequent targets of WTO disputes (De Bievre et al. 2017). The WTO Secretariat has signaled its openness to carbon border taxes if appropriately designed (WTO 2009)—but they too may be legally contested.

It remains unclear whether the climate crisis rationale is legally sufficient to exercise exceptions to WTO and other PTA rules (Lewis 2014, 13). But it is plausible that as the crisis deepens patterns of deference may shift. Scholars point to various ways this could occur via state decision-making or Appellate Body judgments (Das et al. 2019). Climate-related waivers agreed to by the WTO Ministerial Conference could integrate carve-outs that allow for otherwise prohibited subsidies and LCRs to promote renewable energy (Eliaison 2019, 565ff).⁵ Alternatively, “environmentally motivated arrangements” could “act as an overlay on prevailing trade and financial arrangements in the WTO and IMF” (Dong and Walley 2008, 4–5). In such a system, trade rules would be reinterpreted through the goals of the Paris Agreement. In either approach, LCRs that promote renewable energy would be treated differently than other trade-distorting subsidies based on their environmental benefit. Fossil fuel subsidies—which have not been the subject of WTO disputes—could also be treated differently as patterns of deference shift (Meyer 2013).⁶

Beyond the WTO, scholars investigate the inclusion of environmental provisions in preferential trade agreements (PTAs). Lechner argues that domestic lobbying explains variation in the design of provisions not directly related to trade, including environmental standards. Which domestic groups are mobilized depends on the type of states included in the PTA. For instance, when a negotiating partner poses concerns due to its relatively cheap labor, unions and import-competing firms are likely to lobby for environmental provisions (Lechner 2016). Environmental provisions are a function of concerns about job loss rather than environmental protection. By contrast, among Swiss nationals, individuals with stronger environmental concerns held less favorable views of economic openness and held more protectionist attitudes than others (Bechtel et al. 2012). The findings suggest that a deepening climate crisis will reduce support for liberalization and increase support for strong environmental provisions independent of economic calculus.

The Trade and Environment (TREND) dataset introduced by Morin, Dur, and Lechner provides new opportunities to understand the broad contours of the trade-environment

nexus and explain variation across agreements (Morin, Dur, and Lechner 2018). The dataset codes environmental provisions in 630 trade agreements signed between 1947 and 2016. Environmental provisions in trade agreements have increased over time, and have grown more robust (Morin et al. 2018; Jinnah and Lindsay 2016). Using the dataset, Morin and Jinnah (2018, 542) provide the first systematic account of “the regulatory contribution that PTAs make to global climate governance.” They conclude that despite enormous potential, the weak legalized nature of most climate provisions leaves their potential mostly untapped.

Global Value Chains

Approximately two-thirds of world trade occurs through global supply chains.⁷ Supply chain globalization has been shown to have significant impacts on attitudes toward trade liberalization (Osgood 2018), the types of firms that support open trade (Meckling and Hughes 2017), and firms’ preferences in trade agreement design (Kim et al. 2019). It has also had significant effects on carbon emissions. Reports indicate that companies’ supply chains produce “on average more than five times the emissions from their direct operations” (Carbon Disclosure Project 2019, 5). Historically, concerns about the competitiveness of national companies make governments reluctant to legislate extraterritorial liability for buyer firms that would make them accountable for the behavior of their suppliers (Evans 2020). Some attribute the absence of regulation to neoliberal ideology, but this seems insufficient since at the domestic level neoliberal ideology coexists with varying degrees of environmental regulation and workers’ rights.

Two recent trends require attention. First, buyers are taking voluntary action to encourage suppliers to shift toward greater reliance on renewable energy (Carbon Disclosure Project 2019). As Ponte notes, lead firms increasingly take a proactive role on sustainability issues. They are often motivated to do so to mitigate reputational risks, improve their bottom line, and increase control over supplier firms, which often bear compliance costs (Ponte 2020, 75). Second, the potential for national legislation that applies extraterritorial liability to domestic firms now has precedent. France’s Duty of Vigilance Law (2017) marks the first legislation mandating “that large French firms reduce such risks [related to environmental abuses and human rights] in their global production networks,” thereby creating extraterritorial liability for buyer firms (Evans 2020).

At least three sets of questions are relevant for IPE scholars. First, scholars investigating the politics of benchmarking or “global performance indicators” (Kelley and Simmons 2019) should consider available metrics from the CDP, which adds companies to an “A” List (or not) based on environmental performance.⁸ Consistent with Green’s conception of entrepreneurial authority, private actors like the CDP are attempting to attract “those who might comprise the governed” (Green 2013, 7). Here they have had some success: a growing number of firms report emissions data to the CDP each year, but reporting varies by country, region, and across sectors, raising questions about the politics of who reports, why, and when. Second, absent regulation from states, does transparency in emissions reporting, and the inclusion of environmental metrics in firms’ accounting,⁹ effectively “change the chain”? That is, do buyers use reporting data to alter the chain toward green suppliers? If so, under what conditions does this occur? Third, how does the emergence of national-level regulation interact with private, voluntary initiatives? The potential for domestic legislation that

makes buyer firms accountable for supplier behavior—an increasingly plausible scenario as other states consider legislation like France’s Duty of Vigilance Law—is itself a risk that buyer firms must manage that may lead to more robust voluntary regulation.

THE TRANSITION TO RENEWABLE ENERGY

Early framings of the climate crisis employed Garret Hardin’s classic “tragedy of the commons” (ToC) (Hardin 1968; DeSombre 2004; Paavola 2012; Brown and Adger 2019). In that frame, the atmosphere is a global commons subject to GHG emissions; actors’ mixed motives combined with uncertainty about others’ behavior and intentions and the absence of enforceable rules provide barriers to cooperation. Standard neoliberal institutionalist solutions come to mind: cooperation can be achieved through the design of international rules that lengthen the shadow of the future (Axelrod 1984), and improve transparency of state behavior (Keohane 1984; Mitchell 1998). However, the stunted nature of intergovernmental climate policy relative to other environmental problems (e.g., ozone) and other issue areas (e.g., trade), suggests that GHG emissions are especially hard to quit and cast doubt on the relevance of the ToC frame.

Drawing on the concept of path-dependence, Unruh usefully reframed climate change as a problem of “carbon lock-in,” writing that “industrial economies have become locked into fossil fuel-based technological systems through a path-dependent process driven by technological and institutional increasing returns to scale” (Unruh 2000, 817). In a distinct but related argument, Colgan, Green, and Hale (2020) argue that holders of climate-forcing assets (those that contribute to climate change) have consistently obstructed effective climate policy. The scope of carbon lock-in has been expanded to include “governance, institutions, and decision-making that affect energy production and consumption” and behavioral norms and habits “associated with demand for energy-related goods and services” (Seto et al. 2016, 427). The growing literature on decarbonization examines how change occurs in path-dependent systems and, importantly, how green energy might develop its own path-dependent dynamics (e.g., Aklin and Urpelainen 2013; Farmer et al. 2019; Bell 2020; Kelsey 2019).

Some scholars point to the importance of changing ideas about the relationship between economic growth and climate mitigation. Through an analysis of IO publications, Meckling and Allan (2020) demonstrate that neoclassical economic ideas, which understand economic growth and environmental protection as only weakly complementary, dominated in the 1990s. Today, an alternative green growth concept underpinned by Green Keynesian and Schumpeterian ideas is equally popular (Meckling and Allan 2020). Others emphasize critical junctures to jumpstart change (Aklin and Urpelainen 2018). For example, the first phase of a global energy transition might be caused by external shocks (e.g., a sudden increase in the price of oil caused by a terrorist attack or a global pandemic) (Aklin and Urpelainen 2013, 48). Shocks open space for affected states to consider renewables. But to compete, renewables initially require government assistance. Schmitz et al. refer to such policy as “green rent management,” in which the government provides opportunities for above average profit on green investment and withdraws opportunities for such profits on dirty investment (Schmitz et al. 2013, 5).¹⁰ These contributions should be read jointly: green rent management is more likely when Green Keynesian ideas rather than neoclassical ones dominate.

Critical junctures do not always alter paths, which heightens the importance of work that investigates how to build “winning coalitions for climate policy” (Meckling et al. 2015). For instance, experimental research indicates that individuals working in polluting industries are less likely to support climate policy than others (Bechtel et al. 2019), pointing to the importance of offering alternative job opportunities for those employed by the fossil fuel industry. Echoing early GEP contributions on international institutional design (e.g., Mitchell 1994), Bechtel and Scheve find that design matters; individuals are more likely to support international climate cooperation when agreements involve lower costs, but also when they distribute “costs according to prominent fairness principles,” and include “a small sanction if the country fails to meet its emissions reduction target” (Bechtel and Scheve 2013, 1).

In most models, government intervention drives the first phase of the renewable energy transition in which politicization and the preferences of economic actors play important roles in enabling or impeding the process.¹¹ When state investments impact the price, market effects should reinforce a positive trajectory for renewable energy (Bell 2020). This stage is well underway, especially in the solar industry. The question that follows is the extent to which private money from fund managers will flow toward green energy and away from fossil fuels. Absent regulation and enforcement from governments, change requires that fund managers extend their time horizon to incorporate medium-term risks in investment decisions. Harmes cautions that such behavioral change is difficult given that fund managers are under “intense pressure to attract and retain customers by making sure their funds perform strongly in the short-term” (Harmes 2011, 104).

Announcements to move away from fossil fuel investments by financial institutions like BlackRock, Goldman Sachs, and HSBC, and insurance companies like The Hartford, suggest the tide is changing.¹² In his 2020 letter to CEOs, BlackRock Chairman and CEO Larry Fink writes that “climate risk is compelling investors to reassess core assumptions about modern finance” and he predicts that “we are on the edge of a fundamental reshaping of finance.”¹³ Questions about implementation abound. We know little about variation in firms’ positions on the issue, both across firms and over time. Longitudinal variation may primarily be a function of the costs associated with climate risk moving into the present (Bell 2020, 63–4), and the declining profitability of coal. Increased public pressure, especially via targeted divestment movements, may also play a role. Diffusion effects are likely as announcements from large financial institutions increase pressure on others.

The potential emergence of private financial institutions as constructive actors on the climate crisis during a period in which international climate policy continues to disappoint raises questions about the role of the state and the centrality of intergovernmental problem-solving moving forward. Similar questions are being asked about global economic governance, where scholars argue that the US retreat from global economic cooperation is both unlikely to unravel integration and is likely to alter it in ways that further empower multinational corporations (Johns et al. 2019). Substitute, private governance arrangements may gain steam if large fund managers and buyer firms begin to implement their stated desire for change in earnest. However, even in a world in which renewable prices fall and private actors play a constructive role, state policy choices will continue to impede or enable transitions. Scholarship aimed at answering questions related to which route states choose—like all others raised in this review—will be better off for IPE scholars having engaged them.

NOTES

1. Some label the field International Environment Politics (IEP), others label it Global Environmental Governance (GEG).
2. Green and Hale (2017) demonstrate that from 1980 to 2012, just 1.2 percent of articles published in 12 top IR journals were from the GEP subfield.
3. For an overview of the Bank's role in climate finance see Michaelowa and Michaelowa 2011.
4. The International Environmental Agreements Database Project should be helpful in investigating these questions, see Mitchell 2020.
5. Eliason points to waivers agreed to by the WTO Ministerial Conference on food security for climate-related reasons as a template.
6. See Tucker 2019 for a proposal to use the WTO as an arbiter of a two-track system in which states implementing "new green deal" policies receive favored treatment.
7. Global Value Chain Development Report 2019. <https://www.wita.org/atp-research/global-value-chain/>
8. New metrics are likely forthcoming. See Shaw and Grant 2010 on the emergence and incorporation of environmental metrics in measuring supply chain performance.
9. For instance, Bell (2020, 68) notes that "A range of firms, including Dow Chemical, Microsoft and Mars have begun factoring in a notional carbon price in internal accounting and investment planning. CDP analysis shows that in 2014 150 major companies had done the same."
10. Schmitz et al. (2013) also make an important meta-level point. Scholars can draw insights from economic transitions that are not directly related to energy to theorize how decarbonization dynamics could occur. Their own work draws on economic transitions of East Asian economies.
11. See especially Aklin and Urpelainen 2018; On states differentiated investments on green energy, see Lachapelle et al. 2017.
12. Bill McKibben (2020) asserts that "seismic" is the only word to describe BlackRock's decision to begin redirecting investments away from fossil fuels.
13. Larry Fink 2020 CEO Letter, "Fundamental Reshaping of Finance." <https://www.blackrock.com/corporate/investor-relations/larry-fink-ceo-letter>

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CHAPTER 44

SEXUAL INJUSTICE

ELLIE GORE AND NICOLA J. SMITH

INTRODUCTION

IN April 2022 the UK government sparked controversy by announcing that trans people would be excluded from its proposed ban on “conversion therapy.”¹ This incident marked the latest in a series of pushbacks against trans rights, which have been dubbed by the British media as a “trans culture war” (Ferber 2022). The increasingly hostile environment facing trans people in the UK is reflected in the International Lesbian and Gay Association’s (International Lesbian and Gay Association; ILGA Europe 2022a) “Rainbow Europe Index 2022,” which places the UK in 14th position, a drop of four places. Backlash against trans and other dissident forms of gender and sexuality is not new. Yet these struggles have taken on fresh intensity in the contemporary juncture, in the context of growing right-wing populisms and the global “anti-gender” movement. This movement comprises a loose and heterogenous coalition of conservative forces that is seeking to roll back “progressive legislation won in the last decades by both LGBTQI and feminist movements” (Butler 2021). Key targets of this movement include reproductive freedom; single-parent families; legal and policy initiatives aimed at addressing sexual and gender-based violence; and queer and trans activism.

As these dynamics indicate, sexuality—broadly construed to include sexual practices, behaviors, desires, and identities—remains a key site of political struggle and intervention around the world. From a political economy perspective, the conversion therapy controversy illuminates the central role of the state in regulating sexual orientation and gender identity, notably by introducing (or reforming) laws and policies that govern intimate relations. The state is, as Suzanne Bergeron and Jyoti Puri (2012, 493) note, a particularly “dense node of governance” when it comes to sexuality. This means that state laws, policies, and practices have wide-ranging material consequences for minoritized gendered and sexualized individuals; they can result in criminalization and incarceration, exclusion from employment, citizenship, and the right to family life, the denial of healthcare, housing, and social welfare, and restrictions on freedom of expression, association, and assembly. However, even in contexts where sexual orientation and gender identity are legally-protected characteristics, people continue to encounter violent sanctions for transgressing hegemonic

sexual and gender norms, as exemplified by the elevated levels of LGBTQ+ “hate crime” across Europe (ILGA Europe 2022b). Thus, in many parts of the world, queer and trans lives are shaped by both state-sponsored and extra-legal forms of discrimination, oppression, and targeted neglect (Snorton 2017). This is especially the case for queer and trans people living at the intersections of racial, geopolitical, and socioeconomic hierarchies—such as poor and working-class queers, Black, Indigenous, and queer people of color, queers with insecure immigration/citizenship status, and queer people living in the Global South—who are at disproportionate risk of violence and premature death.

In this chapter, we explore some of the material bases of sexual injustice in the global economy. In so doing, we highlight what a queer approach to International Political Economy (IPE) tells us about the relationship between capitalism and sexuality. We argue that although mainstream (and some critical) IPE has tended to overlook the importance of sexuality, there is a rich tradition of feminist and queer scholarship that locates sexual subjectivities, labor, and oppression firmly within the boundaries of the global capitalist economy. In order to unpack this, we examine the relationship between sexuality and IPE across three primary dimensions: hetero- (and homo-)normativity and the nation-state; hetero- (and homo-)normativity and global governance, including global development processes; and the everyday political economies of queer lives and resistance. In essence, we argue that it is impossible to understand the social relations of global (re)production and the types of exploitation and oppression they entail without paying attention to the social construction of sexuality (and therefore to the lives and experiences of LGBTQ+ people). Moreover, since capitalism constitutes (and is constituted by) sexuality across multiple scales and modes of governance, sexual injustices should be understood as fundamentally globalized in character. This means that scholars of IPE must not only interrogate the sexual politics of neoliberalism and how this plays out in different parts of the world, but also examine how the legacies of slavery and colonialism continue to shape global terrains of sexual struggle and injustice.

SEXUALITY IN/AND IPE

As feminist critiques have long shown, IPE as a discipline has not historically been organized around questions of embodiment, intimacy, and desire (see *inter alia* Pettman 1997; Youngs 2000; Peterson 2003; Agathangelou 2005; Chang and Ling 2010; Bedford and Rai 2010; Griffin 2009; Daigle 2019; Hozic 2021). In part, this reflects an enduring preoccupation with what Anna Agathangelou calls the “upper circuits” of capitalist power relations (e.g., trade, investment, and finance) as opposed to its “lower circuits” (e.g., caring and reproductive labor) (Agathangelou 2005, 7). But it also reflects how IPE has tended to regard sexuality itself as external to global capitalism and hence beyond the purview of political-economic analysis. This is a consequence of disciplinary histories, for IPE was founded on, and continues to bear the imprint of, a number of dichotomies and dualisms. These include states/markets, politics/economics, national/international—all of which are regarded as internal to the field—as well as the boundaries that demarcate the inside and outside of IPE (Pettman 1997; Youngs 2000; Steans 2003). Especially significant here has been the dichotomy between the public and private realms, with the former associated with “the world

of political and economic power, of decision-making and influence” (and hence within the remit of IPE) and the latter with “the world of social reproduction, of home and family” (and hence beyond the remit of IPE) (Youngs 2000, 19). In this context, it is no accident that sexuality—that most “private” of social relations—has frequently been assumed to have scarce, if any, relevance for the study of IPE.

This is not to imply, though, that little or no scholarship on sexuality exists in IPE, for feminists have long been forging lines of inquiry into the intersections between economic and sexual (in)justice both within and outside of IPE (see, for instance, hooks 1981; Mies 1986; Gibson-Graham 1996; Pettman 1997; Ling 2002; Peterson 2003; Federici 2004; Agathangelou 2005; Meger 2016). Yet, despite the empirical presence of these longstanding interventions, sexuality has frequently been positioned on the constitutive margins of IPE. That is to say, although there have been numerous studies of explicitly sexualized themes ranging from sexual violence to sex work in IPE, sexuality is rarely if ever taken up as “ontologically central” (Waylen 2006, 147) to either mainstream or critical political economy. In this context, sexuality is typically regarded as an unproblematic synonym for sexual orientation and gender identity to be added on to pre-existing frameworks rather than as intrinsic to, or transformative of, such frameworks. Thus, despite the vital, vibrant work already being undertaken on sexuality and political economy, IPE has on the whole proceeded “as if” sexuality is neither particularly relevant to the field nor constitutive of global capitalism itself.

This is a problem because, as feminist political economists have been arguing for decades, the economic is the intimate, the political is the personal, and the sexual division of labor is a major part of how labor is organized under capitalism. Feminists have been at the forefront of attempts to expose and contest the state-centric and productivist foundations of IPE (Waylen 1997; Peterson 2003; Bakker 2007; Steans and Tepe 2008); they have highlighted how the structures and hierarchies of global capitalism are not only gendered but also racialized, sexualized, classed, ableized, and territorialized (Elson 1999; Steans 2003; Peterson 2003; Kapur 2005; Waylen 2006; Puar 2017; Bhattacharyya 2018; Tilley and Shilliam 2018); and they have illuminated the centrality of reproductive labor to the functioning of the global economic order (Bezanson 2006; Bakker 2007; Hoskyns and Rai 2007; LeBaron 2010). Importantly, and as part of this wider project, feminists have made visible the role of sexuality in sustaining gender hierarchies such that sexuality is “systematically tied to the mode of production” rather than being separable from it (Butler 1997, 272). In so doing, they have problematized not only how “states and markets” are themselves gendered constructs (Youngs 2000; Bedford and Rai 2010) but also how the separation of gender and sexuality from political economy serves to reproduce gendered and sexualized power relations—including by rendering invisible how reproductive labor is “integral to the functioning” (Elias 2010, 849) of the global political economy.

Feminist analyses of the interconnections between sexual and economic (in)justice have thus documented the pivotal importance of reproductive labor, the family, and the household in capitalist relations. Indeed, there is a rich tradition of feminist scholarship, going back to the 1970s and beyond, that has conceptualized sexuality in terms of women’s sexual labor as part of larger efforts to connect patriarchal power relations to capitalist economic structures (Mies 1986; Hartmann 1979; Vogel 2013). The Wages for Housework campaign, for instance, was fought on the basis that women’s caring and sexual labor in the household was highly profitable for capitalism, and yet what enabled this labor to be appropriated and extracted as unpaid work was that it was not being recognized as “work” at all (Federici 1975).

In Marxist feminist theorizing, social reproduction comprises the “acquisition and redistribution of the means of existence,” such as shelter, food, and clothing, as well as the reproduction of the labor force (Katz 2001, 711). Intimate relations, family structures, and households therefore *matter* because the reproduction of persons is key to the sexual division of labor and to the overall reproduction of the working class. Furthermore, social reproductive labor is not just gendered and sexualized, but is also differentiated by race, geography, and citizenship (Davis 1983; Glenn 1992; Bhattacharya 2017). Although diverse, this vein of feminist scholarship has usefully re-drawn the boundaries of the economy to include sexual labor. In so doing, it has shown how the construction and control of (women’s) sexuality is internal to capitalism *and* how it relates to other axes of oppression, such as race and gender.

This focus on women’s lives and labors has been vital, given how gender works as a structural and systematic relation of inequality (and how feminized labor has been historically invisibilized). However, it does mean that the feminist subject in IPE has tended to be women and, when it comes to sexual matters, (cis and heterosexual) women’s bodies and sexuality. As invaluable as these contributions have been, then, they have at times been marked by a tendency to equate sexual relations with heterosexual relations, to use gender as a synonym for women, and to reify rather than destabilize the nuclear family (Danby 2007; Bergeron 2009; Barker 2012; Capper and Austin 2018; Thorburn 2020). This means that feminists have sometimes perpetuated the “presumption of heterosexuality” (Richardson 2004, 405; Carver and Chambers 2007, 155), thus limiting the potential of feminist political economy both to denaturalize sexuality as a system of domination/subjugation and to center questions of queer resistance and oppression. Put otherwise, feminist IPE has usefully shown how heterosexuality is produced and institutionalized through labor and household relations. But it has not always fully articulated the material impacts of these processes on queer and trans subjectivities (Raha 2021)—and, as we shall now explore, this is where queer theory (and especially queer political economy) comes in.

QUEER THEORY AND POLITICAL ECONOMY

Before turning to the burgeoning field of queer political economy specifically, it is worth spending a moment to describe and reflect on the wider field of queer theory since (like sexuality more broadly) this has so often been neglected in IPE. Rising to prominence in the 1990s, queer theorists such as Judith Butler have sought to deconstruct and disturb essentialist understandings of sex and gender—including the primacy and coherence of the category “woman” (Butler 1990, 1993)—as well as to delineate more explicitly between the analysis of gender and that of sexuality (Rubin 1984). Alongside other projects (see, for instance, Ahmed 2010; Halberstam 2011; Kafer 2013; Haritaworn, Kuntsman, and Posocco 2014; Hong 2015; Harris and Jones 2019), queer theorists have explored how sexual and other identities are constructed as uniform and static rather than as multi-layered and flexible, and how particular embodiments, expressions, and exchanges are authorized, naturalized, and privileged while others are rendered aberrant, unnatural, and illegitimate (Danby 2007; Giffney and Hird 2008; Browne and Nash 2010; Weber 2016;). Consequently, many queer scholars use “queer” not only as a noun (“being” queer) but also as a verb (“doing” queer) (Berlant and Warner 1995; Jakobsen 1998; Yekani, Kilian, and Michaelis 2013) so as to avoid it

from becoming a site of identity politics in its own right (as in Judith Butler's oft-cited claim that queer is "never fully owned, but always and only redeployed, twisted" (Butler 1993, 228)).

In rejecting the fixity of social categories, queer theorists challenge the very basis of a great deal of social research, not least by questioning how foundational categories such as "man" and "woman" have so often been treated as variables to be identified, classified, analyzed, and understood by social researchers (Browne and Nash 2010). In so doing, queer scholars have explored how the marking-out of discrete identities—including those of the "heterosexual" and the "homosexual"—is exactly what makes it possible to not only to demarcate between "normal" and "abnormal" sexualities but also to organize and discipline them (Foucault 1978; Danby 2007; Browne and Nash 2010). Indeed, it is this move that allows heterosexuality to be constituted as normative (i.e., heteronormativity), even as "[i]ts coherence is always provisional, and its privilege can take several (sometimes contradictory) forms: unmarked, as the basic idiom of the personal and the social; or marked as a natural state; or projected as an ideal or moral accomplishment" (Berlant and Warner 1998, 548). One of queer theory's key interventions has therefore been to challenge prevailing assumptions that sex and sexuality are somehow natural, immutable, and, above all, outside of power (Foucault 1978).

However, and despite the huge influence of queer scholarship across the social sciences and humanities,² queer contributions have largely been ignored in IPE. As for the study of sexuality more widely, this is not to suggest that queer theory is wholly absent from IPE since numerous scholars are already undertaking vitally significant work in this vein (see *inter alia* Peterson 1999; Bedford 2005; Agathangelou, Bassichis, and Spira 2008; Bergeron 2009; Lind 2010b; Griffin 2011; Rao 2015; Díaz Calderón 2021). But it is nevertheless the case that queer concerns have often been placed on the disciplinary outskirts of IPE, thus allowing the field to define itself "as if" queer theory does not matter. This means that IPE is still a long way off from addressing, let alone fully integrating, the political, methodological, and ethical implications of centering sex, sexuality, and sexual justice in the study of global capitalism.

To be fair, queer approaches have for their part often neglected insights from political economy—and especially feminist political economy—as pertinent to the study of sexuality. Indeed, some critics have even called for a "post-queer" agenda (Ruffolo 2016, 1) on the grounds that it is "alarmingly distanced from the critique of capitalism" (Penney 2014, 3) and so lacks the tools to engage in political-economic analysis. Yet, while queer studies have by no means always centered political economy questions, it is certainly not incompatible with, or incapable of undertaking, the study of global capitalism. Nor is queer theory's desire to deconstruct and dismantle the naturalization of heterosexuality and normative gender at odds with IPE's interest in the unequal distribution of power, wealth, and resources or its exploration of the mechanisms through which such inequalities are produced in the first place. Indeed, over recent years queer scholars have increasingly been positioning sexual justice within the context of economic justice (Bedford and Jakobsen 2009), arguing that the study of sexuality must attend to the dynamics and structures of global capitalism, and vice versa (see, for instance, Cornwall 1997; Duggan 2003; Hennessy 2006; Binnie 2009; Joseph 2010; Hong 2012; Fojas 2017; Ferguson 2018; Keeling 2019; Gržinić, Kancler, and Rexhepi 2020). In so doing, they have pointed to the material effects of economic injustice on queer lives including in terms of unequal welfare and healthcare, unemployment, homelessness, and incarceration—outcomes that are inextricably intertwined with structural inequalities of gender, race, dis/ability, class, and territory (DeFilippis 2012).

That having been said, the emergent field of queer political economy could do more to openly acknowledge the substance and significance of the feminist political economy contributions discussed earlier (although see, for instance, Butler 1997; Fraser 1998; Duggan 2003; Bergeron and Puri 2012; Jacobs and Klesse 2013; Dhawan et al. 2015; Peterson 2017; Smith 2020; Gore 2022). As it is, queer political economy scholarship is often framed as an engagement with, or intervention into, Marxist theorizing (see, for instance, Floyd 2009; Chitty 2020; Liu 2015), and yet it is also made possible by the enduring legacy of feminist political economy that has taken questions of economic and sexual (in)justice as a major point of departure. This does not mean, however, that queer political economy simply imitates or appropriates feminist insights since it makes clear and substantial contributions of its own. If feminist political economists have provided much of the intellectual and political scaffolding necessary to understand economic and sexual relations as inextricably enmeshed, then one of the main things that queer theory offers in return is to open up sexuality itself as a category of analysis (Danby 2007). As part of this project, queer scholars have sought to analyze and politicize how non-normative sexualities are marked out against the heteronorm across multiple scales of social existence from the micro-practices of everyday life to the macro-processes of global political economy (Chambers and Carver 2008; Lind 2010a), as we discuss in the following sections.

HETERO-/HOMONORMATIVITY AND THE STATE

Given its key role in governing sexuality, it is perhaps understandable that much of the queer and feminist political economy literature has focused on the links between capitalism, heteronormativity, and the state. According to Bergeron and Puri (2012), political economists must look beyond relations and modes of sexual labor within the household in order to uncover the state-level mechanisms through which hegemonic configurations of gender and sexuality are constructed. V. Spike Peterson's (2014, 2020, 2021) scholarship has made a crucial contribution in this regard by delineating how the state has historically relied on and reproduced intersecting structural inequalities, including along the lines of sexuality. Amongst other things, Peterson shows how the gender binary was constructed and institutionalized through early state-making practices: "making states makes sex," as she puts it (2014, 390). Binary sex and gender (i.e., male-female sex and masculine-female gender identities) were codified through laws governing marriage, tax, inheritance, and property. This binary was further reinforced through the constitution of differently gendered socioeconomic spheres (Peterson 2014, 401). This analysis not only reveals how seemingly "natural" configurations of sex and gender are constituted through state practices, laws, and institutions; it also underscores how sexual hierarchies are bound up with economic, racial, and geopolitical ones. As Peterson (2021, 298) argues, "in our state-based system, 'economic' inequalities are never simply that but are necessarily implicated in and co-constituted by sexualized, racialized, and geopolitically differentiated inequalities and their effects."

This corpus of scholarship on the state overlaps with work in queer International Relations that looks at the hetero- and homo-normative logics of statecraft, sovereignty, and inter-state power (Weber 2016; Agathangelou 2013; Picq and Thiel 2015). Heteronormativity is, according to Berlant and Warner's (1998, 548) influential definition, "the institutions,

structures of understanding, and practical orientations that make heterosexuality seem not only coherent—that is, organized as a sexuality—but also privileged.” Heteronormativity therefore works across multiple sites and scales: through the family/household, the state, structures of global governance, and “sexualized orders of international relation” (Weber 2016, 6). Historically, heteronormative logics have served to make culturally-specific divisions of labor and household/family arrangements—namely married, heterosexual, and monogamous couples living with children—appear “natural” and “universal” (Sears 2017, 173). Conversely, “homosexual” and other queer subjectivities have been constituted as “deviant” and “unruly” under capitalism (in part because they threaten the naturalized sexual division of labor and heteropatriarchal family/household).

Evidently, the boundaries of “normal” and “acceptable” sexual/intimate relations have shifted over time. In some contexts (largely but not exclusively in the Global North), they have expanded to include unmarried couples, same-sex couples, single-parent families, queer families, and some forms of trans embodiment (Sears 2017). This reveals how changes in the social relations of (re)production reshape and redefine sexual normativities (see also Valocchi 2017). Thus, while feminist scholars have shown how the organization of life-making activities under capitalism relies on gendered (and racialized) forms of sexual labor, scholars of sexuality and capitalism have revealed how these same relations determine queer identities and forms of oppression (and how this is subject to historical change) (Hennessy 2000; D’Emilio 1993; Drucker 2015).

In light of these shifts, neoliberalism is characterized by what Lisa Duggan calls a “new homonormativity,” in which certain queer subjects have been assimilated into the contemporary nation-state, for example, through the extension of marriage laws and adoption rights. This assimilationist politics has co-opted and de-mobilized mainstream gay and lesbian organizations and created queer populations that are “anchored in consumption and domesticity” (Duggan 2003, 50). Homonormative discourses ultimately serve neoliberal agendas by articulating queer politics in terms of the fight for formal, legal rights rather than for economic and social justice. In this sense, the equal marriage agenda in contexts such as the US reflects the interests and priorities of the wealthy, gay elite and diverts both attention and resources away from struggles against poverty, inequality, and material oppression (see also Warner 2000; Ferguson 2004; Puar 2007; Eng 2010).

HETERO-/HOMONORMATIVITY AND GLOBAL GOVERNANCE

The state is undoubtedly a key nexus of power relations (and site of struggle) for queer sexual politics. However, in the era of globalization, there exists a range of supra-state formations and actors that critically shape sexual norms and power relations. These formations call for an understanding of the “layers of institutions that are involved in defining and regulating our intimate lives” (Lind 2009, 35), including international financial institutions (IFIs), development agencies, and non-governmental organizations (NGOs).

Responding to this, scholars have sought to illuminate the heteronormative foundations of global development policy and practice (Bedford 2005, 2009 Lind 2009; Jolly 2011). From

population control policies to contemporary initiatives to tackle poverty, gender inequality, and HIV, social and economic development processes have had a transformative impact on intimate relations across the Global South. One key issue here is global development policy's enduring attachment to and investment in heteronormative household models (Bergeron 2009; Jolly 2011). Such policies not only ignore queer people and other non-normative household/family arrangements, but actively work to (re)produce structures of heteronormativity, including the (Western) ideal of the nuclear family. Examples include poverty reduction programs that seek to strengthen the heterosexual family and/or "empower" women and, in so doing, rely on unpaid social reproductive labor (Bedford 2009) and "re-traditionalized" gender roles and responsibilities (Molyneux 2006). Indeed, as Peter Drucker notes, "Official development policy . . . usually sees the heterosexual, nuclear family as a useful contributor (and beneficiary of) market-based economic growth" (Drucker 2009, 828).

Elsewhere, scholars have shown how development interventions on HIV have reshaped local queer politics, identities, and forms of resistance (Gosine 2013), including by engendering processes of "NGO-ization" (Gore 2022). This links to a broader critique of the take-up of LGBT rights agendas within IFIs such as the World Bank, which operate as a form of "global homocapitalism" (Rao 2015, 38). This analysis suggests that IFIs are advocating LGBT rights in an instrumental fashion, in order to explain queer oppression as a result of "culture" (i.e., rather than political economy) and to obscure their own role in producing the material conditions and power relations that systematically exploit and dispossess working class (queer) people in the Global South. Viewed through this lens, "LGBT rights" have been folded into a particular neoliberal, de-politicized, and homonormative vision of economic growth and development, which is compatible with the agendas and needs of global capital.

EVERYDAY POLITICAL ECONOMIES OF QUEER LIVES AND RESISTANCE

Hetero- and homo-normativity are useful organizing analytics for the study of queer sexualities within political economy, since they shed light into the *material* bases of sexual oppression. As we have seen, this includes the organization of social reproduction, state laws and practices pertaining to marriage, citizenship, and inheritance laws, and global development policies and practices. Through this analysis, we have considered how state and supra-state modes of governance work to promote and perpetuate structures of hetero- (and in some contexts homo-)normativity and thus to engender particular forms of sexualized and gendered injustice. However, in order to avoid the same preoccupation with the "upper circuits" (Agathangelou 2005, 7) of the economy characteristic of some orthodox IPE, queer approaches must also attend to the *everyday* political economies of queer lives and resistance (Gore 2022). This is consistent with a broader turn towards the "everyday" in (feminist) IPE, which seeks to challenge top-down approaches centered on "states and markets" and to uncover how the everyday practices of non-elite actors both reflect and resist patterns of global economic transformation (Elias and Roberts 2016). From a queer perspective, this means that it is insufficient to acknowledge how heteronormativity is reproduced through different modes of governance and who is *excluded* from this, that is, queer and trans people. We must

also study the lived experiences of marginalized queer and trans people themselves, their quotidian practices, and, importantly, the ways in which they are resisting the exploitative and oppressive effects of global capitalism.

Nat Raha (2021) provides a useful insight into “everyday” queer resistance in her analysis of the radical politics and organizing strategies of Street Transvestite Action Revolutionaries (STAR). This group was a “radical Third World and Black Gay/Trans liberation group” set up by Marsha P. Johnson and Sylvia Rivera in New York in the 1970s. Through activities focused on housing and education programs, the provisioning of food, clothing, and other types of material support, STAR carved out spaces of resistance and solidarity, primarily for Black and Latinx street queens (Raha 2021, 103). Not only were these activists and the communities they served targeted by oppressive state laws (and multiple and intersecting forms of economic, racial, and sexual injustice), but they were largely excluded from the white, middle-class politics of mainstream gay activism at the time. As this brief example demonstrates, scholars of IPE have much to learn from marginalized queer and trans people’s everyday political practices, since they reveal how different axes of oppression work together and are rooted in political economy, *and* how these dynamics might be subverted and resisted (in this instance, through alternative forms of social reproduction).

While a turn to the “everyday” in queer IPE is potentially valuable, there remains a paucity of scholarship examining political economies of queer oppression in the Global South (for exceptions see, e.g., Rao 2020; Kumar 2021; Gore 2022). Indeed, despite its international orientation, much of the political economy literature on sexuality has tended to focus, empirically at least, on Europe and North America. Yet, as Rahul Rao shows, it is important to understand the relationship between sexuality and capitalism in its “globality” (Rao 2015, 48), including by examining how the “afterlives” of colonialism shape contemporary formations of queerphobia (Rao 2020). Put simply, anti-queer violence and discrimination has colonial roots; from the imposition of Britain’s sodomy laws across its colonies to the brutalities enacted on Two-Spirit people through settler-colonialism (Driskill 2004). Although the genealogy of these laws and practices varies according to geographic context, they laid important foundations for contemporary legal, economic, and cultural forms of queer oppression.

The entanglement of global queer politics in the afterlives of colonialism is further exemplified by what Jasbir Puar (2013) calls “homonationalism.” This refers to the processes through which LGBTQ rights have become “a barometer by which the right to and capacity for national sovereignty is evaluated” (Puar 2013, 336). In other words, nation-states are deemed to be either “gay-friendly” or “homophobic,” a marker that serves as a new dividing line between the “civilized” and the “savage” worlds (Puar 2013, 337). The discursive construct of the “gay-friendly” West obscures the role of European colonialism in instituting the legal and political mechanisms of queer oppression in formerly colonized countries. It also disguises how processes of neoliberal economic transformation—structural adjustment, indebtedness, austerity—create *material* relations of insecurity for queer people across the Global South, as well as the *cultural* conditions in which queerphobia can take root. In Uganda, for example, neoliberalism has facilitated the rise of Pentecostal Christianity, which is a key vehicle for conservative discourses on sexuality (Rao 2020). These discourses do not just target queers, but also include other non-normative gendered and sexualized subjects. As Rao (2020, 163) notes: “The precarity of everyday life under neoliberalism has exacerbated popular anxieties . . . Moral panics thrive in the fertile soil of these anxieties,

fastening on a range of marginal figures including queers, sex workers, ‘witches,’ women who wear short skirts, and others who appear to disrupt normative kinship.”

BEYOND AN “ADD QUEER AND STIR” APPROACH

So far in this chapter we have explored how—given the habitual erasure of non-normative sexualities in IPE—it is crucial for queer IPE to attend to the material consequences of global capitalism on queer lives. This means treating sexual justice as a question of economic consequences (or “outcomes”)—not least in terms of the unequal distribution of power, wealth, and resources—as well as shining a light on the everyday political economies of queer lives and resistance. Queer IPE thus can and should advance an “ethics of recognition” (Elliot 2010, 74) precisely in order to struggle against what Nancy Fraser terms “injustices of redistribution” (Fraser 1995, 85).³ We should immediately add, however, that in highlighting the need for IPE to do more to center queer experiences and subjectivities, we are not calling for IPE simply to “add queer and stir” to global capitalism (Smith 2016, 231). This approach would treat queer as a variable to be added-on to pre-existing frameworks, that is, as an empirical rather than analytical category to deploy V. Spike Peterson’s helpful distinction (Peterson 2003; see also Smith 2015; Richter-Montpetit and Weber 2017). As discussed, one of queer theory’s major contributions has been to question the stability and coherence of sexuality—for example, constructions of homosexuality as “a” discrete sexuality to be defined and marked out against heterosexuality as “the” natural, normal, and privileged sexuality (Sedgwick 1993; Berlant and Warner 1995; Danby 2007; Browne and Nash 2010; Weber 2016). In other words, to understand queer as an analytical category is to treat queer not just as an adjective (“being” queer in the global political economy) but also as a verb (“doing” queer to global political economy) by critically analyzing how sexual injustice is not simply a consequence of economic injustice but is also constitutive of global capitalism itself.

No doubt, conceptualizing sexual orientation and gender identity as a variable (or axis of “difference”) may be a useful starting point, particularly since queer, trans, and other minoritized gendered and sexualized groups continue to be disproportionately impacted by political economic phenomena: recession, austerity, global crisis. However, queering IPE means going beyond this to recognize how sexual injustices are (re)productive of the global capitalist economy (i.e., *contra* a liberal account in which sexual inequality is seen as an effect or outcome of—largely cultural—forms of exclusion). As we have seen, one key element of this involves analyzing how structures and practices of hetero- and homonormativity work to undergird and sustain the global economic order. This reveals how sexual injustice is bound up in and (re)produced through the organization of social reproduction, historical and contemporary state laws and practices, patterns of neoliberal economic transformation and structures of global governance, including social and economic development processes, and everyday forms of violence and dispossession.

In this way, queering IPE recognizes that sexual injustices have material bases and drivers—that is, they are not “merely cultural” (Butler 1997, 265)—and cannot be remedied through liberal policy and market-based approaches focused on inclusion and representation. For poor and working-class LGBTQ+ people, Black, Indigenous and LGBTQ+ people of color, disabled LGBTQ+ people, LGBTQ+ people living in parts of the Global South, and

LGBTQ+ people with insecure immigration/citizenship status, sexuality is not a discrete category or realm of experience. Rather, it is co-constitutive of broader economic, racial, gendered, embodied, and geopolitical hierarchies. Put more simply, “economic” exploitation and “social” oppression are systematically related under capitalism (Davis 1983). This means that sexuality is articulated through and in relationship to class relations and other axes of oppression rather than being distinct from or in tension with them, even if they are so often depicted as such.

Indeed, what this also highlights is how queering IPE is at once a reconstructive and deconstructive project, for it means investigating *both* how sexuality and capitalism are entwined *and* how the uncoupling of sexuality from capitalism is itself constitutive of unequal power relations—not least the gendered division of labor as a fundamentally “racialized and sexualized division of labour” (Park and Wald 1998, 614; Rosen Velásquez 2017, 323). Penny Griffin, for instance, observes that the expansion of Western capitalist imperialism has relied on the elision of its gendered, sexualized, and racialized underpinnings so as to naturalize and normalize what is, at heart, a “highly masculinized and ethnocentric model of human activity” (Griffin 2013, 9). Likewise, Lisa Duggan urges critical scholars to reject the “rhetorical separation of economic/class politics from identity/cultural politics,” which “disguises the upwardly redistributing goals of neoliberalism” whilst also hampering queer politics and activism (Duggan 2003, xiv, xxi). Queering IPE, then, entails asking questions not only about the nature and dynamics of sexual injustice together with the modes of resistance and struggle that arise from such injustice, but also what is (and is not) counted as an “IPE” question in the first place.

CONCLUSION

While the study of sexuality has often (if by no means always) been overlooked in IPE, we have sought in this chapter to shine a light on the invaluable work of feminist and queer scholars who have placed questions of sexual justice center-stage in the analysis of global capitalism. Drawing from and building on these contributions, we have argued that sexual injustice is a major effect (or “outcome”) of global inequalities in wealth, power, and resources; that, more than this, the global economy is itself structured by sexual injustice; and that sexuality, in turn, is globalized in character, functioning as it does along multiple levels and forms of governance. In so doing, we have hoped to show above all that IPE as a field can only benefit from a closer engagement with queer and feminist scholarship on the political economy of sexuality—for, as Svati P. Shah writes, to expand our frameworks is also to “expand the political and intellectual spaces for discerning power, states, and utopias in ways that remain unimagined” (Shah 2009, 7).

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in the Global Economy: Towards a Theoretical Framework for Political Economy." *New Political Economy* 27 (2): 296–311.

NOTES

1. "Conversion therapy" is a highly controversial (and widely discredited) intervention that seeks to change someone's sexual orientation or gender identity. Lesbian, Gay, Bisexual, Trans, and Queer (LGBTQ+) rights activists have long called for its prohibition and it is recognized as an "unethical" and "harmful" practice by the British Psychological Society (BPS 2022). While the proposed UK ban would cover sexual orientation i.e., gay, lesbian, and bisexual people, it would exclude gender identity i.e., trans people.
2. For a discussion see Smith and Lee (2015).
3. On which see the famous debate between Nancy Fraser and Judith Butler about Fraser's characterization of lesbian and gay struggles as a question of recognitional rather than redistributive justice (Fraser 1997; Butler 1997; Fraser 1998).

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PART VIII

ASSETS AND
RESOURCES

CHAPTER 45

OIL AND GAS

ANDREAS GOLDTHAU AND NICK SITTER

INTRODUCTION: STATES, MARKETS, AND POWER

IF International Political Economy (IPE) is about the inextricable link between state and market, then oil and gas epitomize this link. On the one hand, both are private goods traded on international markets. Oil is the world's most actively traded commodity, and gas is going increasingly global thanks to Liquefied Natural Gas (LNG). On the other hand, energy security is as important as ever. Reliable supply (for importers) or demand (for exporters) at acceptable prices is a public good, and as such it merits public intervention. Energy therefore is a "strategic good" (Goldthau and Sitter 2015a): it is not just a public good, but a public good that is vital to the health of the economy or even the very survival of the state. The premise of the 2015 TV series *Occupied*—that Norway's closing down its fossil fuel industry causes the European Union (EU) to call on Russia to occupy Norway and restore its oil and gas exports—might be far-fetched, not the least in light of Russia's aggressive war against Ukraine and the EU's ensuing efforts to reduce dependency on its dominant energy supplier. But it drove home the point that energy is widely seen as far more than just another traded commodity. It is this location at the intersection of international economic relations and global (security) politics which puts oil and gas at the heart of what IPE research is all about.¹

Much like International Political Economy as a discipline more generally, the IPE of energy has become a very problem-centered area of research. Key questions center on oil market functioning and transparency, representation in global organizations such as the International Energy Agency, and the effectiveness of regional frameworks governing gas transit, as well as new questions about the dynamics and effects of transitions to low-carbon economies. Yet there is a deep divide between the two dominant schools of thought in IPE energy research: the liberal school, which tends to focus on international energy institutions, companies, and trade, and which, by and large, subscribes to the notion of markets as an efficient and effective organizing principle; and the realist school, which subscribes to a state-centered and, in large parts, economic nationalist IPE, and which extends into geopolitics. This should not come as a surprise, given that energy commodities have the characteristics of both public and private goods. But there is little interaction between the two schools, and indeed a lack of scholarly debate on how liberal and realist thinking may help us understand

the dynamics shaping the state–market relations in global energy. Much of the focus in what follows is therefore on these two dominant schools, but also how to advance academic investigation beyond those schools.

Clearly, energy has played its part in the broader IPE debates between the classical realist, liberal, and Marxist approaches and their structuralist, constructivist, postmodern, feminist, and other critical theory rivals. A highly interesting strand of research particularly stems from discursive approaches to energy IPE (Kuzemko 2014a, 2014b). But the role energy has played in these IPE debates has, by and large, been a minor one. In fact, although oil and gas are analyzed as part of broader IPE questions, the discipline has paid rather little attention to energy as a genuine object of research. This is particularly surprising as, arguably, the 1973 oil crisis and its ripple effects on the global order were instrumental in shaping IPE as a discipline (Hancock and Vivoda 2014). The oil shock drove the point home that states can leverage on their dominant (economic) position in crucial commodity markets and turn this position into political power. As aptly summarized by Stoddard (2013), “the production, transportation and sale of energy commodities, both subject to trends in transnational market and of enormous strategic value to political actors, lie at the intersection of the transnational capitalist economic and the interstate systems” (449).

Admittedly, some of the field’s giants did—early on—put energy in the context of power and interdependence (Keohane 1984). And yet, compared to key IPE themes such as world trade, financial markets, or multinational corporations, energy features only at the margins. Few IPE textbooks have a chapter on energy, and besides -Balaam and Veseth 2009- the present handbook indeed is a notable and welcome exception to the general rule. In most of the existing textbooks energy merely merits a brief mention, if any (Smith et al. 2013; Stillwell 2006; Ravenhill 2014; Oatley 2010). OPEC sometimes features as an example of a successful producer cartel, or even the exception that proves (or refutes) the structuralist rule about how the global core dominated the periphery. In the context of the political economy of development, energy is discussed as a type or resource that promotes or impedes economic advancement. But such use of energy as a case or example often says more about energy than about the theoretical debates in IPE. For example, the postmodernist question about the importance of how international events are understood can shed considerable light on the very different ways in which the role of oil or gas plays in conflicts, from the First World War to the First Gulf War and the Russian annexation of Crimea, is interpreted. But even a brief examination of how the actors in these kinds of conflicts interpret the world takes us straight back to the old realism vs liberalism debate: it became abundantly clear in 2014 that Russia sees oil and gas as a strategic good—and even a tool to be deployed in conflict—whereas the EU tends to approach oil and gas as a commodity to be traded.

To be sure, energy as an IPE theme found its readership and was recognized in a rapidly growing number of devoted textbooks (Garavini and Claes 2020; Goldthau et al. 2018; Van de Graaf et al. 2016); yet it remained outside the IPE mainstream. It is only in the context of the emerging strand of research on global energy governance and global energy policy that oil and gas have come to feature prominently again, almost as “IPE in disguise.” A case in point is the handbook by Kuzemko et al. (2012), whose introductory chapter makes a case for mainstreaming energy “back” into IPE (Keating et al. 2012). In other words, scholars had to “take a detour” and speak to the global governance audience in order to eventually “make IPE think energy” again. With that in mind, this chapter reviews the most pertinent scholarship in energy. It does so with a view to three questions: (1) What are the main assumptions,

research questions, and debates in the liberal and realist schools of energy IPE? (2) Given that both schools are problem-driven, might new developments that blur the line between trade and security push the two sets of research agendas closer? And (3) how may energy research contribute to IPE as a discipline?

ENERGY AS A DIFFICULT COMMODITY: GLOBAL GOVERNANCE

A dominant branch of the academic literature on oil and gas conceptualizes energy as a transnational phenomenon, acknowledging the complex interaction of state and market players in energy production, trade, and distribution. It is firmly rooted in the “liberal camp” of IPE and by and large rejects realist accounts of energy politics (which we shall return to in the next section). In this strand of scholarship, the analytical focus is on the *governance* of global energy relations (Goldthau and Witte 2009). The novelty of adopting a global governance angle, as Van de Graaf and Colgan (2016) argue, lies in bringing to the fore “a critical set of actors and structures that geopolitics neglects, most notably markets and institutions” (2). The body of literature that has emerged around the tag of “global energy governance” explores a highly diverse set of questions. The “classics” include: “Who governs global energy?” (Florini and Sovacool 2009); “What are the rules of the game according to which energy actors play?” (Goldthau and Witte 2010); and “What institutional structures are needed to address pressing energy challenges?” (Lesage et al. 2010). These questions also define the broader analytical realm of global governance research on oil and gas, which comprises a focus on institutions, the interaction of state and non-state actors in global energy affairs, but also on normative aspects. This includes questions about international institutions and organizations; how to manage market failures for public and private goods; the dynamics of regulation and the nature of regulatory power; and the challenges of transition to a low carbon economy.

First, in terms of *international institutions and organizations*, one set of writings focuses on formal organizations, informal institutions, and patterns of trade in global energy, with a view to mapping the landscape, delineating patterns, and identifying gaps. Prominent objects of analysis include OPEC, the oil cartel, and its ability to manipulate markets thus projecting global power (Colgan 2014; Goldthau and Witte 2011; Morse and Jaffe 2005); the International Energy Agency (IEA), the rich world’s consumer club, and its role and steering capacity in oil market governance (Heubaum and Biermann 2015; Florini 2011; Van de Graaf and Lesage 2009); and attempts to bridge the producer-consumer divide in order to facilitate global energy cooperation, for instance in the shape of the International Energy Forum (Harks 2010; Wilson 2015; Mitchell 2005; Fattouh and van der Linde 2011). The “Gs” feature in their various shapes (Andrews-Speed and Shi 2015; Lesage et al. 2009; Downie 2015a) as do the UN (Karlsson-Vinkhuyzen 2010) or the BRICS as an emerging powers club (Downie 2015b). Gaps are identified both with regard to the existing energy regime and its ability to effectively address pressing problems such as oil price volatility, supply security, or climate change (Florini and Sovacool 2011), but also systemic qualities and the global energy system’s overall resilience (Cherp et al. 2011).

Exploring the legal frameworks for global oil and gas trade further, some scholars turned to the role of multilateral agreements such as the Energy Charter Treaty (Tienhaara and Downie 2018; Selivanova 2012), or the World Trade Organization (WTO) and its role in governing fossil fuel commodities trade (Cottier et al. 2009; Meyer 2016). Numerous research projects have also analyzed regional arrangements such as those of the EU, including its agreements for trade in fossil fuels and related legal alignment (Chaban and Knodt 2015; Dimitrova and Dragneva 2009; Padgett 2011). An important strand of works also centers on the interaction between legal institutions and private contractual structures, notably in long-term gas trade (Talus 2011). This research extends to the role of private actors in shaping energy market governance (Goldthau and Hughes 2020; Blas and Farchy 2021) and state-market “hybrids,” such as in the governance of energy finance (Newell 2011).

In the context of an increasingly fragmented world of oil and gas, the pace and dynamics of institutional change merit attention. Building on the works of Raustiala and Victor (2004) as well as Keohane and Victor (2011), Colgan, Keohane, and Graaf (2012) conceptualize energy as a “regime complex” highlighting the importance of both path dependency and disruptive dynamics in energy governance. Another pertinent example is the firm-centered explanation for patterns of change offered by Hughes (2014), who shows how energy companies responded to deep structural changes in the oil market and drove the latter from a vertically integrated governance structure to one characterized by vertical segmentation. Bringing in the notion of energy paradigms, Goldthau (2012) points to a shift from market-centered to more state-centered and hybrid governance models, particularly in the context of the rise of Asian economic powerhouses and their increasing importance in global energy consumption. This shift, as De Graaff (2012) argues, also creates more hybrid forms of cooperation as well as new alliance in global energy. All these works share an implicit or explicit normative outlook centered on a rule-based international order, which is generated, maintained, or challenged by governments, sub-state actors such as regulators, private actors, or even policy networks.

Second, an important strand of liberal IPE research puts the quest for a rule-based international energy order in the context of the rise of the Western “regulatory state” (Majone 1997; Moran 2002). By many accounts the regulatory state epitomizes the liberal way of thinking about the public-private nexus, for its preference for setting framework rules (rather than direct intervention), nudging and incentivizing (rather than coercing), and the reliance on markets rather than state ownership. The regulatory state is clearly embedded in the liberal paradigm and subscribes to the idea that unless markets fail, state activities should be restricted to ordering them, whether by way of direct regulatory intervention or more indirect forms of experimentalist governance (Eberlein 2010; Sabel and Zeitlin 2012).

This focus on regulation as a policy tool does not necessarily imply that the role of the state as a regulator is overly passive. Major Western economies such as the US and particularly the EU have sought to project their own regulatory model onto the international stage, and shape international economic governance. This has given rise to a debate on the EU as a “regulatory power” or “global regulator” (Bradford 2016; Young 2015). (Although largely unnoticed, this ties well into Luttwak’s (1990) argument that states aim at shaping global regulatory frameworks for geoeconomic purposes, an argument we will return to in the next section.) Importantly, the international expansion of regulatory regimes in energy is not necessarily driven by governments. Instead, it may be the result of the selective empowerment of an executive authority such as the EU Commission (Goldthau and Sitter 2015b, 2015a)

and a supranational polity combining nodality, treasury, and connectivity to “catalyze” its objectives (Prontera 2019). With this, the regulatory state, which started off as an organizational principle with a bureaucracy attached to it, acquires an external dimension and starts exerting spillovers onto the international stage.

Third, taking oil and gas research firmly into neoclassical economics territory, some scholars have started to conceptualize global energy in terms of *public goods* or *bads* (Karlsson-Vinkhuyzen et al. 2012; Goldthau 2011). This framework embraces all actors and levels of analysis of the global energy governance literature. What changes, however, is the focus of analysis. This shifts from explaining the existence of certain governance patterns or the occurrence of specific actor sets, to assessing the nature and dynamics of global risks and collective action problems. Moreover, approaching energy as a matter of global public policy makes a normative case for public intervention—because of market failure. This not only “brings the state back in”—admittedly as part of a broader set of relevant actors—but also locates this strand of the literature firmly in the “problem-solving” branch of IPE. In this vein, recent works on the global energy “dilemmas” (Bradshaw 2013) or “challenges” (Kuzemko et al. 2015) firmly embed energy in the context of broader global policy agendas such as fighting poverty, tackling climate change or ensuring energy access, or fostering good (resource) governance. Addressing these challenges, some authors argue, requires nexus thinking and an intellectual openness in IPE research, so as to give proper analytical attention to the relevant actors and scales (Kuzemko et al. 2018).

Fourth and finally, a rapidly growing strand of IPE inquiry explores the implications and analytical imperatives of oil and gas—and fossil fuels more generally—leaving the global energy system by way of a *low-carbon transition*. Contributions laying out new research agendas in IPE point to the imperative of looking at materiality, scale, and the distributional consequences of leaving the fossil world (Kuzemko et al. 2019); the interaction of existing structures in the world economy with dynamics of change (Newell 2019); and the necessity of re-thinking socio-technological interactions jointly with political economy questions (Kern and Markard 2016). Works particularly pertinent to the liberal IPE camp have started to address issues as different as the politics of incumbency of energy system change (Newell and Johnstone 2018), the importance of the local in enabling the global transition (Kuzemko 2019), the effects of global supply chain interdependence on clean energy transitions (Meckling and Hughes 2018), and the enabling or preventing role of emerging economies in energy transitions in the Global South (Power et al. 2016). The importance of these works lies in the fact that they, on the one hand, bring IPE questions to the study of a new phenomenon—the low carbon transition; while, on the other hand, they are open to this very shift also informing IPE thinking and shaping the IPE research agenda.

As this—admittedly selective—review demonstrates, liberal scholarship on the global political economy of oil and gas, even if not explicitly labeled “IPE research,” contributes to academic conversations in global governance, regulatory politics, and global policy. It acknowledges power structures by giving room to a variety of actors and shifting government-firm relations in the “state-market condominium” (Underhill 2000). In contrast to this literature, the second dominant strand of IPE analysis of oil and gas puts the state centerstage and approaches energy as a unique commodity that is of vital importance to the modern state, its prosperity, and its survival. The next section therefore turns to the realist take on energy IPE.

ENERGY AS A TOOL OF POWER: GEOPOLITICS, MERCANTILISM AND THE RISE OF GEOECONOMICS

A second, and in many ways opposing, strand of IPE research is preoccupied with energy and power. It is firmly rooted in the state-centered, realist—or economic nationalist—school of thought, and subordinates energy to broader questions of international security and state survival. At its core, the realist branch of IPE research on energy conceptualizes oil and gas in terms of means or ends of grand strategy (O’Sullivan 2013). With this, energy becomes an instrument to project state power onto the international stage with a view to achieving foreign policy objectives. Alternatively, it is access to energy resources which guides foreign policy thinking and strategy. This approach to the subject informed the best-known book on the history of oil—Daniel Yergin’s *The Prize* (1991).

Not surprisingly this strand of the literature is mainly associated with a geopolitical reading of international energy politics (Klare 2009; Noreng 2006; Barnes and Jaffe 2006). Albeit academic in terms of method, many of the hard-nosed security analyses on international energy affairs originate from think tanks and the foreign policy community. While some works adopt a global perspective on the entanglement of energy and foreign policy (Kalicki and Goldwyn 2005), the central themes have long been linked to the national security consequences of oil imports (Deutch et al. 2006; Crane et al. 2009), the ability to project global power, for instance in the context of the US shale revolution (Blackwill and O’Sullivan 2014; Dunn and McClelland 2013; Kim and Blank 2014), or the extent to which energy can be used as “weapons” (Paust and Blaustein 1974; Hughes and Long 2015). In this context, terms such as “energy superpower” (Rutland 2008) or “energy empire” (Hill 2004) rose to prominence in order to describe great power status derived from energy resources (which, in the case of Russia, was strongly contested (Goldthau 2008)). While not all of these works are explicit about their ontological underpinnings, they firmly subscribe to the key realist notions such as national interests, an international struggle for power, an anarchic global stage, and to energy as an element of national capability to excel in this struggle. The central problems that have driven this research agenda include the role of fossil fuels as a source of conflict, both inter- and intranational; energy as a foreign policy tool, including the role of both private and state-owned companies; and the importance of trade regimes and regulatory rules in a world where geoeconomics is replacing geopolitics, and regulatory power might be as important as hard military power in some regions.

First, then, the classical geopolitical question about oil and gas is whether the *quest for control of natural resources causes conflict*. Yergin’s above-cited history of oil puts this question centerstage, and suggest that states’ struggle for the ultimate prize has shaped much of the history of oil and gas and played a significant part in many of the conflicts of the last century. Indeed, British, French, Russian, and US security-driven concerns to dominate oil markets, and the strategic role their (private) oil companies played in the process, plays a considerable role in histories of great power struggles in the Middle East, such as those penned by James Barr (2018).

Taking research into the territory of conflict studies, some important works have explored the causal links between oil and war. Some straightforward realist accounts posit that the finite nature of energy resources will lead to scarcity and, as a consequence, to interstate

conflict and a “race to resources” among major powers (Klare 2001)—the classic “energy as an end” argument. Some scholars, such as Meierding (2016), reject the notion of “oil wars.” Others offer a more granulated analysis of how oil may translate into conflict. Key studies here comprise Colgan’s investigation into how resource wealth leads to a greater inclination of petrostates to engage in interstate conflict (Colgan 2013a), and the pathways characterizing that process (Colgan 2013b). Likewise, at the level of intrastate conflict, much of the literature on the “resource curse” and the structural causes of civil wars and insurgencies lies in this crossroads between (domestic) political economy and realist IPE (Sambanis 2004; Kissane and Sitter 2013). This literature developed in the quest for structural explanations of conflict that include a wide range of variables such as weak states, nationalist separatism, and conflict in neighboring states (Fearon and Laitin 2003), but developed into a natural resources-focused literature that identifies the rational pursuit of greed as the key cause of armed conflict. The main contribution here has been a series of hypotheses about how “lootable” resources—of which oil is a prime example—provide both a direct cause of armed conflict and a crucial resource to sustain prolonged conflict once it has started (Ross 2012). Some, like Paul Collier, go as far as to argue that the presence of lootable resources makes conflict inevitable sooner or later (Collier and Hoeffler 2004; Collier et al. 2004).

This type of analysis can be extended beyond fossil fuels, to the geopolitical consequences of the energy transition. As and when oil and gas come to play a less prominent role in the world’s energy mix, this will have a profound effect on the wealth and power of some. By 2000, many oil companies were concerned about stranded assets and unburnable carbon. In the near future, entire states may face the prospect of becoming “stranded nations” (Manley et al. 2017; Van de Graaf and Bradshaw 2018). Particularly rentier states that combine a high dependency on revenues from fossil exports with fiscal vulnerability (a high fiscal break-even oil price, high budget deficits, low currency reserves) and low production to reserves ratio might face incentives to maximize output while there is still a market for their product (Goldthau and Westphal 2019). An in previous energy transitions, control of bottlenecks (this time, perhaps patents or control of rare earth extraction) will cause redistribution of rents. The age of abundance may make market power shift from producers to importers of oil (Scholten 2018). New trade and tariff regimes, or protectionist measures, may emerge in the context of the low-carbon transition, with profound consequences for geopolitics (Bazilian et al. 2020). Clean energy trade may give rise to new international tensions (Ghosh 2016) and uneven transition patterns may come with potentially severe impacts on global equity and conflict (Eicke and Goldthau 2021).

Second, another strand of literature focuses on the role fossil fuels can play as a resource or policy tool in geopolitical struggles. Exploring in detail the way energy intersects with foreign policy, one strand of literature on the challenge that a more assertive Russia and rising China present to the West has focused on Russia’s use of “coercive” and “well-oiled diplomacy” toward the Eurasian region (Stulberg 2008; Maness and Valeriano 2015), Beijing’s “oil diplomacy” in Africa and elsewhere (Jaffe and Lewis 2002; Taylor 2006), or how “pipeline diplomacy” unfolded in the Caspian (Bahgat 2003; Stulberg 2012). Some recent works started to explicitly separate energy diplomacy from strictly security-focused accounts by way of exploring the multi-stakeholder approach of the EU (Chaban and Knodt 2015). The debate on energy diplomacy extends to an academic conversation on mercantilist accounts of national energy security policies, notably in the context of China’s Belt and Road Initiative (Flinta and Zhub 2019) but also when it comes to the “windfall” the surge in domestic oil

and gas output represents for US grand strategy (O'Sullivan 2017). In this context, the political role that oil and gas companies—whether private corporations or state-owned National Oil Companies (NOCs)—play in energy security matters has attracted considerable interest. Whilst Mahdavi (2020) shows that political rulers primarily nationalize to redistribute revenues from private firms and to financially sustain political power, successive waves of energy sector nationalization in key oil producer nations, coupled with the rise of China and its mercantilist energy strategy, secured NOCs a substantial role the production of oil. This inspired inquiries into the security implications of NOCs (Chen and Jaffe 2007), the hegemonic effects on countries where consumer NOCs (or other state-owned companies) are active (Campbell 2008), and their role in projecting power as part of national grand strategy (Bilgin 2011). Some studies attempt to open the black box and investigate the international functioning—or, in IPE terms, the domestic political economy—of NOCs. A case in point is Victor, Hults, and Thurber's (2011) seminal study, which sheds light on why performance and strategy vary significantly across NOCs, and how this loops back to the politics of their oil-rich host countries. Extending the analysis to private energy companies and their role in *realpolitik*, Abdelal (2013) acknowledges the role firms can play in international politics—a notion that is typically discarded by realist scholarship. His analysis, which makes an attempt to bridge the realist-liberal divide in IPE debates, explains the persisting Russian influence in Europe's energy sector by linking private commercial interest back to political power.

Third, the International Relations (IR) literature on geopolitics has gone through a turn toward *geoeconomics*, which can be summed up as the “use of various economic instruments in the pursuit of strategic goals” (Scholvin and Wigell 2018, 82) or “war by other means” (Blackwill and Harris 2016). The renewed interest in *geoeconomics* reflects a return of mercantilism and the politicization of trade, the increasing relative utility of economic hard power (such as sanctions) compared to military hard power, and the idea that interdependence and “connectivity” may bring about more vulnerability rather than more cooperation (Leonard 2016; Farrell and Newman 2019). These themes have of course long been a central theme in IPE, but the thrust of the *geoeconomics* literature is that the quest for security through control of territory has been replaced by, or at least complemented with, a quest for security based on control of “gated markets,” trade infrastructure, and flows. China's Belt and Road Initiative is the most prominent case in point, dubbed by some as “the most tangible manifestation yet of China's seemingly unstoppable rise to regional primacy” (Beeson 2018, 252). While China pursues a *geoeconomics* strategy of fragmenting global energy markets and establishing hegemony over parts of the market, the US has lost a strong part of its economic incentive to maintain its presence in the Middle East and Africa, thanks to abundant domestic supply. To be sure, the latter does not de-link the US from global oil and gas markets; it does, however, provide the basis for the US to pursue a more selective, interest-based foreign policy strategy underpinned by energy resources, which in the Trump years culminated in the concept of “energy dominance.” Consequently, the “unipolar moment” of the 1990s might be replaced not so much by a global bi-polar contest between the US and China, as a regionalized, fragmented, and multipolar world order—in the world of energy as in global politics.

In this changing world of energy, the heavily energy import-dependent EU faces a very different dilemma from the US. Because it lacks the classical attributes of a state in foreign policy—tanks, troops and treasure, the EU's ability to “entice” third actors to abide by EU

rules rests exclusively on its market might, and its willingness to use it. What Birchfield (2015) aptly terms a “coercion with kid gloves” essentially amounts to the EU’s ability to leverage its \$16 trillion economy in its drive to foster the extraterritorial application of European regulation, as it happened in various sectors, including the aviation industry. The EU’s market power—or soft power, in Joe Nye’s (2004) terms—acquires a “harder edge” when used strategically and selectively targeting individual non-EU actors. A case in point is the EU’s dealings with Gazprom, where a selective use of regulation has led to the demise of Russia-sponsored pipeline projects (Goldthau and Sitter 2015b). This is where regulation becomes part of grand strategy thinking in the realist sense (Smith 2011), as the EU regulatory state risks overstepping and touching upon to mercantilist territory (Andersen et al. 2016). There is some irony in the fact that in classic IPE terms, mercantilism stands for the deliberate use of economic means for political ends, firmly grounded in realist thinking, whereas the EU regulatory state represents the liberal paradigm. The selective use of regulation, however, blurs the traditional distinction as made in the literature, and makes the liberal toolbox available for non-liberal ends. Returning to our earlier treatise of the EU as a regulatory state, for the EU IPE literature this offers an important analytical twist by linking international rule-setting—a liberal notion—back to the concept of power.

ADVANCING IPE RESEARCH AND SCHOLARSHIP ON ENERGY: FOUR QUESTIONS THAT BLUR THE TRADE-SECURITY BOUNDARY

The international political economy of oil and gas changed considerably in the first two decades of the new millennium. Power shifted away from the OECD counties, toward rising powers such as China. Russia assumed a more assertive posture. Energy security now tops the EU’s energy policy agenda, and so does the EU’s transition to a low carbon paradigm. Climate change became a major driver of global policy action. As a corollary, oil firms worry about stranded assets. The surge in domestic fossil fuel production changed the interest and power resources of the US. Oil price volatility, in turn is expected to put a dent into the US shale industry and the national budgets of many oil producing states. In Europe, international LNG supplies and the drive to complete the Single Market for energy (sometimes called the “fifth freedom,” complementing the free movement of goods, services, capital, and labor) introduced a strong element of competition and spot-market dynamics to gas trade. The combination of the rise of China, Russian assertiveness, US energy independence, and the EU’s regulatory power points to an increasingly fragmented energy world. Taken in the context of the theoretical state of the art and the debates in the literature, these changes raise four broad questions to which the answers are not only important for those interested in the IPE of energy, but potentially also to wider IPE debates.

The first of the four big issues in contemporary energy politics that might have implications for broader IPE debates concerns the *nature of power* and the utility of different types of power. The literature on power in IR has long debated the gray areas between hard and soft power, and the related difference between low and high politics. The economic,

regulatory, and political conflicts over (or at least related to) energy between the EU and Russia in the new millennium have demonstrated clearly that the EU and Russia take diametrically opposed approaches to the IPE energy. In the words of Correlje and Van der Linde et al. 2006): this is a contest between the advocates of markets and institutions and the champions of empires and geopolitics. The EU's Energy Union initiative shows that a range of policy tools are available between hard coercive military and economic power and the soft power of arguments, norms, and attraction (Andersen et al. 2016). The study of the IPE of energy in Europe allows scholars to explore under what conditions these various types of power can be effective. Our own work on the EU suggests that the kind of power explored in the geoeconomics literature is conditional in the sense that it works best when targeted at actors that have much to gain by maintain trade and market access, and therefore is more effective when targeted at state-owned national champions (such as Gazprom) than at the governments themselves (Goldthau and Sitter 2020, 2014).

The second broad question is about *international or global governance*, and what kind of regimes might be best suited to tackle new challenges. In fact, as Graaf and Colgan (2016) argue in their review of a decade of global energy governance research, “energy has been overlooked within the large research programme on international regimes” (2). One of the themes in the IPE work on energy is that regional or global governance regimes are necessarily designed according to what kind of public goods problem they seek to deal with. Climate change is by far the most significant of these, but other issues such as energy poverty and the distributional effects of low carbon transitions are also likely to dominate the international policy agenda. This takes IPE deeply into the cross-disciplinary territory where politics and economics meet other fields such as public policy, regulation, and law (Watson 2005; Underhill 2000). Oil and gas present different types of public goods problems—for example the difference between price risk (oil) and the risk of interruption (gas) as the central security of supply risk—and therefore merit different types of regimes. A key difference has emerged between states (or actors) that see oil and gas mainly as a private good with important public goods characteristics (and therefore prone to market failures, which can be corrected), and those that see it as a strategic good. It does not take much imagination to see that energy might not be the only good that is contentious in this way in the twenty-first century. Most notably, any low carbon transition will inevitably generate a new set of public goods problems, and it is likely also to raise new security issues.

Third, developments in the energy industry and the debates on the global role of private and state-owned energy companies in the IPE literature drive home an important point that has been debated in Western Europe since the waves of privatization, liberalization, and “New Public Management” that began in the 1980s: *the blurring of boundaries between the private and the public*. At the heart of this lies the nature of the relationship between political authority and private market actors in global governance. This is of course not unique to the energy industry, but the sheer scope and magnitude of the global oil and gas trade means that considerably more attention has been paid to the patterns, dynamics, and consequences of state ownership and political use of companies in the energy sector than other sectors. As the liberal—or “Washington consensus,” “US hegemonic,” “hyperpower,” or “unipolar”—era of globalization is increasingly contested, and China and Russia promote their own version of illiberal state-controlled capitalism, the study of the relationship between states

and “national champions” and new forms of mercantilism in the energy sector might offer a basis for developing ideas and hypotheses that go beyond the realm of fossil fuels. Indeed, the COVID-19 crisis and the ensuing debate surrounding the reshoring of strategically important industries have demonstrated that global supply chains have become an issue of concern for many states as they deprive governments of control of important goods—regardless of whether these supply chains are dominated by private or state-owned entities.

The fourth and final big set of questions that the study of the IPE of energy points to concerns what *might replace the era of globalization* and Western efforts to establish regimes for global governance. Writing just before 9/11, Kissinger (2001) argued that different analytical lenses were required to analyze different parts of the world (e.g., realism for Russo-Chinese relations, and liberalism for the EU-US relationship). The following two decades have seen a turn from globalism to regionalism, with more complex interaction between the domestic and international policy levels (Keating et al. 2012), or even a fragmentation of world politics, that makes Kissinger’s point even more resonant in 2020. The IPE literature on oil and gas suggests that this process could well continue, and is probably an indication of fragmentation in global governance more generally. In the case of oil and gas this is not so much a matter of failure to establish global rules (though that is important), as of emerging powers rejecting established institutions. China’s “strategic goods” approach to energy and its mercantilist approach to fossil fuels but also critical minerals, is but the clearest example. At the same time as Russia and China are more assertive, US energy independence has increased the world’s only superpower’s foreign policy options considerably. Both the IPE literature on energy and policy debates on either side of the Atlantic point to more fragmented global governance, and as well as the likelihood that security concerns will play a major role in shaping the energy world.

Returning to the central theme of the IPE of energy as a problem-oriented research agenda, it is tempting to conclude that these four broad questions about power, global governance, state capitalism, and global fragmentation are likely to push European, North American, Chinese, and Russian policy debates in very different directions. Whether the academic debates will follow a similar path is more uncertain. Over the last decades, the main dividing line in the energy IPE literature has reflected the policy debates over whether to treat oil and gas as ordinary commodities or strategic goods. The 1990s saw a broader focus on trade, with work on security featuring less prominently in the IPE literature, whereas the re-politicization of energy in US and European policy debates was accompanied by a rapidly expanding literature on the public goods and strategic goods dimensions of oil and gas trade. The liberal and realist schools continue to operate according to what the editors of this handbook issue call “constructive non-engagement”—mutual awareness and engagement that eschews ontological or epistemological debates—but the main division is between liberal and realist IPE approaches rather than schools that can be labeled for continents, cities, or universities. The recent debates in the literature about global governance or the rise of geoeconomics are unlikely to change this. The scholarship on how measures to build and manage international markets and/or manage the security implications of oil and gas trade (and green transitions) are designed, operated, and contested is not only an increasingly important part of the IPE on energy—it also merits attention by scholars interested in broader IPE debates.

NOTE

1. This chapter uses the terms of “energy” and “oil / gas” synonymously. While there exist other energy fuels, including nuclear, coal, and renewables, oil and gas are internationally traded whilst at the same time having a clear strategic character for both importers and exporters. Oil and gas therefore are of most interest to IPE research, which justifies this analytical choice.

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CHAPTER 46

KNOWLEDGE

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WHAT IPE SCHOLARS KNOW ABOUT KNOWLEDGE AND HOW THEY KNOW IT

TAKING stock of knowledge and its production in International Political Economy (IPE), or any field for that matter, is treacherous terrain.¹ As Friedrichs (2009) cautions, “it is much more hazardous to contemplate the way we gain knowledge than to gain such knowledge in the first place.” Such exercise is especially perilous when contemplating knowledge about knowledge. Examining how knowledge is produced, controlled, transmitted, as well as acted upon by individuals and organizations can rapidly submerge analysis into epistemological discussions. Rather than arguing for a certain way of knowing or of resolving difference between epistemologies,² this chapter advances three claims in response to the central contentions animating this handbook.

First, this chapter confirms the existence of a gap in knowledge about knowledge in IPE. Although *several* gaps exist, the most profound divide in knowledge about the generation, control, and dissemination of knowledge is between positivist and post-positivist epistemologies. This chapter confirms the gap noted by other IPE studies between scholarship seeking “generalizable knowledge about robust and objective knowledge” and scholarship “situating and destabilizing knowledge about subjective and contingent knowledge.”³ While each investigates how knowledge is produced, controlled, and disseminated, scholarship in the two “camps” of IPE tend to remain siloed from one another. There tends to be a lack of communication—in the forms of citations and engagement with the findings in each camp—linked to a perceived lack of common ground between what are widely considered as “incommensurable” epistemologies (Higgott and Watson 2008).

In a second instance, this chapter supports the claim of the handbook editors that the gap in research investigating knowledge is not *necessarily bad*. As the following section details, IPE knowledge about knowledge—its generation, control, and dissemination—is quite “deep” in each camp. Nevertheless, the wisdom that IPE “knowledge about knowledge” conveys can be enhanced by better communicating and fostering interactions across this gap. Imparting wisdom from knowledge is an urgent task for IPE given the failures of many

economists and experts to convey practical knowledge for addressing interconnected crises facing the planet.

Third and finally, this chapter outlines three avenues for communicating “knowledge about knowledge” across the positivist-post-positivist epistemological divide. Research foregrounding knowledge governance, historicity, and technology provide a trio of paths for navigating between siloed IPE knowledge about knowledge. The multi-laned avenues provided by these research emphases can enable IPE scholars less to overcome epistemological divides than to better communicate “knowledge about knowledge” across the field, as well as beyond it—to communicate not only amongst scholars but also with policymakers, students, and wider publics.

SILOED KNOWLEDGE ABOUT KNOWLEDGE

IPE scholarship on the production, control, and dissemination of knowledge in the international political economy tends to fall into two “camps.” The perceived irreconcilable epistemological commitments of each underpin the lack of communication, engagement, and interaction across positivist and post-positivist camps. As the next sections emphasize, exceptions and “paths” between these “islands of knowledge” (Odell 2010) do exist. Before outlining these, however, this section first briefly details the debates and insights generated in each “camp,” as well as advancing the argument that there is wisdom lost in the gap between siloed knowledge about knowledge.

Generalizing Knowledge about Robust and Objective Knowledge

A first camp of IPE scholarship following positivist epistemologies⁴ seeks to identify the general origins, implications of, and solutions to (unequal) distributions of knowledge across the international political economy. How actors that are more knowledgeable than others are able to profit from knowledge asymmetries or to exploit their less knowledgeable counterparts, and through what processes, is chronicled, for example, in studies of financial markets in the run-up to the global financial crisis of 2008 (Bell and Hindmoor 2015; Nesvetailova and Palan 2020). Similarly, IPE work on “cognitive” forms of “regulatory capture” in which “public institutions lack sufficient resources to generate knowledge and defer to industry-generated expertise” (Campbell-Verduyn 2017) details how knowledge asymmetries lead to the “intellectual shackling of the regulators” reliant on “firms’ models and private knowledge” (Dorn 2012; e.g., Baker 2010; Young 2012). IPE research in this camp further illustrates how the international communication and sharing of knowledge can mitigate the asymmetries involved with “insider knowledge” and enhance the prevalence of “common knowledge” in ways that “reduce transaction costs, thereby facilitating cooperation” (Allan 2018; e.g., Culppepper 2008; Pagliari 2012). Learning and the emulation of policy choices perceived to be successful are highlighted in studies of policy diffusion and transfer (Simmons and Elkins 2004), as well as literatures on global experimentalist governance (Brassett et al. 2012; Campbell-Verduyn and Porter 2014; Posner 2014).

The extent to which knowledge asymmetries—as with other inequalities—are less easily overcome is also stressed in this first camp of IPE scholarship. Central features what Susan Strange (1988/2015) called the “knowledge structure,” such as power asymmetries, are regarded as persistent and intrinsically difficult to overcome. Strange famously identified “what knowledge is discovered, how it is stored, and who communicates it by what means to whom and on what terms” as one of the four key structures of the international political economy besides finance, production, and security. What she relatedly referred to as “structural power,” or the “power to decide how things shall be done, the power to shape frameworks within which states relate to each other, relate to people, or relate to corporate enterprises” is illustrated across IPE studies of a range of actors and processes. For instance, the “best practices” and international standards developed for the mobilization of knowledge in and across policy areas ranging from international education and “human capital” development to financial modeling and macroprudential governance have been chronicled (Baker 2013, 2018; Bernstein and van der Ven 2017).

A key contribution of this camp of IPE has been to identify the structural power exercised within relatively small groups of expert “knowledge actors.” These “knowledge intermediaries” (French and Leyshon 2004) have been identified as organizations ranging from multinational firms (MNCs) like the “Big Four” professional services firms, to smaller think tanks (Hernando et al. 2018) and foundations (Parmar 2002; Huo and Parmar 2020; Wong et al. 2017). Transnational professionals and knowledge workers (Hartman 2015) staffing these organizations, as well as operating in and across overlapping “knowledge networks” or “innovation networks” are also brought out. IPE studies reveal how these individuals and organizations coalesce in formal gatherings of the World Economic Forum (Elias 2013), the Bank of International Settlements (Westermeier 2018), and at international organizations including the Organisation for Economic Co-operation and Development (OECD) (Mahon and McBride 2009), the World Intellectual Property Organization (WIPO) (May 2006) and the World Bank, which famously sought to become a global “knowledge bank” in the late 1990s (Broad 2006; Stone 2003, 2013; Kramarz and Momani 2013; Robertson 2009).

The degrees of convergence and consensus, as well as divergence and difference, amongst knowledge actors is debated across literatures on epistemic communities (Haas 2016) and professional networks (Seabrooke and Henriksen 2017). On the one hand, individual and organizational inclusion in knowledge networks is regarded as stemming from shared “commitment to certain journals, conferences or other gatherings and organs that help bestow scholarly, ideological and scientific credibility” (Stone 2010), including those provided by colleges and universities (Mittelman 2016). Technocratic, knowledge-based expert networks have also been identified as “exclusionary male clubs” (Young and Scherrer 2010, 11). On the other hand, asymmetries and degrees of epistemic arbitrage amongst these actors are identified in attempts to gain influence in expert groups, such as those involved in the post-2008 reform of global financial governance (Seabrooke 2014; Seabrooke and Tsingou 2014).

Despite a focus on non-state actors, state power features prominently in IPE investigations of what Mayer (2012) labels “global knowledge power politics.” Countries are revealed to be competing in attempts to “outsmart” rival states in the “global political knowledge economy” by, for example, promoting the “employability”⁵ of their domestic labor forces (Moore 2012). States are also shown to be key enablers of “Intellectual Monopoly Capitalism” (Durand and

Millberg 2020) in which public, or at least more commonly held forms of knowledge, become commodified and privately held through intellectual property rights (IPRs), patents, and trademarks (May 2010; May and Sell 2005; Muzaka 2016; Rikap 2019; Zeller 2008). These legal objects are shown to have not “naturally” arisen as key components of the “global knowledge economy,” but to have gained prominence due to interstate arrangements, like “free” trade agreements negotiated since the US demanded Trade-Related Aspects of Intellectual Property Rights (TRIPS) as part of the creation of the World Trade Organization (WTO) (Çoban 2004). Here again asymmetries are identified in the relative power to generate, control, communicate, and act on knowledge between hegemonic states (Schwartz 2019), leading MNCs, and the many less dominant creators and producers who must acquire at times expensive permissions to overcome such “monopolization of knowledge” (Haggart 2017b, 173).

How “knowledge hegemony” can be contested and challenged is further investigated in this initial “camp” of IPE scholarship. Brazil, Russia, India, and China (the so-called “BRIC” countries) are characterized as “emerging knowledge powers” (Muzaka 2018; Mayer 2012) who seek to fill a void left as “infrastructural power” of the US over IPRs is regarded as eroding (Weiss and Thurbon 2018). These state-led contestations are linked to the activities of international governmental organizations in channeling knowledge to developing states (Hannah et al. 2017), as well as the activities of academics in facilitating knowledge transmission and contributing to growing “scepticism concerning the social and economic impacts of intellectual property” (Morin 2014). The generation of international solidarity, as well as forms of “shared knowledge” is emphasized across emancipatory studies stressing how, paraphrasing Robert Cox, “all knowledge is for someone and for something” (Farrands and Worth 2005). For instance, critical, particularly Gramscian, studies identify and promote a number of alternatives to “neo-liberal” knowledge structures (Gill 1994). Despite their varying theoretical approaches, however, these studies tend to share positivist epistemological foundations that stand in stark contrast to studies of knowledge in a second camp of IPE scholarship.

Situating and Destabilizing Knowledge about Subjective and Contingent Knowledge

A long-recognized limit to positivist IPE is its understanding of knowledge and information interchangeability (May 1996), or at least regarding knowledge as simply a “more-complex forms of information” (Haggart 2019, 12). This conceptualization is typically linked to Susan Strange’s attempt to develop objective analyses of identifiable knowledge/information against the more Foucaultian understandings of diffused knowledge/power (de Goede 2003, 87–9; e.g., Strange 1990). Power is of course not absent from the Strangean knowledge structure. Yet, as Langley (2009) explains, “for Strange, knowledge (largely as informational resource) is power that is wielded in a constraining manner by rational agents.” The stance taken by Strange in the wider “unresolved debates over the nature of knowledge itself” (May 1996, 182–4) advanced important implicit assumptions still held across much of positivist IPE, including that the generation of *more* knowledge/information is likely to spread reason and possibilities for achieving enlightenment.

Yet, post-positivist IPE emphasizes that the (post-)modern “information age” has been marked less by *deficits* than *gluts* of knowledge/information. Contemporary challenges including making sense of *over-abundant* information and deciphering *which* information to draw knowledge from. Individual and organizational information management is typically guided by combinations of human emotions and non-human information communication technologies (ICTs) (Andrejevic 2013; Winter 2019). Investigating the complexities of the human mind, as well as that of proprietary ICTs, presents key methodological challenges for positivist research seeking to explain increasingly complex information processing processes. To navigate such challenges, some positivist IPE studies have built on economist Herbert Simon’s (1997) notion of “bounded rationality” to take “into account the cognitive limitations of the decision maker, limitations of both knowledge and computational capacity” (e.g., Odell 2002; Poulsen 2015). However, what has been a largely separate camp of IPE research investigates affect, anxiety, resilience, trauma, and other human emotions (Brassett 2018; Brassett and Vaughan-Williams 2012; Da Costa 2016; Lerner 2019; Widmaier 2010). It is these latter studies that shift from knowledge/information toward power/knowledge.

In contrast to the attempt to generate generalizable, robust, and objective knowledge about knowledge, post-positivist IPE research stresses the situatedness and contingency of knowledge production, control, and dissemination. Jettisoned here are positivist assumptions that “a deeper truth is to be discovered, or that underneath ideology the real motivating forces of actors can be detected” (de Goede 2006). Instead, researching knowledge/power is itself regarded as an “inevitably political” (Peterson 2004) activity, one that is fundamentally dependent on the subjectivities of observers, which include IPE scholars. “The norms and standards in the GPE that many hold to be true, essential and universal” are repositioned here as resulting from “historical, culturally specific and highly regulatory discourses of governance” (Griffin 2010, 86–7). Stress is put on how *forms* of knowledge become accepted, and positioned as acceptable, at specific times and in certain places. In doing so, the goal is to reveal possibilities for *alternative* claims about power/knowledge that are more sensitive to a world that is, as leading technologists have come to recognize, simply “too big to know” (Weinberger 2011).⁶

While attentive to asymmetries in knowledge production, control, and dissemination, research in this second “camp” of IPE tends to stress the *limits* of “knowledge about knowledge.” Emphasis remains on “the exclusionary production of knowledge about economy” (Langley 2003, 2) by many of the same “knowledge actors” examined across positivist IPE research. For instance, Amoore (2013) investigates attempts by a variety of multinational consulting and technology firms, including Accenture, McKinsey & Company, PwC, and IBM “to incorporate the very unknowability and profound uncertainty of the future into imminent decisions” (Amoore 2013). Building on the recognition of German sociologist Niklas Luhmann that a “lack of authoritative knowledge, has become an all-pervasive aspect of our lives,” Best (2012, 88) and others (Amoore and Langley 2004; Holmes 2013) foreground *ignorance* and *ambiguity* in addressing the *cui bono* questions of how what is not known can still benefit some over others.

Research in the post-positivist “camp” of IPE also reveals the particularistic nature and effects of acceptable *forms* of knowledge through a focus on objects and techniques that are typically couched in “scientificity.” For instance, the risk models relied upon throughout global finance are shown to “channel credit access and they create and distribute financial resources” (de Goede 2003, 95). The specific logics and rationalities underpinning

such technical objects and further “devices of classification” (Langley 2009) are linked to the “knowability” of formal and informal activities across the international political economy. These, and forms of indirect and diffused knowledge management, are shown to render life amenable to calculation in manners enabling “control-at-a-distance” through governmentality. Building on, and contributing to, a wider “global governmentalities” literature (Larner and Williams 2004), post-positivist IPE studies identify the practices of auditing, credit rating, insurance, micro-lending, and international efforts to promote financial literacy as all promoting the dissemination of individual “responsibilization” (Deuchars 2017; Paudyn 2014; Lobo-Guerrero 2016; Aitken 2010; Guzzini and Neumann 2012; Santos 2017). Liberal governmentality, regarded as cultivating “in its individual subjects a certain responsibility for prudence and self-government” (Best 2007), is revealed to be promoted in the conduct of actors ranging from banks (Amicelle 2011) and countries (Joseph 2010; Vestergaard 2009; Glenn 2019) to diasporas (Kunz 2011).

Emphasis on the idiosyncracies, provincialness, and governance effects of scientific knowledges all contribute to attempts in post-positivist IPE research at opening spaces for *alternative forms* of knowledge. The centrality of “EuroAmerican techno-scientific knowledge” is contrasted with marginalized “decolonial epistemic perspectives” to foreground “the perspectives, theories and experiences of the colonized as the epistemological viewpoint from where to generate knowledge about the world” (Mantz 2019). Similarly, the biases of “masculine bodies, experience, interests, and knowledge claims” (Peterson 2004, 31) are juxtaposed with gendered “knowledge of social reproduction and informal activities” (Peterson 2004, 37). The “colonial, racist and sexist roots” of “Eurocentric and techno-scientific epistemic monocultures” characterizing academic disciplines, IPE included, are contrasted with ongoing efforts to promote the “pluriversalization of knowledges” by better incorporating “multiple geographical and cultural positions” (Tilley 2016 quoted in Mantz 2019). Throughout these and other studies, cultures are explicitly foregrounded in scrutiny of the expansion of, as well as contingency and alternatives to, “market cultures” (Aitken 2007; Best and Paterson 2010; Jessop et al. 2014; *New Political Economy* 22 (4)).

Wisdom Lost in (Siloed) Knowledge

Previous assessments of the generation, control, and dissemination of knowledge in IPE have identified an “insulation or estrangement” between positivist and post-positivist studies (Langley 2009). *Overlaps* between camps of course exist, such as between post-positivist and critical studies promoting alternative knowledges and insisting that “all knowledge claims imply an orientation towards the matter studied” (Farrands and Owen 2005). Despite some commonalities, however, epistemological differences split IPE knowledge about knowledge into one camp examining asymmetries in the production, control, and dissemination of knowledge/information; and a post-positivist “camp” scrutinizing asymmetries in forms of power/knowledge.

Is this gap necessarily bad? Might the benefits arising from such “silo-ization” outweigh the pathologies of the “silo effect” (Tett 2015)? In a very literal and quite specific sense, generating “knowledge about knowledge” along these “parallel tracks” is not necessarily problematic for IPE. The “constructive non-engagement” identified by the editors of this handbook does not appear to undermine the generation of debates and deeper “knowledge about knowledge” *in*

each camp. Yet, in a broader and less specific sense, siloed research undermines the potential for generating “knowledge about knowledge” *across* camps—particularly in ways that enable IPE to more widely impart *wisdom*. In seeking to achieve what Wullweber (2019) has called a “fuller analytical grasp of the pressing politico-economic issues of our time,” IPE scholars can strive to better communication and interactions across “islands of knowledge” (Odell 2010). Interactions between camps should strive less to resolve epistemological divides than to provide avenues for more widely disseminating IPE “knowledge about knowledge” in ways that can equally enhance the broader impact of the field, a continual concern of IPE scholars in the wake of the 2007–2008 global financial crisis (Drezner and McNamara 2013; Mosley and Singer 2009; Green and Hay 2015). Insights generated in, yet communicated across and beyond, siloed IPE research on knowledge can be more productively harnessed in addressing the numerous “real-world” problems and crises facing the international political economy. At the very least, IPE might benefit from the types of debates in International Relations (IR) over whether and how to engage with “plural global audiences” (Michelsen 2018) in making “scholarship a more public enterprise” (Mayer and Smith 2019; see *New Perspectives* forum 2019, 2). The next section identifies three possible paths for improving communication and interactions across these camps in, paraphrasing T. S. Eliot, attempting to locate the wisdom that has become lost in siloed knowledge about knowledge.

AVENUES BETWEEN SILOED KNOWLEDGE ABOUT KNOWLEDGE

Three IPE literatures can be identified as providing productive paths for enhancing communication and interactions between siloed IPE “knowledge about knowledge.” Growing stress on (1) knowledge governance, (2) historicity, and (3) socio-material applications of knowledge each provide useful avenues for not only better circulating knowledge about knowledge amongst IPE scholars, but also more widely to policymakers, students, and the public at large. Before elaborating on each of these paths, a trio of caveats are necessary. First, these paths are neither entirely new, nor unexplored. Rather they are best understood as “greater emerging emphases” in which multiple lanes of research form broader avenues that can enable productive interactions across research silos. Second, these avenues are not manners of resolving gaps between positivist and post-positivist epistemologies. Recognizing the perils of any such “bridge-building” exercise, the multi-laned avenues identified here should be seen as productive paths *for communication rather than for fundamentally overcoming or “resolving”* epistemological divides. Third, more such broad, multi-laned avenues undoubtedly exist. These can and should be productively traveled upon in ways that can communicate knowledge about knowledge not only within, but also beyond, IPE.

Knowledge Governance

Knowledge about knowledge in both IPE camps foregrounds governance. As noted, cognitive or intellectual forms of “regulatory capture” along with interstate agreements

like TRIPS are scrutinized in one camp while governmentality and disciplinary logics of “control-at-a-distance” tend to be stressed in the other camp. Overlaps exist of course, such as with a shared emphasis on “knowledge-actors” and efforts to develop alternatives to existing “common sense” and hegemonic knowledges (e.g., Stephen 2011). Yet research in each camp tends infrequently to interact directly in addressing questions of governance.

One pathway for enhancing interactions between this siloed knowledge about knowledge is found in efforts to revise Strange’s knowledge structure that foreground “knowledge governance.” Blayne Haggart (2017a) and colleagues have explicitly sought to overcome the “siloeing of research and the stifling of dialogue across disciplinary borders” by integrating into Strange’s insights a focus on knowledge/power and post-positivist assumptions that “all knowledge is necessarily partial” and best regarded as “historical constructs” (Haggart et al. 2019, 4). In doing so, Strange’s knowledge structure, Haggart (2017b, 164) argues, can be *re-centered* as the “primary vector for the exercise of power in the global political economy.” Instead of being “just one factor among many” (Haggart 2017b, 164)—one of four structures identified by Strange in her analysis of the international political economy—the knowledge structure becomes regarded as the “*primary* source of power” (Haggart 2017a, 182). By side-stepping Strange’s positivist commitments⁷ and positioning “language and knowledge as necessarily prior to the other structures” (Haggart 2017a, 186) yet linking “regulatory aspects” and the “knowledge-production processes” characterizing the knowledge structure into conversation, this research path offers a useful avenue for communicating between siloed “knowledge about knowledge” in IPE. Stress on largely informal knowledge-construction processes in finance, production, and security can be explicitly connected with the more formal “regulatory aspects” of the knowledge structure, defined by Haggart (2017a, 187) as “rules governing the creation, dissemination, and use of knowledge.” Here, wider conversations across silos can be generated by foregrounding how “knowledge production, possession, control, and legitimization are primarily regulated by Intellectual Property law (*regulation*), which is deeply rooted in Enlightenment notions of the individual creator, private property, and capitalism (*knowledge construction*)” (Haggart 2017a, 191).

The work of Haggart et al. (2019) exemplifies how political scientists, criminologists, communication, and legal as well as science and technology studies scholars all can be brought into productive conversation in foregrounding knowledge governance across economies of privacy, censorship, and the “weaponization” of copyright (e.g., Halbert 2019). In addition to these *interdisciplinary* conversations, *intra-disciplinary* communication across “various knowledge-governance silos” (Haggart 2017a, 182) can productively travel along this path. Various studies at the intersection of knowledge, finance, and security, for example, can interact across epistemological silos by foregrounding knowledge governance (e.g., Farrell and Newman 2019; Heng and McDonagh 2008; Seabrooke and Nilsson 2015; Wesseling et al. 2012). The stress on informal knowledge-construction processes in more post-positivist studies could complement the focus on knowledge-regulation aspects of the knowledge structure. Similarly, knowledge governance could enhance interactions between research on various forms of “regulatory capture” and research exploring the “knowledge practices” and “communities of practice”⁸ that include regulators, professionals, and everyday actors (e.g., Broome et al. 2018; Rethel 2020). Further attempts to revise Strange’s “knowledge structure” can usefully bridge divides in communicating “knowledge about knowledge” within

and beyond IPE, for instance by situating linkages across informal knowledge practices and more formalized knowledge regulation in the theories, concepts, and data through which IPE itself generates knowledge.

Historicity of Knowledges

Positivist IPE studies drawing on historical institutionalism, historical materialism, and other well-established approaches usefully trace the origins of the knowledges underpinning accounting standards, IPRs, and pharmaceutical regulation (*Review of International Political Economy* 17 (4); Blyth et al. 2016; May 2007). Meanwhile, post-positivist IPE studies undertake genealogies of political, economic, financial, and professional knowledges that respond to calls to “look to history as an anchor from which to construct knowledge of the international political economy” (Langley 2003 e.g., Aitken 2006; de Goede 2001; Campbell-Verduyn 2017). Both camps generate deep insights into the historical development and applications of knowledge over time. Yet they have tended only infrequently to interact directly. Communication and engagement across this divide can be enhanced through a second growing research emphasis investigating the histories of the very theories, concepts, and data through which IPE produces knowledge.

Actualized histories, or historicities, of the theories and concepts through which IPE produces knowledge are a first lane in this second broad avenue for fostering interactions between “knowledge about knowledge.” Historical studies of African, Asian, and Latin American thinkers, for example, highlight “how relevant non-Western thought traditions can be reclaimed and cultivated for contemporary IPE” (Mantz 2019; e.g., Helleiner 2015; Helleiner and Rosales 2017a, 2017b; Chey and Helleiner 2018; Helleiner and Wang 2019). These and wider efforts to “globalize IPE” (Phillips 2005) and enhance “global conversations in IPE” provide useful reminders of how the flows of ideas in inter-civilizational debates contributed to a far more “relational, multi-directional and multipolar” development of “classical” political economy theories (Mantz 2019; e.g., *Review of IPE* 20 (6)). Similarly, actualized histories of capitalism (Anievas and Nisancioglu 2015) and the very notion of markets (Watson 2018), concepts like comparative advantage (Watson 2017a), crisis (Samman 2015) and *homo economicus* (Watson 2017b) all provide opportunities for engagements between post-positivist research destabilizing accepted knowledge and positivist studies seeking to re-construct foundations of knowledge in far more nuanced and ultimate more historically “accurate” ways (see Hobson 2013a, 2013b). Investigating the colonial, Eurocentric, and gendered legacies of taken-for-granted theories and concepts offers avenues for debates over whether to jettison or attempt to “correct” the foundations of knowledge that positivist studies seek to build.

A second lane in the broader avenue formed around a growing emphasis on historicity involves studies that trace the actualized histories of the data IPE scholarship typically relies upon in producing knowledge. A growing set of studies of indexes, indicators, metrics, and rankings⁹ respond to a specific post-positivist critique that IPE fails to “enquire how financial knowledge, including statistics and indices, has been historically developed” (de Goede 2003). These studies are sensitive to the post-positivist scrutiny of “scientificness” in numerical knowledge and the governance effects related practices like benchmarking enable (Broome and Quirk 2015). Yet in largely side-stepping epistemological debates for a

stress on the empirical limits and “growing defects” (Linsi and Mugge 2019) of “bad science” (Broome et al. 2018) produced over time, historicities of indicators and indexes offer a path for interactions across largely siloed IPE studies of indicators (e.g., Kelly and Simmons 2015; *International Organization* 73 (3)). Here debates across camps could generate and disseminate insights into how the “lure” (Alenda-Demoutiez and Mügge 2020) of imperfect economic statistics often remain “good enough” for the pragmatic purposes of bureaucratic decision-making (Rocha de Siqueira 2017).

In their book *Savage Economics*, Blaney and Inayatullah (2010, 9) stress how the ability to (re-) produce “history—knowledge of times now past—is a privileged vantage point from which to assess human capabilities for future progress.” IPE scholars can best make use of such privilege in better communicating “knowledge about knowledge” not only across the intra-disciplinary epistemological divide, but also more widely to policymakers, students, and the broader public. Recent historicities of IPE theories, concepts, and data sources offer a second multi-laned avenue for enhancing communication and interactions in and beyond IPE. A third multi-laned avenue for interactions across epistemological divides lies in growing emphases on socio-material applications of knowledges.

Socio-Material Applications of Knowledges

Long a mere “passing interest” (Palan 1997, 18) in IPE, technology has received growing emphasis since the turn of the millennium. Positivist studies seek to develop generalized knowledge on whether technology—applications of expert knowledges—influence cooperation or competition amongst state and non-state actors, market competitiveness, as well as business cycles (e.g., Kim and Urpelainen 2014; Reinsberg 2019; Rosenau and Singh 2002). Post-positivist IPE investigations meanwhile seek to reveal the messiness, complexity, and unintended consequences of technological change, which include increasingly dystopian forms of control-at-a-distance (e.g., Amoore 2013; Gabor and Brooks 2017; Flyverbom et al. 2017; Kremers and Brassett 2017; Youngs 2007). While each broadly foregrounds asymmetries in the production, distribution, and benefits of material applications of forms of knowledge, studies in both “camps” only infrequently interact, as highlighted in emerging studies of labor and consumption across digital platforms (Culpepper and Thelen 2019; Langley and Leyshon 2017; Shibata 2020). The lack of communication and interaction can once again be chalked up to irreconcilable epistemological differences. In developing various cycles, waves, and other typologies, positivist studies seek generalization of knowledge and future predictions about the causal roles of technologies, as well as the trajectories of technological change (Weiss 2005; see, e.g., Akaev and Pantin 2014). Post-positivists eschew any attempt to separate technology as a “variable” amongst many, regarding it as fundamentally intertwined with the international political economy. Such exogenous and endogenous understandings of technology underpin a gap in IPE “knowledge about (socio-material applications of) knowledge.”

This third broad avenue for communicating and interacting across camps is exemplified in IPE studies investigating material bundlings of social relations. What can broadly be described as a “socio-material” stress across IPE scholarship on technology is far from new. The international regimes literature long considered the social responses of states to

technological changes (Ruggie 1975; Cowhey 1990). Critical IPE also long regarded technology “as being shaped by social forces at least as much as it shapes these forces” (Cox 1987, 21), as well as being an “intersubjective social phenomenon” (Talalay et al. 1997). Post-structural studies have further examined how social practices are entangled with technologies of risk calculation, such as financial models (de Goede 2003; Vestergaard 2009). Yet what Mayer et al. (2014) characterize as “international techno-political economy” builds on the more recent “material turn” and “new materialisms” developed across the social sciences to (re)consider the multi-fold implications of non-human objects in enabling and disabling human activities. Often drawing on insights from Science and Technology Studies (STS), where stress is typically on the often unexpected and fragile ways in which “technology is society made durable” (Latour), a range of IPE studies have sought to “open technological black boxes” (MacKenzie 2005; e.g., Campbell-Verduyn and Goguen 2019; Hansen and Porter 2017; Wullweber 2016). Examining socio-material applications of knowledge enables these studies to “zoom in at the technological micro-practices and macro-processes that perform and stabilize ‘the world economy’” (Mayer et al. 2014). In doing so they are able to offer “certain levels of granularity” (Germain 2019) in analysis that usefully connect key microstructures—computer codes and protocols—with macrostructures—finance, production, and security—affected by socio-material applications of knowledge across an increasingly digitized international political economy (Berry 2012; DeNardis 2009; Hester and Williams 2020; Moore and Joyce 2020).

IPE studies tracing the evolution of technology and the international political economy as an intertwined set of phenomena enable interactions with positivist efforts to identify and advance generalizable claims and make predictions about the past, present, and future of the international political economy. For instance, foregrounding “infrastructures” as the “socio-technical systems enabling basic, often backgrounded functions are assembled out of multiple old and new devices” (Bernards and Campbell-Verduyn 2019), provides paths for discussing the empowering effects of the “turn to technology” (Bernards 2019). Here positivist and post-positivist studies can cut across epistemological divides and situate applications of Big Data, blockchains, psychometrics, and other information technologies within an informal “habit of authority” (Singh 2019), as well as local networks of debt, reciprocity, and mediating gatekeepers (Rodima-Taylor and Grimes 2019) that underpin intertwined payment, credit, and remittances infrastructures. Studies stressing the agency of both human *and* non-human agents offer similarly useful paths for engagement across siloed IPE research on knowledge. Kiggins (2018, 229) for instance investigates “state, non-state, and inanimate material objects” as “consequential agents” in seeking to “avoid a crisis of explanatory power brought on by the advent of artificial intelligence-enhanced machines.” Research on applications of blockchain technologies in global finance echoes such arguments, contending that forms of human agency and “techno-agency” need to be considered in parallel in order to understand patterns of authority across emerging areas of transnational activity enabled by rapid technological change (Campbell-Verduyn 2019).

Interactions across epistemological divides in IPE studies of knowledge can be enhanced through recent studies foregrounding socio-material applications of knowledge. Communication amongst IPE scholars through this and the other multi-lane avenues outlined in this section can, in turn, provide the basis for better transmitting “knowledge about knowledge” more widely to policymakers, students, and the general public.

CONCLUSION: KNOWLEDGE, WISDOM, IMAGINATION

This chapter confirmed the existence of a gap in IPE production of “knowledge about knowledge” across what has been regarded as irreconcilable epistemologies of positivist and post-positivist scholarship. Several of the debates and “knowledge about knowledge” generated in each “camp,” as well as avenues for enhancing communication across these “islands of knowledge” were identified. Emphasis on knowledge governance, historicities of knowledge, and socio-material applications of knowledge were characterized as multi-laned avenues for sharing knowledge about knowledge across scholarly debates, as well as for communicating IPE insights more widely in ways that can enhance the practical impact of the field on everyday and regulatory decisions alike. Emerging transnational efforts to regulate applications of machine learning, for example, could benefit from both historicized and present knowledge of socio-material applications of expert knowledge and knowledge governance. IPE insights into the potential and the limits of “knowledge solutions” advanced for overcoming the social and material impacts of the digital economy, such as enhancing disclosure and transparency, could also usefully inform growing public and regulatory debates (e.g., Best 2005; Haufler 2010). Imparting wisdom from “knowledge about knowledge” can be enhanced through the paths described in this chapter, as well as through the *intersections* of the broad multi-laned avenues identified. Historically situated knowledge about “black” and “white” lists in global governance provide one example of intersections between the avenues allowing for communicating and sharing across epistemological divides (e.g., Eggenberger 2018; *Environment and Planning D* 34 (1); Morse 2019; Sharman 2009). Research tracing fragilities developed across information infrastructures in global finance also points to how qualitative early warning signal indicators might be developed as the type of “regulatory aspects” of knowledge-based governance (Campbell-Verduyn, Goguen, and Porter 2019).

The wisdom imparted by IPE “knowledge about knowledge” may also be enhanced in better linking research and pedagogy. Decolonizing IPE curricula can inspire students and scholars alike to investigate novel research questions and provide “fresh analyses of ‘old’ IPE problems” (Mantz 2019). Indigenous knowledges that provide practical ways of attending to the various crises that IPE investigates, from currency instabilities and inequalities, to the ecological limits facing accumulation and growth regimes are being explored in IR (Tickner 2015) and other social sciences (Alcantara and Dick 2017; Tekobbe and McKnight 2016) and beginning to receive attention in IPE (Katz-Rosene et al. 2021). Other innovative pedagogical initiatives, such as the “International Political Economy of Everyday Life” website, point to how research foregrounding key objects and practices underpinning the international political economy can inform novel self-directed knowledge transmission.¹⁰

Communicating “knowledge about knowledge” across epistemological divides can respond to the call of *Review of International Political Economy* editors for a “pragmatic IPE” focusing on “real-world developments and recognizes the complex examinations of multi-faceted phenomena demand a commitment to methodological pluralism” (Johnson et al. 2013). In actualizing this kind of impact, however, more imaginative ways of knowing and disseminating knowledge must be developed and applied. Albert Einstein (1931) famously maintained that “[i]magination is more important than knowledge. Knowledge is

limited. Imagination encircles the world.” Imagination certainly needs to be mobilized in researching a world of increasingly privatized and secret knowledge generated by self-learning algorithms that are being continually tweaked to optimize the digital platforms at the heart of contemporary “knowledge ecosystems.” Recent efforts to develop modes of “encircling” sites of secret or classified knowledge (Bosma et al. 2019), as well as “iterative reflexive research” strategies (Montgomerie 2017), offer imaginative paths for generating such knowledge. Further mobilization of collective, interdisciplinary imagination is required for generating and disseminating knowledge about knowledge in and beyond an IPE that is both practical and pluralist.

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NOTES

1. The title of this section paraphrases the title of the book *What Engineers Know and How they Know It*, in which Walter Vincenti (1990) argued for aerospace engineering to be understood as a knowledge-generating activity.
2. For such evaluations in IPE see Weber (2015) and in International Relations see Hamati-Ataya (2010).
3. While concurring with the gap identified by Lake (2009), I avoid the reductionism of this divide to the division between “American” and “British” IPE. While this and any label to these “sides” can be problematic, I adopt the categories of positivist and post-positivist IPE only reluctantly and in recognition, as I later elaborate, of scholarship seeking to bridge these “sides.”
4. Despite variants, positivist research generally entails three “-isms”: objectivism and the notion that neutral and impartial knowledge of the world is possible; empiricism and the belief that knowledge is accumulated through recurrent experience; and naturalism, the idea that human societies are part of the natural world and thus can be investigated following the methodologies of the natural sciences.
5. Defined as how “people are required to take a new form of subjectivity and self-awareness as well as responsibility for learning and self-education in the form of lifelong learning” (Moore 2012).
6. Importantly, post-positivist IPE typically maintains that “[p]ost-positivism is not anti-positivistic” (Wullweber 2017). Rather the focus is on how particularistic forms of knowledge become positioned as “more plausible than others. But not because they are closer to a transcendent reality, but due to the fact that these analyses resonate more than others with a certain horizon of truth” (Wullweber 2017). In other words, stress here is on how “[s]ome claims to knowledge become naturalized as ‘truths’ and as authoritative, while other claims to knowledge are not, becoming or remaining, perhaps, folklore, speculation, or superstition” (Cutler 2016).
7. In which knowledge is treated “as something ‘out there’ to be discovered, stored, and communicated or withheld” (Cutler 2016).

8. Whose repeated interactions lead to the translation of abstract knowledge claims into real world outcomes (Rethel 2020).
9. See for instance Petry et al. (2021), the special section of *Global Governance* 23 (1) "Powered and Disempowered by Numbers: Data Issues in Global Governance," *Review of International Studies* as well as the output of the "Fickle Formulas" project at <https://www.fickleformulas.org/output>.
10. <http://i-peel.org/>

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CHAPTER 47

TERRITORY

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INTRODUCTION

TERRITORY is unquestionably an important element in international economic relations, yet it has been treated almost entirely in an implicit fashion in mainstream International Political Economy (IPE) scholarship. It is not that spatial scale and geography have been ignored; instead, territory as a distinct concept has only rarely been closely examined. More than simply space or land, territory refers to a set of ideas and practices concerning spatial political claims and control: borders defining the limits of states or other political entities and control or authority within those lines. In other words, territory is not just a matter of a spatial area or the resources contained within that area; territory is a particular type of spatial politics.

Economic and political interactions are certainly shaped by territory understood in this way, and territory itself is structured by political and economic factors. Yet there is little direct interrogation of territory as such in the IPE literature.¹ Moreover, there are numerous research programs directly examining territory that are not identified as belonging to IPE but that nonetheless examine the political consequences of international economic exchange. Yet these other fields have rarely been in dialogue with IPE.

Instead, when IPE scholarship does address spatial phenomena, it rarely engages with territory directly and instead examines other concepts, such as scale or changes in scale. Much of this literature looks at the drivers or effects of the shift to supranational scales (such as the European Union (EU)) or to subnational scales, for economic and political activities. But territory as a concept and a specific set of political practices is only very rarely examined, a concern famously highlighted by Agnew (1994). Thus, further engagement between IPE scholarship and research programs from other related fields could productively connect conceptual work on territory (as well as de-territorialization and re-territorialization) with empirical studies of scale and re-scaling of IPE.

This chapter therefore explores the existing and potential engagement with territory in IPE, beginning with a brief overview of the concept of territory and how it has rarely been examined in IPE. Next, we review a number of research programs within IPE that do engage with spatial issues or concepts, albeit often in ways that do not speak to each other enough. Then we examine some existing and potential ways to bring studies of territory together with IPE research programs and models.

WHAT IS TERRITORY?

Territory is more than simply a land surface, a spatial area, or the resources contained within an area. Instead, territory refers to a spatial form of political control, which has a set of underlying ideas, operationalizing practices, and important consequences. Territory has rarely been defined explicitly in international relations, even in those studies that do engage with spatial issues directly. Instead, much of the conceptual work on territory has come from political geography. These discussions note that territory as it exists today is a modern and far from universal set of ideas and practices relating to spatial political authority and control. Sack (1986, 5), for example, influentially defined territoriality as “a primary geographical expression of social power” and “a powerful geographic strategy to control people by controlling area.” (See also Gottman 1973.) Furthermore, “territoriality is always socially or humanly constructed in a way that physical distance is not” (Sack 1986, 30). More recent work by geographers such as Elden (2013) has refined this concept and its genealogy, noting the emergence of territory in the seventeenth century: “the term *territory* became the way used to describe a particular and historically limited set of practices and ideas about the relation between place and power” (Elden 2013, 6–7). (See also Painter 2010.) Political geographers, in short, have defined territory as a concept and strategy of political control, the outcome of a particular set of political ideas, practices, and techniques.

In contrast, in international relations—and in IPE in particular—territory itself is rarely examined conceptually or defined explicitly, even though spatial factors are central to a number of research programs. In the research program on territorial disputes, for example, the object of analysis is defined as conflicting claims over delimited pieces of land (or sea, in maritime disputes). This has yielded important findings on the drivers of militarized conflict (see, e.g., Huth 1996), yet it has missed the opportunity to engage with territory as a deeper conceptual foundation that frames the terms of those disputes to begin with.

Furthermore, the conceptual and theoretical work on territory that has taken place within IR scholarship has predominantly fallen outside of IPE. Ruggie’s (1993) examination of the historical emergence and contemporary “unbundling” of territory is a clear example of this. His article argued that the territorial structure of modern statehood emerged out of a set of specific circumstances in early modern Europe, and was potentially being altered in fundamental ways by the 1990s (by European integration, for example). Territory, according to Ruggie, is merely one possible form of political rule, and territorial authority itself can take on a variety of forms, including non-fixed nomadic territoriality, non-exclusive and overlapping claims, or the mutual exclusion and clear demarcation of modern statehood. Yet the more conceptual aspects of this argument were taken up less within IPE than by non-IPE scholars; for example, research programs on state formation and the historical origins of the international system have built extensively on Ruggie’s conceptualization.

One of the most direct critiques of the tendency of IPE scholarship to ignore the concept of territory is Agnew’s argument that international relations and its IPE subfield have fallen into a “territorial trap”: assuming that the state form of territorial control—that is, clearly delineated, mutually exclusive spatial areas—is and always has been the predominant form of political organization (Agnew 1994; see also Agnew and Corbridge 1995). As Agnew notes, “[i]t has been the geographical division of the world into mutually exclusive

territorial states that has served to define the field of study” of IR (53). The assumption of territorially defined units, which is shared by even supposedly competing arguments (such as neorealism and neoliberal institutionalism), “has served to obscure the interaction between processes operating at different scales,” from the global to the local (59).

The reaction and follow-up to Agnew’s argument has been revealing. The “territorial trap” article has been widely cited, but most of the citation and especially the engagement has come from outside IPE, in geography for example, or in self-identified “critical” international relations scholarship (which was where Agnew originally identified examples of IR scholarship that had not fallen into this trap). Many of the discussions of the territorial trap in IPE journals, for example, continue to come in the form of calls to more directly engage with this issue, rather than noting any progress toward doing so (see, e.g., Wissen 2009).

As the rest of this chapter will make clear, there is still much room to engage more closely with territory and territoriality in the IPE literature. There has been some progress along particular lines, and a few recent efforts to more directly do this (highlighted in the Conclusion). The following section, however, outlines the main research programs in IPE dealing with issues related to territory. The work on scale, and on re-scaling the state, forms the central framework with which to think about these issues. The research programs discussed below—the global cities literature, the work on glocalism and regionalism, the work on the size of states, and literature on the growing prevalence of offshore financial centers—all contribute to the debate over what spatial scale is most useful for studying the political consequences of international economic exchange.

IPE AND SPATIAL SCALE

The original call for scholars of IPE to re-scale the state came from Agnew and Corbridge (1995). They argued that the political and economic division of the world into territorial states had privileged the economic interactions between states while simultaneously neglecting the importance of non-state actors, subnational actors, and transnational issues. Globalization, in their view, gave further impetus to begin examining the important roles played by entities other than the territorial state. This new international economy would be shaped as much by “pension funds and transnational organizations, as well as international organizations” as by territorial states, and therefore we were likely to witness “further differentiation and uneven development at all spatial scales” (Agnew and Corbridge 1995, 5).

Unfortunately, few scholars heeded this call to reimagine the world economy at different scales. Yet those that did uncovered fascinating insights concerning the role of subnational units (generally, cities and regions) in the global economy, and new ways to analyze the association between the local and the global; the world was being glocalized, translocalized, and offshored. In adopting the concept of scale, these scholars could better understand the distributional consequences of a rapidly globalizing world.

Much of the extant work on re-scaling the state comes from a Marxist-inspired form of political economy (Cerny 2006). In the broadest sense, this work holds that the territorial state has lost its analytical significance. The Fordist mode of production, common in industrial states throughout the twentieth century, was predicated upon a territorial equalization—a national political and economic “flattening.”² Yet with new technology and

increasingly mobile capital, this politico-economic project is no longer sustainable, and we see instead an increasing differentiation within territorial states. No longer are national strategies entirely feasible for large states; rather, subnational regions and large cities vie for scarce capital by engaging in economic specialization and creating attractive institutional structures for investment and development (Jessop 2002). Taken together, these practices lead to increasingly differentiated spaces within territorial states.

Overall, alongside the often-noted dismantling of the welfare state, the literature on re-scaling the state notes that state structures built throughout the era of modern capitalism are increasingly being abandoned in favor of more specialized, local, and flexible structures. Urban regions appear best-suited for this new mode of accumulation. Large cities are becoming increasingly delinked from their surrounding environment and can better be seen as an “archipelago” unto themselves (Cerny 2006, 689). National territorial equalization has been eschewed in favor of increasing differentiation—exploiting unevenness in development is becoming the dominant strategy.

While Agnew and Corbridge’s call to re-scale the state laid out the logic for such a shift in our analysis, further research has expanded the scale debate. Some work interrogates how this observed differentiation and unevenness comes about, while other work focuses on how the various actors—firms, local governments, states—strategically position themselves to compete globally, and what would be the consequences of these changes for the international economy.

The question of how differentiation within states comes about has largely been tackled by economists. Most notably, Fujita et al. (1999) have forwarded what they term the New Economic Geography (NEG), an attempt to incorporate space into neoclassical economic models. Originally, neoclassical economics had no spatial dimensions. Goods and services were modeled as moving “costlessly and instantaneously between dimensionless points,” and “even if space were incorporated into the standard neoclassical framework, its assumptions of perfectly competitive equilibrium and constant returns would imply a smooth world in which there are no locational advantages, and population, production and wealth are evenly dispersed” (Wilson 2011, 377–8). Through incorporating insights from New Growth Theory, such as increasing returns based on market proximity and imperfect competition, Fujita et al. are able to elaborate a parsimonious theory of why we observe economic specialization within and across cities. They find that firms are drawn to centers of population and demand, as they wish to be near suppliers as well as customers. As a result, similar industries agglomerate in particular areas, and this leads to the development of enclaves of industrial specialization (Power 2001).

Other work has focused on how glocalization can be seen as a development strategy by both state and local governments. This literature complicates the idea that globalization has led to a unilinear decline of the state, and instead conceptualizes globalization as “a highly contradictory reconfiguration of superimposed spatial scales” (Brenner 1998, 1). In turn, “contemporary reconfigurations of both urbanization patterns and forms of state territorial organization are best conceived as contradictory, contested strategies of *reterritorialization* through which place-based and territorial preconditions for accelerated global capital circulation are being constructed on multiple spatial scales” (Brenner 1998, 3).

Glocalism, intended to describe a particular set of local responses to increasingly intrusive international politico-economic forces, is one such strategy of reterritorialization. Broadly, glocal strategies can be seen to be bringing about a city-centric capitalism, in which

global cities form the primary building blocks of the international economy. As Brenner (1998) describes, each historical phase of capitalism has rested on a certain form of territorial organization—"a socially produced 'second nature' composed of elaborate transportation, communications and regulatory-institutional infrastructures . . . through which capital can circulate" (Brenner 1998, 13). For the past few centuries, this has been the state. However, in a world of financial deregulation and increasingly globalized production, states, traditionally focused on the national regulation of wages and the international regulation of currency and trade, have gone glocal. It is in these new glocalized states that we see "this increasingly dense superimposition and interpenetration of global political economic forces and local-regional responses within the parameters of a single, re-scaled framework of state territorial organization" (Brenner 1998). That is, given the nature of transnational capital, states now engage in strategies of promoting the competitiveness of their cities and regions, which then feeds back into the global economy. In turn, major cities become so engaged with the global economy that regulation of these cities is in some ways transferred up a scale to supranational agencies (e.g., the City of London).

While global cities comprise the fundamental building blocks of the globalized economy, the regions surrounding these cities are subject to the territorial and regulatory changes brought about by globalization as well. Brenner (2004, 447) refers to these regions, at least as they apply to Europe, as "Glocalized Competition State Regimes" (GCSRs). GCSRs are conceptualized as subnational regions to which economic regulation has been devolved, which then use their regulatory authority to compete for scarce capital with other GCSRs. GCSRs, then, provide the institutional foundation on which the global city "building blocks" rest.

In all, by highlighting the importance of global cities to the international economy, and outlining the concept of GCSRs as the regulatory basis for this new "glocalized" economy, scholars working on issues of IPE and territory point to the changing nature of territorial authority in the global economy.

Yet the literature on the territorial dimensions of the global economy does not solely focus on the need to disaggregate the state. Perhaps more famously, scholars have drawn our attention to the importance of supranational regions to the international economy. However, the question of "what we study when we study regionalism" is somewhat unclear (Hettne 2005, 443). Regions can be taken to mean at least two different things. There are regions in the geographic sense—"territorial, military, and economic spaces controlled primarily by states" (Väerynen 2003, 27)—as well as regions in the functionalist sense, understood as regions defined by non-territorial factors, such as culture. Furthermore, economic regionalism is often subsumed under the broader conversation on regions in IR, focused on regional political and security arrangements. Therefore, the direct relevance of regions for IPE has been understudied.

What is clear from the literature on regionalization is that, over the last 30 years, we have witnessed a growth of regional economic arrangements in response to economic globalization (Väerynen 2003). Yet though the link between regionalization and globalization is well-established, scholars working in this area are not fully in agreement as to *why* globalization has led to an increase in regionalization. This particular question has been investigated under two broad research agendas—those that ask whether regionalization is an integral

part of globalization, or a reaction to globalization, and those that ask whether regions should be seen as the building blocks of the new globalized economy, or detrimental to the expansion of a liberal world economy.

Pelagidis and Papatotiriou (2002) argue that the growth in regionalization stems from the fact that the major states in Europe and Asia wish to lessen their dependence on the US while limiting their exposure to volatility in the global economy, as most trade is directed toward others in the regional arrangement. However, this purely protectionist story has been questioned in subsequent studies. Hettne (2005) argues that what we are witnessing is instead analogous to a Polanyian “double movement.” As he conceptualizes it, regionalization, through regional trade agreements and other such measures, is a move toward greater integration into the world economy. However, at the same time, this move toward regionalization carries with it an interventionist component. Therefore, further study is needed to determine which of these impulses—free trade or protectionism—wins out.

Other scholars examine the effect of regionalization on the global economy, asking whether or not it furthers or hinders globalization. Bhagwati (1991, 1992) inaugurated this line of questioning (Väyrynen 2003). His argument focused on the idea of trade diversion, holding that the increased emphasis on regional trade hindered efficiency and, therefore, was detrimental to international liberalization overall. However, others find that regionalization strengthens global liberalization (Busch and Milner 1993; Milner 1997). From this perspective, regionalization strengthens the hand of export-favoring constituencies within a given region, which in turn increases tendencies toward liberalization. Overall, it is clear from the literature on regionalization that there are a number of overlapping dynamics occurring simultaneously. Coupled with the increasing number of regional arrangements we see in the international economy, the work on regions makes clear the need to expand our analysis from viewing the state as the sole unit of analysis. As Hettne and Söderbaum (2000, 462) note, regions themselves need not conform to the territorial borders of their constituent states, but can bisect states themselves. For example, the coastal regions of China can be seen to be an integral part of the East Asian economic region, while the inland areas may be excluded.

Another spatially oriented research program that comes out of economics but has been closely linked to IPE is the study of the relative efficiency of different borders, that is, the effects of changing state size and shape. These studies ask how states can best be sized and shaped for economic and political efficiency, and then seek to provide explanations for divergences from those expectations.³ As Spolaore (2012) summarizes this research program’s findings, the principle trade-off in state size is between increasing economies of scale and increasing costs of political heterogeneity. In other words, larger size gives greater economic efficiency and better economies of scale in providing public goods (such as defense), but greater heterogeneity—often a result of larger size—makes many public goods less efficient because different groups have different interests. Since this theory then predicts states of a certain size and heterogeneity, the way in which the world is full of a variety of state sizes is explained by historical conquests and democratic decision-making.

While much of this literature has come out of economics, related studies within IPE have taken up similar questions. For example, Lake and O’Mahony (2004) examine the shifting trends in state size: toward bigger states in the nineteenth century and then toward smaller

states in the twentieth. They test a variety of explanations for this pattern (“economies of scale, international openness, preference heterogeneity, and regime type” (708)), and find that regime type is most closely correlated with state size, particularly once democratic regimes are separated into unitary and federal systems (with the growth of larger federal democracies in the nineteenth century).

Although yielding important findings, these studies have been critiqued, mostly by geographers, for ignoring the conceptual underpinnings of territorial statehood. Fall (2010), for example, notes that this research program generally ignores the possibility that ethnic homogeneity or heterogeneity could be endogenous to territorial statehood and even specific border formations, rather than an independent factor simply shaping the efficiency of particular boundaries. In other words, the findings on the size and shape of states from the economic literature could be usefully integrated with conceptual examinations of territorial statehood, thus stepping out of Agnew’s “territorial trap” and possibly yielding further findings about the costs and benefits of state size.

Finally, there also exists a burgeoning literature on the “offshore economy,” mainly meant to denote the growing relevance of tax havens, but also in references to special economic zones (SEZs). The literature on tax havens, commonly referred to as OFCs, or offshore financial centers, is especially relevant to discussions of the role of territory in thinking about international political economy. While OFCs are commonly associated with both illicit activity and public outcry over the tax benefits conferred upon the world’s most powerful corporations, this only covers part of the story. Palan (1998) demonstrates that tax havens can actually be seen as strategies by which small states can cope with a rapidly changing international economy populated by highly mobile economic actors. By delimiting territorial spaces in which government intervention and regulation is less onerous, these states can both attract international capital and contain its effects, while carrying on their traditional functions elsewhere. In this way, states can in fact strengthen their positions within the globalized economy.

Sharman (2012) also demonstrates the strategic importance of OFCs in the modern world economy. He argues that developing-world businesses, and global investors wishing to gain access into developing industries, find doing business easier in OFCs. Free from stringent or outmoded regulations and corrupt political practices of developing states, these businesses can interact with international investors in OFCs—neutral territories with well-functioning legal systems. He uses this logic to explain the otherwise anomalous facts that the British Virgin Islands is a larger source of FDI into China than the US, and that Mauritius is the largest source of foreign direct investment (FDI) into India (Sharman 2010). In these cases, conducting business outside of the territorial confines of the developing state can in fact be beneficial to international development and liberalization.

Yet these same dynamics cause some problems in the international economy as well. Sharman dubs the problem of extra-territorial OFCs “calculated ambiguity” (Sharman 2010, 1), the ability for transnational corporations to take advantage of the benefits of ownership of offshore assets, while limiting their liability over those assets. This is what is commonly seen as the negative side of OFCs. However, Sharman demonstrates even further the dark side of such extra-territorial spaces in his work on money laundering (Sharman 2011), in which he is able, through an ambitious field experiment, to incorporate shell companies engaged in illicit activity through companies legally based in OFCs. In all, the work on the offshore economy demonstrates not only the challenges and opportunities states face in an

increasingly globalized economy, but illustrates the importance of focusing on the new and evolving territorial dimensions of the international economy.

BRINGING IPE AND TERRITORY TOGETHER

In short, there are a number of research programs and areas of study within IPE that consider territory, both explicitly and (more often) implicitly. The implicit character of much of that engagement is unfortunate, because there are productive ways for IPE scholarship to build on, and converse with, studies of territory itself. Thus far, however, this has rarely happened. Agnew's (1994) call for IR to find a way out of the "territorial trap" has rarely been discussed in IPE. Furthermore, even efforts to draw territorial arguments and debates from political geography into international relations tend to come either from the geographers doing much of the work on territory (e.g., Elden 2013) or from IR scholars much closer to the security studies subfield than to IPE (e.g., Kadercan 2015).

This last actually suggests one direction for connecting IPE to questions of territory more directly: bringing together the extensive body of research on territory as a source of conflict (in security studies within international relations) and traditional IPE issues. Several studies have moved in this direction. For example, Simmons (2005) argues for conceiving of borders not just as physical markers, but as institutions that shape and are shaped by political interests and processes. This allows for us to examine the impact of territorial disputes and militarized conflicts on trade, showing the economic benefits of settled borders. Schultz (2015) likewise points toward the value of examining the effect on trade of border disputes and their resolution.

Studies like these suggest the potential benefits of drawing work on territory into conversation with IPE research questions. Of course, even the territorial disputes research that this draws on has been critiqued for lacking a solid conceptualization of territory, and simply treating territory as synonymous with a spatial expanse defined by area (e.g., Elden 2013, 3). Nonetheless, these types of studies are an essential step toward bringing territorial questions directly into conversation with traditional IPE issues like trade.

Another important intervention scholars can make to bring territory back into IPE is to highlight and further interrogate the territorial assumptions of common IPE models. Both hegemonic stability theory (HST) and, more importantly, open economy politics (OEP) rest on territorial assumptions. HST is predicated on the territorial size of the major states in the system.⁴ The theory holds that the larger a state is geographically, the better it will be able to shift factors around, extract natural resources, or build infrastructure, and the fact that this large size, for the very same reasons, makes it rather self-sufficient. Therefore, the size of states in a given system will, at least partially, determine the structure of international trade.

Perhaps more importantly, OEP, the dominant paradigm in IPE, similarly rests on an assumption of a unified, undifferentiated territorial state. Drawing on models from both neoclassical economics and international trade theory, OEP scholars focus on the role of societal preferences in the formation of a number of economic policies, from tariffs to exchange rate policies to FDI.⁵ The underlying assumption of the main models OEP scholars employ is that the international economy affects domestic politics in a number of ways. At base, it creates winners and losers, which in turn creates both conflict between these winners

and losers—grouped along the lines of factor endowments, class, or sectors—and enables the winners from a given set of policies to seek to continue and expand the benefits they accrue. Most interestingly, within these models, the state is modeled as a unified whole, in which distributional conflicts take place on a national scale.

However, the work on re-scaling the state demonstrates that perhaps the best way to move forward is to disaggregate the state, focusing on the effects of openness to trade on politics and society at the sub-state level. As Paul (2002) demonstrates, subnational units provide the physical infrastructure of globalized production, and use various investments incentives, such low taxes and easy access to labor, to attract capital. Most importantly, “subnational units provide the structural location at which social alliances between capital and local classes are forged” (Paul 2002, 468). However, IPE has largely missed these subnational developments. Remedying this oversight can not only lead to better specification of IPE models, but open up a new and fertile area of research.

These moves toward re-scaling IPE models can be combined with conceptual work on territory as a concept, re-examining the way in which the delineation of spatial entities—subnational units, states, supranational regions, etc.—is both a cause and an effect of the kinds of political and economic processes at the heart of IPE.

Yet there is a potential epistemological barrier to incorporating territorial and spatial concepts into the study of IPE. The studies analyzing territorial concepts largely identify with critical and Marxist theories of international relations, while standard OEP models of IPE begin with a positivist, individualist epistemology drawn from microeconomics. Of course, these different ways of framing the international political economy are not necessarily incompatible. Bridging this divide, however, will likely require not only that scholars of IPE engage with concepts drawn from the literature on territory, but also that those focusing on territoriality and spatial scale present their contributions in a manner amenable to the types of modeling common in mainstream IPE.

NOTES

1. While demonstrating the absence of the concept of territory in IPE scholarship is difficult, we took major IPE journals as a means of gauging the level of overall engagement. The journals examined were *Review of International Political Economy*, *International Organization* (IO), *International Studies Quarterly* (ISQ), and *New Political Economy*. In each, we searched using keywords and by manually browsing abstracts for the last two decades of issues. Once we removed articles from IO and ISQ that concerned territory but that were clearly not speaking to IPE concerns (since those are also generalist international relations (IR) journals), the remaining collection of articles directly addressing territory was surprisingly limited. It is this survey that forms the basis of our overview of the engagement with territory in IPE.
2. Fordism refers to a political and economic system predicated on rationalized and standardized mass production regimes. Such systems, associated with the work of F. W. Taylor and the industrial plants of Henry Ford, implied standardization in (relatively low) skills, equality among factory workers, a tempering of class conflicts, and uniformity of consumption practices. Inaugurated in 1914 with Ford’s five-dollar, eight-hour workday,

- Fordism reached maturity post-Second World War, lasting until about the 1970s. See Harvey 1989.
3. Key works here include Friedman 1977; Alesina and Spolaore 1997; and Alesina and Spolaore 2003.
 4. See, e.g., Kindleberger 1973 and Krasner 1976.
 5. See, e.g., Rogowski 1987; Milner 1988; Frieden 1991; and Hiscox 2001.

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CHAPTER 48

THE MANY MEANINGS OF CAPITAL IN IPE

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SEVERAL contributions in this handbook show (mostly in passing) a key point that this contribution gives focus to. Implicitly or explicitly, IPE research invokes and deploys one or more concepts of “capital” as a key part of its analytical project.

However, the concept of capital *qua* capital is rarely foregrounded: for all the “big” debates in IPE, the meaning of “capital” has rarely featured as one of them (for an exception see Nitzan and Bichler 2009). One reason, perhaps, is that IPE has not (yet) had an equivalent to the capital controversies that occurred 60 years ago in economics (Harcourt 1969; Hicks 1974) a debate that was central to fracturing that discipline. The absence of an equivalent, though undoubtedly more social and political, debate about the nature of “capital” in IPE is somewhat surprising. IPE’s study of capitalism as a global social, economic, and political phenomenon has seen significant and sustained debates about globalization, financialization, power, and inequality, etc. but not so much about the nature of capital itself.

The aim of this chapter is to highlight the significance of that omission and bring “capital” to greater prominence in the conceptual frameworks of IPE.

The proposition here is not that the term “capital” is absent in IPE. Indeed, it could be said that IPE is a lively and interesting field of debate in part because the concept of capital is defined and used in different ways. Without the dominance of an orthodoxy that weighs on debate in economics, IPE scholars can and do adopt conceptions of capital that suit their particular analytical objects. The result is a diversity, though no doubt also tension, in how capital is defined. Here are some prevalent meanings of “capital” which may overlap but are far from identical: a pool of wealth, a flow of wealth, an asset to be applied in production (e.g., a factory), a corporation, a social class, and a competitive, calculative system.

Generally, as we will show, the concepts of capital in IPE, like other disciplines in which it is intertwined and eclectically draws from, are typically instrumentally, or what Palloix (1977) termed functionally derived. The instrumental question here is typically what concept/s of capital is adequate for the purpose of the analysis, rather than the ontological status and theoretical rigor of the concept itself.

Concepts of capital in IPE have not been static in their development and the chapter will also show that the needs of analyzing developments in capitalism over the last two decades have seen considerable change in the sorts of concepts that have been deployed. IPE has become such an important field of research in part because it has been flexible in applying and developing concepts of capital that attempt to respond to developments in capital on different international scales and times. But this is not a linear history of successively more “correct,” appropriate, or sophisticated definitions of capital in IPE. Perhaps the “applied” stance of IPE to the concept of capital has contributed to a more agnostic arc of conceptual development here.

So what have been those conceptual changes and how do we understand their lineages? In order to explore how different ways of defining capital are embedded in different paradigms of IPE research, we will often need to identify implicit and embedded depictions of “capital” rather than find it being explicitly articulated. The contribution provides an interpretative scheme to excavate concepts of capital in IPE.

IPEs CAPITAL’S CONCEPTUAL PARADIGMS

In order to explore the different concepts of capital in IPE, we first need to establish a workable taxonomy of those different concepts. Any taxonomy will be contestable and somewhat arbitrary, and so must necessarily elide some aspects of temporal and spatial scales and forms of competition (Clifton 1977; Palloix 1977; Harvey 1990; Postone 1993; Bryan and Rafferty 2006; Bryan et al. 2017; Levy 2017; Banaji 2020). Our taxonomy as well as our mapping of the field necessarily commits sins of omission. But that may be the (acceptable) price of bringing to the fore a concept that has not yet received the attention it deserves in IPE. In order to move toward the focus on capital, the chapter develops a blended taxonomy.

The first taxonomy comes from an important recent essay by Jonathan Levy (2017) on the concept of capital in the cognate discipline of economic history. Following economist John Hicks, Levy argues that political economy has historically had two main understandings of capital—materialist (or what Hicks also referred to as the *volume* of capital paradigm) and fundist (or the *value* of capital paradigm). For Hicks, materialist concepts of capital focus on capital as produced means of production (factories, machinery, and equipment), while fundist approaches focus on capital as a fund of money for investment in the pursuit of profit. As Hicks argued, capital as materiality consists of the “stock of real goods, with power of producing further goods (or utilities) in the future—the stock of such goods existing in the economy at a moment in time” (1974, 308). With capital in the value sense, by contrast “capital is not physical goods. It is a sum of values which may conveniently be described as a Fund. A Fund that may be embodied in physical goods in different ways . . .” (1974, 310). Levy proposes a third approach to capital which he terms *capital as process*, not as a thing or fund but perhaps in the line of David Harvey, as an economic *process*, governed by a form of pecuniary valuation, namely, “capitalization” (cf. also Nitzan and Bichler 2009).

To get to a more granular meaning of capital, we can usefully break the Levy/Hicks binary into a fourfold taxonomy of capital (see Bryan and Rafferty 2006): capital as a stock or means of production, as in “capital equipment” (hereafter Capital as MoP); capital as individual unit of production, as an “individual capital” in reference to a single process or unit of production

and exchange as in a “mining capital” or in its mid-twentieth-century corporate form, multinational capital (hereafter Capital as UoP); capital as financial wealth, as in “the investment of capital,” or “American capital” (hereafter Capital as FW); and capital as “social relation in movement” and as “value in motion”—in reference to the general capitalist system of property ownership, production and surplus appropriation (hereafter Capital as ViM). In sum:

Capital as:

Materialist	Means of Production (MoP)
Institutional	Unit of Production (UoP)
Fundist	Financial Wealth (FW)
Process	Social Relations of Value in Motion (ViM)

Both taxonomies share similar framing, but because the second allows for IPE’s greater focus on the global scales and times of capital, the social relations of capital and different national systems capitalism, and especially the particular interest of IPE in the different institutional forms of capital, we will use this taxonomy as the base, and incorporate Levy’s important insights.

The two propositions we want to establish are first, that these four definitions of capital can be used to differentiate the approaches of important research in IPE; and second that, combined, they allow us to build toward some general understandings of key features of IPE, even if they are generally not expressed in these categories. We will start by identifying the concepts and their common interpretation and expressions, before adding how they have been challenged and changed by debates and historical developments in the political economy of international capital.

Capital as Means of Production (MoP)

The first concept of capital, means of production (MoP), is defined as physical stock or means of production and is consistent with what Levy, following Hicks, calls the “materialist” concept of capital, as in the materiality of “capital equipment.” Historically, starting here points to the development of capitalism as based in the development and application of a technology with associated evolution of labor processes, often cast more generally as class relations. So while we refer here to capital as MoP in terms of a stock, we also include the important insight of Geoff Kay (1979) that labor and the labor-capital relation are the “starting point” of capital. In IPE, capital as MoP can be seen in the development and global expansion of industrial capital, the industrial revolution, wage labor, and, in the twentieth century, notably Fordism, and post-Fordism (Maier 1977; Aglietta 1979; Van der Pijl 1984; Cox 1987; Lipietz 1987; Gills and Gunder Frank 1996; Boyer and Saillard 2002). This is also evident in research that focused on the labor process and its changing forms (Braverman 1974; Burroway 1985). It also flowed through to the analysis of “deindustrialization,” initially registered as a new international division of labor (NIDL) (Froebel et al. 1980), and giving rise to a literature on the capacities of (developmental) states to marshal combinations of MoP that could launch new processes of industrialization (South Korea, Taiwan, etc.) (Johnson 1982; Wade 1990; Amsden 1992; Weiss 1998; Woo-Cummings 1999). This focus

on state-led paths to industrialization was perhaps the high point of IPE's focus on capital as MoP.

More recently, waves of decomposing and unbundling of production have led to a focus on global outsourcing as a technological innovation driven by cutting costs of production and accessing new pools of cheap industrial labor (Gereffi and Korzeniewicz 1994). Here the reframing of capital as MoP engages the growth in trade in semi-finished rather than finished commodities and even in tasks rather than outputs (Baldwin and Robert-Nicoud 2014). This focus starts to shift away from MoP being site-specific and builds toward global value chains (GVC) and global production networks (GPN). These developments have implications for capital as UoP and ViM, but, in the context of capital as MoP, it is critical that there is an understanding that capital equipment now becomes coupled with clear (although fluid) patterns (chains and networks) of internationalized production and exchange (Gereffi et al. 2005; Coe et al. 2008; Coe and Yeung 2015; Gereffi et al. 2019).

This network framing is aligned with the rise of completely outsourced manufacturing, Original Equipment Manufacturers (OEM) and even factory-less goods producers, such that leading global firms may have severed any associations with capital as MoP, at least in direct ownership terms. For these leading firms, asset values and profits are more tied to the generation of intangible capital, including brand names, licensing and franchising, patents, and so on (May and Sell 2005; Bryan et al. 2017a; Durand and Milberg 2019; Schwartz 2021). Here, data are one of the most important outputs, and MoP takes on a new focus: on the coordination, image, and culture of corporations and industries (Thrift 2005). These developments often leave behind a focus on MoP, or conceive it more and more as pure abstraction, and capital as ViM becomes the focus. In accounting, the recognized impossibility of reliably defining and valuing intangible capital seems to correspond with the shift from a focus on a formal and quantifiable global division of labor bounded by discrete UoPs to the cultural-centered emphases of racial and performative capitalism, where labor does not necessarily produce a physical commodity in a factory, but is becoming more performative and immaterial (Hardt and Negri 2001; Clark et al. 2010; Tyler 2011; Rethel 2019; Danewid 2021; Jenkins and Leroy 2021).

The class relations of capital as MoP have followed the same transformations. When MoP are conceived as machinery in workplaces, workers are identified by their separation from the means of production and subjugation to the disciplines of the factory and factory life. This separation was historically a typically violent process, involving enclosure, dispossession, and subjection, and continued in forms of authority and domination in the workplace. In IPE this is first registered as mercantilism, slavery, and imperial occupation (Williams 1944; Shilliam 2012). In the twentieth century, it became embodied in what became variously known as global Fordism (Lipietz 1982), the growth of new forms of imperial dependency on US hegemony both as a way of focusing on the changing technology embodied in means of production, and the political and social relations that were built around it (mass consumption, US economic hegemony, etc.). More recently, capital as MoP also arises with issues of "southern-ness" and racial capital—discourses of dispossession and enclosures; industrialization and non-industrialization of the Global South (Tilley and Shilliam 2018), although these find their strongest cultural expression in capital as ViM.

With the growing predominance of intangible capital comes the growth of labor's engagement with capital outside of the putative factory; indeed it has seen labor itself taking on intangibility, be it as a precariously attached to MoP, independent contracting, and blurring

of earlier distinctions between production and consumption; work and leisure, etc. In this setting, developments in digital media appear as frontier forms of MoP (Castells 1996; Fuchs 2014; Anwar and Graham 2020; Birch 2020; Atal 2021; Grasten et al. 2021; Raj-Reichert et al. 2021).

Accordingly, IPE has noted the innovations in global economic relations as intangible MoP has expanded. With the global spatial and organizational unbundling of production, capital as MoP has tended to be eclipsed in recent decades by concepts of capital that focus attention on either the new institutional modes of international production and exchange, the neo-Smithian focus on new modes of international exchange (characterized by various forms of unequal exchange), or the financial aspects of capital, and more “cultural” explanations of “exploitation.”

Capital as Individual Institutional Units of Production (UoP)

A second concept of capital depicts discrete units of production, (owned and) controlled by individual capitalists, which exist in relations of competition with each other. In IPE, the standard focus is the multinational corporation (MNC) as a/the standard UoP, and perhaps their relations to smaller UoPs (small outsourced workshops, peasant household producers, etc.).

It is, however, a historically-specific connection, and not a definitional one. In the nineteenth and early part of the twentieth centuries, these UoPs were not necessarily corporations. In nineteenth-century Britain, mercantile families were important organizational units of global capitalism (Jones 1987), while in the American South, slave-owning plantations were important UoPs (Levy 2021).

With the twentieth-century development of “the giant corporation” as depicted by Berle and Means (1932), Chandler (1977), and Drucker (1950), the MNC came into its own as giving institutional form and order to modern industrial societies. As Drucker termed it, the large industrial firm became seen as the “representative institution of an industrial society” (1950, 31). For Berle and Means, at the time, and more directly pertinent to the concerns of IPE, “[t]he future may see the economic organism, now typified by the corporation, not only on an equal plane with the state, but possibly even superseding it as the dominant form of social organization” (1932, 313). For IPE, the large corporation’s move to international prominence via expansion to other countries often imparted an imperialist and dominating order, but order nonetheless, to the global economy (Hymer 1970, 1976). The separation of ownership and control that was considered a driving force of corporate expansion opened the possibility for thinking of corporations as actors in their own right on a global stage (Gilpin 1975, 1976; Stopford et al. 1991; Ylonen and Teivainen 2018).

The sheer size of MNCs meant that they had, and could acquire, certain competitive advantages over nationally-constrained UoPs in terms of, for example, political influence, and the technological and scaling benefits of horizontal and vertical integration. In economics, these UoPs were framed as forms of monopoly, oligopoly, and imperfect competition, with surplus profits and inefficiencies attached. One of the developing features of IPE was the recognition that this economic framing failed to capture the power and political influence of large corporations, and left unexplained the relations of these UoPs with both home and host states.

In IPE, this conception of capital as institutional units of production has been extensively developed, both through the centering of analytical attention on corporate expansion via the growth of overseas subsidiaries of MNCs (Barnet and Müller 1974; Hymer 1976), and in studies of corporate power in product and labor markets (monopoly and monopsony) (Baran and Sweezy 1966; Foster et al. 2011; Selwyn and Leyden 2021), as well as corporate political power by the use of lobbying and so on (Apeldoorn 2003).

But within IPE, MNCs are not the sole expression of capital as UoP. If the focus is on a UoP as a unit of competition, and competition relates to the pursuit of profit, then competition occurs within as well as between MNCs. A seminal essay by James Clifton (1977) made stark the difference between an individual capital and a firm or corporation in relation to competition. Clifton pointed out that capitalist competition is not (ultimately) for market share or sales, it is competition for profit, and profit is earned on investment. Hence a key strategic decision of the capitalist is what to invest in and the defining characteristic of capitalist competition is the mobility of investment—mobility over space and between different commercial/financial/industrial activities. The implications of this insight are taken up more fully under capital as ViM (Bryan 1995).

Hence a UoP approach to capital can engage both the formation of MNCs as monoliths and their breaking down through both returns to “core business” and decomposing their operations, including blurring the boundaries of a UoP, and through subcontracting, franchising, licensing, joint ventures, and strategic alliances (Bryan et al. 2017a).

Where a corporation focuses on the valuation of its individual component activities and decides which to hold, which to sell off, and which to subcontract, etc. this draws us towards a Financial Wealth (FW) notion of capital, where what is “production” and what is “ownership,” and what is just “financial exposure to performance” become gray areas.

With the spatial unbundling and blurring of the boundaries between ownership and control over production we have seen a breaking down in the ontological coherence of the MNC as a core category on which IPE was founded (Walker 1989; Desai 2008). In response, there emerged a re-framing of commodity and production networks, and value chain analysis, with a focus on capital as management (often nominated as governance) of the sourcing of spatially dispersed commodity flows. Analysis of MNCs shifted from the power of juggernauts to a focus on the organization of the spatial location of assets and optimal flows of commodities between them. In this emphasis, a fundist perspective starts to replace (or is blended with) the depiction of capital as fragmented and dispersed means of production and attendant labor processes. Initially, that shift focused on the material form of the commodity (as inputs and outputs) and exchange relations (with capital as UoP being retained via a stress on “lead” firms and their power in circuits of exchange), but soon developed into the issue of where value is being created and appropriated along the chain, and toward a more ViM conception of capital. The fundist (FW) concept of such approaches as Global Wealth Chain analysis has made that shift complete (Seabrooke and Wigan 2017, 2022).

Capital as Financial Wealth (FW)

A third concept of capital focuses on aggregations of money and their role in capitalist circulation. Here, capital is defined in terms of a stock of wealth that is deployed to seek financial gain. This corresponds also to Levy’s fundist concept of capital where capital is defined as a fund of

money for investment in pursuit of profit. In IPE this capital as financial wealth concept has been deployed variously in research focusing on the investment of capital, foreign debt, hot money, capital controls, and, in international terms, “American capital” (Strange 1986; Frieden 1989; Haggard and Maxfield 1996; Pauly 1997; Wade and Veneroso 1998; Grabel 2015).

Capital as financial wealth therefore separates decisively from capital as MoP and partially from capital as UoP. Capital as MoP is fixed; capital as FW is liquid. Capital as UoP may be mobile in location over time, but generally fixed in form: and it must “pass through” FW in order to be reconfigured in another form. Work grounded in concepts of FW tends to focus on financial instruments and institutions and the movements of finance that arise with their transformations. Less attention is afforded to the units of production and competition between them.

FW highlights the flexibility and adaptability of capital. It also locates in the study of “finance capital” often with reference to “casino capitalism” (Hilferding 1985/1910; Strange 1986). Here there is focus on the destructive effects of speculative finance on economic stability (the real economy) and the living standards of citizens, along with the growing structural inequalities of wealth and power in capitalism (between owners and non-owners of financial assets). Another strand of analysis here explores FW in relation to shareholder capitalism, with the power of owners of investment funds to reorganize relations in capital markets to give primacy to the interests of ostensible “owners” over other “stakeholders” such as workers, managers, customers, and even governments (Ireland 1999; Lazonick and O’Sullivan 2000). Financial networks and Global Wealth Chain analysis of the international pattern of financial wealth holdings of corporations and high net worth individuals also sits under this strand (Coates and Rafferty 2007; Dahl 2022; Haslam et al. 2022; Santos 2022; Haberly and Wójcik 2022). Studies of financialization also explore the way that finance has reorganized power and profitability within capitalist economies, including the rise of passive investors; new institutions of finance (private equity, hedge funds); new forms of finance (securitization, derivatives); and finance as new centers of power (the City) (Epstein 2005; Krippner 2005; Froud et al. 2006; Morgan 2008; Wigan 2009; Braun 2016; Christensen et al. 2016; Gabor 2016; Fichtner et al. 2017; Pryke and Allen 2000; Beaverstock et al. 2021).

With capital as a stock of wealth, the class relations of capital are defined in ownership terms, with capitalists as owners of wealth and workers as owning very little financial wealth and holding mainly their capacity to work (human capital) as their principal form of capital. This framing feeds into a literature on global inequality as a global class relation: the rise of rentier forms of capital and associated growth of the working poor, southern theory, racial capitalism, and finance’s liquidity as permitting regulatory arbitrage (Christophers 2013; Bryan et al. 2016; Garcia-Bernardo et al. 2017).

Capital as a Process of Social Relations of Value in Motion (ViM)

A fourth concept of capital defines it not in terms of its physicality (MoP), its institutional forms (UoP), or as a financial stock (FW), but in relational and process terms. Capital as a social relation is both a distinctive concept, but as unifying production and circulation (including finance), it can also potentially integrate the previous three. This concept of

capital is similar to Levy's concept of capital as a societal process, which he describes as an economic mode:

governed by a form of pecuniary valuation, namely, capitalization. At the most abstract level, capital, in this line of thought, is what Thorstein Veblen once called a "pecuniary magnet." Capital is legal property assigned a pecuniary value in expectation of a likely future pecuniary income. Capital valuation is prospective, always occurring under conditions of uncertainty . . . one in which agents and institutions are always doing the ongoing work of capitalizing different legal forms of wealth and property. This definition of capital distinguishes capital from wealth and from other exchangeable commodities (consumer goods, for instance, are not capital). It does not treat capital as a physical factor of production, independent of money, ready to be plugged into economists' production functions. Rather than production or commodification, the primary site of analysis is investment. (2017, 487)

There are several key themes in Levy's depiction, but also some further issues that can be included to account for ViM in IPE.

First, the focus is on valuation and suggests (competing and evolving) modes of quantification beyond market prices. Marxian value theory clearly fits here, with an emphasis on modes of valuation that reveal the deeper meanings of class relations, and not just within capital (relational concepts of value such as the Capital Asset Pricing Model or Value at Risk measures, etc.). Some aspects of this are also engaged with through the concept of "assetization," where the capitalization of rents from intangible assets like patents and brand names, where profits may not exist currently, is now seen as a new frontier of valuation (Birch and Muniesa 2020; Langley 2020).

Behind apparently equal exchange of values are underlying social relations of exploitation, whether they be various forms of asymmetry of powers (gender, race, economic, etc.) or structural relations of class. Competition between capitals (UoP) in pursuit of profit is the quantitative logic by which values are commensurated. Neo-Marxism often seeks to extrapolate this to relations between nations, often based in uneven supplies of capital or sources of economic surplus as MoP.

Valuation can also apply other units of measure, like the valuation of environmental impacts that refer to the quantification of carbon or of the costs of environmental damage (and their transfer). Quantification of value has proved difficult in these applications. They may adopt the principles of Environmental, Social and Governance (ESG—once called triple bottom line) accounting, but they invariably lack quantitative precision. So valuation must remain a conceptual abstraction, marked perhaps more by normative calls for inclusion than actual challenges to capitalist calculation itself.

Second, investment (and movement) embeds the significance of the future. This gives prominence to the motivations for and strategies of actors. As a social relation, a focus on linking the present to the future points both to the momentums of capital as UoP and FW, while managing the separation of direct producers from the future returns of their social productivity (to what Levy calls capitalization). It also indicates the way to give tangibility to intangible capital as MoP: ViM gives the process by which the expected future value of intangibles can be compounded to a present value.

Third, there is an implicit focus on the contradictions and crisis in capitalism itself—the (contingent) future reproduction of the system and process of accumulation. The recognition that processes must secure their ongoing conditions of existence is critical to

explanations of processes as diverse as imperialism, post-Fordism, international diplomacy, and the circular economy. These conditions are revealed as both technical conditions (the reproduction of capital as MoP and UoP), but also broader political economic conditions in the reproduction of the social relations that sustain those technical conditions. Those broader conditions include extractive class relations in a Marxian framing, relations of dependency and unequal exchange in radical traditions, or commitments to environmental policy and practices in the circular economy.

Capital as ViM appears as the most abstract depiction of capital and thereby the “level” at which the other conceptions come to relate to each other *qua* capital. Means of production are merely pieces of machinery; it is only through capital as process, expressed through the activity of organizational units of capital, that these pieces of machinery are realized as “capital.” Similarly, in the absence of a relational and competitive concept of capital, a fund of money exists just as a passive pool of savings. It is the relational concept, expressed through the activity of units of accumulation that realizes this fund of money as “capital” or “money capital,” and establishes the need for money capital to circulate in search of expanded value.

CONCLUSION

The four concepts of capital depicted above—means of production; unit of production; financial wealth; and value in movement—are, no doubt, not the only taxonomy for depicting the application of the concept of “capital” in IPE. The flexibility, and often intuitive and eclectic application of the term makes capital in IPE difficult to codify in a totalizing way.

In the taxonomy we have presented, there is, no doubt, a propensity to historicize the four concepts: that first came the technological developments of capital equipment (MoP)—crudely, what Marx referred to as the forces of production—that created the conditions of the distinctiveness of capitalism over feudalism or other pre-capitalist forms. Over time, the efficiency of capitalism led to the legal form of the limited liability corporation, then the large international corporation (UoP) with the latter in particular opening up both “external” (debt and equity) funding of production—expressed as a pool of investment wealth (FW), and hence the capacity to choose where, when, and in what to invest. This freedom to decide required increasingly sophisticated calculative systems (ViM) by which to give decision-making a formal logic with accountability to owners and other financiers, and encompassed a drive to stay liquid in search of expanded accumulation.

That would be a contrived and indeed trivial history of the development of “capital,” for ViM and FW were, in various historical forms, there at the outset! But the sequence gestures to how the different concepts of capital tie in together. Perhaps it can be said that all analyses of capital, no matter what their emphases, want to take a piece out of each of the four concepts, but often without taking on, or being bound by, the conceptual burden of a fully-spelled-out single category. Indeed, it is perhaps only Marxism that aspires to claim coherence across all four categories, albeit that within Marxism the connections are what Marxists debate. Outside Marxism, those claims to coherence, and a totalizing theory, are often depicted as dogmatic.

The challenge in IPE, and in any system-based analysis, is whether the different concepts of capital tie into each other or sit as discrete, and potentially incoherent, framings. Of

course, authors will continue to slide between meanings of capital and, in the process, slide over sometimes subtle shifts of meaning that, if put under the spotlight, might reveal an analytic incoherence of some kind. This is always likely to happen when the concept of “capital” is not itself a focus of debate within IPE. Perhaps it must be said that the price of using a rich concept like “capital,” whose meanings can range from material to metaphysical, is the importance of vigilance in the consistency of its application.

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